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Karl-Friedrich Israel

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Thèse de Doctorat

Karl-Friedrich ISRAEL

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The Costs and Benefits of Central Banking Modern Monetary Economics along a Methodological Dividing Line

JURY

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The Costs and Benefits of Central Banking

Modern Monetary Economics along a Methodological
Dividing Line

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October 23, 2017

*Für meine Familie und Freunde
und allen voran
für
Laura*

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Chapter 1

Introduction

The topic of this thesis is a broad one indeed. More than a hundred years ago one of the leading monetary theorists of Continental Europe wrote: “The literature on money has grown tremendously. Already some years ago, Menger and Stammhammer have estimated the number of independent monographs and publications in scientific journals on the monetary system - works on numismatics excluded - to be far above 5000 to 6000. Since that time every month brings dozens of publications more [own translation]” (von Mises, 1912, p. iii). As any serious student of monetary economics can attest, the number and frequency of publications on money and finance today has still increased by orders of magnitude.¹ Attempts at systematization and surveys have thus become indispensable.

The present study is above all such an attempt. One of the great sources of inspiration was *The Rationale of Central Banking* by Vera C. Smith (later Lutz) originally published in 1936 (Smith, 1990). Smith’s study covers the histories of banking and economic controversies around the topic of central banking in England, France (and Belgium), Germany, Scotland, and the United States from the early 19th century until the period prior to the year of its publication, which is incidentally the same year that John M. Keynes published his *General Theory*. For many historians of economic thought this year marks the beginning of modern macroeconomics, and for us,² although occasionally recourse is taken to earlier works, it is a suitable starting point

¹Searching for “monetary economics” in the online archive *Jstor* under the discipline of economics between 1912 and 2016 leads to 118,920 search results as of December 18, 2016. 34,235 of these results fall between the years 2000 and 2016 only. On the alternative platform *ScienceDirect*, the keyword “Central Bank” in the category of “Economics, Econometrics and Finance” leads to the following numbers of search results for each year between 2010 and 2016, respectively: 1,850; 1,869; 2,141; 2,871; 2,942; 2,961 and 3,312 - that is, 17,946 titles in the past seven years alone.

²Please note that throughout the thesis the plural form “we” is used although there is only a single author of this text, who would be delighted if the reader felt included in the we. In any other case, please regard the “we” as denoting “myself and the material I have studied and cited.”

for a rejoinder to the *Rationale*, that is, a study of modern monetary theory, or more precisely, the costs and benefits of central banking in the modern monetary system.

1.1 Smith's Account of the Historical Debates

Vera Smith presented a two by two classification scheme. According to her scheme, monetary theorists are on the one hand associated with the teachings of either the *Currency* or the *Banking* school, and are on the other hand advocating either *free* or *central* banking. She identified economists in all four possible cross combinations.³

The distinction between Banking and Currency school was taken from the British tradition. Banking school economists argued that the money supply should accommodate the needs of trade through credit expansion and contraction, while Currency school economists adopted some kind of quantity theory of money and sought to avoid excessive monetary expansion and price inflation. Free banking economists advocated a competitive system for the production of money and money substitutes, whereas advocates of central banking were in favor of the monopolization of money production.

Economists closer to the Banking school position had a tendency to emphasize the need for a flexible money supply. Some of them, such as Thomas Tooke (1774-1858), thought that the right degree of flexibility could best be attained by the institution of a central bank (Tooke, 1838). But most others, such as Henry D. Macleod (1821-1902) thought that a free banking system could best accommodate the needs of trade and would work more efficiently (Macleod, 1855).⁴

The most common position among economists closer to the Currency school, such as John R. McCulloch (1789-1864) in Britain or Carl Knies (1821-1898) in Germany, was to advocate central banking to control and restrict money production so as to stabilize the economy and financial markets (McCulloch, 1858; Knies, 1885). However, there were exceptions like Henri Cernuschi (1821-1896) in France, Otto Hübner (1818-1877) and Otto Michaelis (1826-1890) in Germany, as well as the Austrian Ludwig von Mises (1881-1973), all of which adopted elements of the Currency school and at the same time thought that some system of free banking would be most promising for obtaining monetary and economic stability (Cernuschi, 1865, 1866; Hübner,

³For a summary of this classification scheme and a list of authors discussed, see the table in Smith (1990, pp. 144-145).

⁴Smith also provided a long list of French authors that fall into the category of Banking school free bankers. It includes Jean-Gustave Courcelle-Seneuil (1813-1892), Paul Coq (1806-1880) and Charles Coquelin (1802-1852) to name only a few (see Courcelle-Seneuil, 1867, 1853; Coq, 1865; Coquelin, 1876).

1854; Michaelis, 1873; von Mises, 1928a).

However, advocacy of central banking had become dominant, which was reflected in the establishment and preservation of central banks throughout the developed world. It is important to note here that, as Smith rightly emphasized, a “central bank is not a natural product of banking development. It is imposed from outside or comes into being as the result of Government favours” (Smith, 1990, [1936], p. 169). There is thus naturally a need for justification. Is the political imposition of a central banking system on net beneficial or harmful? What are the costs and benefits of a legal monopoly in money production? Smith outlined and critically analyzed a series of arguments that proponents of central banking had traditionally evoked. She identified five main arguments (Smith, 1990, ch. XII).

The first one concerns the distribution of risk among economic agents. In a free banking system one can expect individual banks to fail from time to time just like businesses in other sectors of the economy. When commercial banks issue notes in excess of their own gold reserves, they run the risk of bankruptcy. At the time a bankruptcy happens to materialize, the respective note holders carry the loss. However, the risk associated to the loss is most likely not evenly distributed among market participants. Especially those, who are for whatever reason least capable of discriminating between bank notes from solvent and insolvent financial institutions will suffer disproportionately. Hence, advocates of central banking have argued that uniformity in the risk distribution should be introduced by monopolizing the note issue. Opponents have argued that this might very well render the risk distribution more uniform among market participants, but it tends to increase the risk overall. This argument is closely linked to the modern debate on *systemic risk*. The crucial question is the following. Is a centralized system of money production conducive to more frequent and more severe economic and financial crises or not?

The second and historically most important argument for central banks has been concerned with an excessive expansion of money and credit that might occur in a free banking system, since any individual bank has a strong incentive to lower its discount rate below the rates of its competitors to gain market share. This argument is closely connected to the cycle theory of the Currency school. At a certain point in the expansionary process commercial banks will be forced to abstain from further expansion when note holders increasingly demand redemption. They will have to increase their discount rates in order to protect their reserves from draining. This, however, can lead to a deflationary depression following the inflationary upswing. A central bank could prevent the excessive expansions as well as the necessary corrective contractions

and thus smooth the business cycle. This argument seems at first glance somewhat paradoxical from a contemporary perspective, given that central banks in the recent past have engaged in comparatively large monetary expansion, at least in a broader historical context.⁵ However, the crucial difference is of course that the historical debates presupposed for the most part an underlying gold standard. Under the modern fiat money standard corrective contractions in the historical sense are not necessary anymore, since reserves can be created and supplied from outside the market, qua *creatio ex nihilo*.

This classical argument assumes furthermore that the mutual checks emerging from interbank clearing in a free banking economy would not be sufficient to prevent excessive expansions. However, as Smith (1990, p. 183) pointed out, there are historical examples of stable systems of competitive note issue from Scotland, Canada, and Suffolk (Massachusetts), demonstrating that it can be a sufficient deterring factor.⁶ Proponents of free banking were thus invoking a type of self-regulating specie-flow argument for the interbank clearing system, the original idea of which goes back at least to the works of Adam Smith (2007, [1776], book II, ch. 2) and David Hume's *Of The Balance of Trade* in the context of international monetary theory (Hume, 1987, [1752]).

The famous lender-of-last-resort argument is the third in Smith's list. It has been argued that central banks are beneficial in that they can prevent liquidity shortages of struggling financial institutions. A central bank would enjoy more confidence by the public due to its legal privileges and its notes would more likely be accepted without serious doubts. In the worst case redemption of notes into specie could be temporarily suspended. This way central banks could prevent severer deflationary downturns and economic depressions. The respective counterargument by free banking advocates was that the very existence of an active lender of last resort fundamentally alters the conditions under which financial institutions operate. They will take on higher risks and decrease reserve ratios even further than would have been the case without a central bank to back them up. Hence, the fragility of the financial system might increase overall. This claim is again tied to the broader modern debate of systemic risk.

At this point it is important to note that all of the three arguments for central banks so far

⁵Take for example the development of the "narrow money" supply (M_1) in the euro area. According to the official OECD data base as of April 5, 2017, M_1 has increased by almost 76% between 2008 and 2016 corresponding to an average annual money growth rate of about 7.3%. The annual growth rates between 2014 and 2016 alone have been 13.1% and 9.1%, respectively. Over the same period, 2008-2016, the US money supply M_1 has increased by 126%, that is, about 10.8% per year on average. The "broader money" supply M_3 has increased at a lower rate in both regions over the same period. In the US it increased at an average annual rate of about 6.5%, and in the euro area at an average annual rate of 2.6%.

⁶For a detailed analysis of the history of free banking in Britain, see White (1984). For a broader overview, see Dowd (2003). For a more concise overview on the theory and history of free banking, see White (2015).

mentioned are relevant only under fractional reserve banking. The implementation of full reserve requirements under a classical gold standard would solve the systematic problems of default risk, excessive credit expansion, as well as liquidity shortages at once.⁷ However, this would also render the money supply rigid and entirely dependent on real gold mining. The money supply could not be expanded at relatively high rates.⁸

It must be understood then that the argument for central banks is ultimately an argument for a flexible money supply as well as monetary planning and management. With an underlying money commodity, such as gold, a flexible money supply can only be obtained by fractional reserve banking. A modern fiat standard, in contrast, allows for a flexible money supply with or without fractional reserves, but it also renders a central bank as coordinating institution indispensable. In that sense, we can also interpret arguments for a flexible money supply, and fiat money in particular, as arguments for central banking. The last two of the historically important arguments come closer to the crux of the case for central banks.

According to the fourth argument, a central monetary authority in every currency area would be needed in order to make international cooperation with respect to monetary policy possible. This aspect of central banking was identified as the most important one in the 1920s at International Financial Conferences in Brussels and Genoa, and was emphasized by leading bankers such as Mantagu Norman (Smith, 1990, p. 191).⁹ In the more recent past, the establishment of the euro area and the European Central Bank is an example that reflects the ongoing importance of this aspect. Attempts at cooperation and harmonization can be found in other regions of the world as well. Numerous national currencies are pegged to the US dollar, the euro, or other leading currencies.¹⁰ These implemented currency pegs are another particularity of modern fiat

⁷Peel's Bank Charter Act of 1844 in Great Britain is an important historic example considered to reflect the triumph of the Currency school. It restricted the note issue of commercial banks and gave exclusive note issuing rights to the Bank of England. All further notes had to be backed by precious metals. Hence, the note issue was effectively limited. However, the same strict reserve requirements were not implemented for bank deposits, which allowed the Banking Department of the Bank of England to expand deposits against bank loans without reserves. The system ultimately did not prevent the problems mentioned above. For a very brief overview, see Rothbard (2008a, pp. 186-189) and Whale (1944).

⁸According to available estimates (<http://www.gold.org/>) about 153,000 tonnes of gold have been mined over history, 63% of which, that is, 96,390 tonnes, since the 1950s. The annual world production has reached about 3,100 tonnes in 2015 and 2016. If we assume the monetary base of the world to correspond to the amount of gold mined since the 1950s, the annual gold production would correspond to a growth rate of the world's gold money stock by roughly 3%. Indeed, parts of the mined gold would be used as jewelry and for industrial purposes.

⁹For an interesting account of the drive towards centralization among leading bankers in the United States, see Rothbard (2002).

¹⁰For example, the national currencies of Bosnia and Herzegovina, Bulgaria and Denmark are pegged to the euro, the latter being a rather flexible peg. Many countries in the Middle East have their currencies pegged to the US dollar, such as Bahrain, Jordan, Lebanon, Oman, Qatar, Saudi Arabia and the United Arab Emirates. Both the West African CFA (*Communauté financière d'Afrique*) franc used in Benin, Burkina Faso, Guinea-Bissau, the Ivory Coast,

money. Under an international commodity standard, national currencies would per definition be on a fixed exchange rate, and differences in monetary policy would be limited by reserve requirements. Fiat standards allow for larger differences in the rates of monetary expansion, but only to a certain extent, due to the depreciation of the relatively more expansionary currency, and the ultimate danger of hyper-inflation. This is another aspect that explains the drive for centralization, as discussed above.

Divergence in monetary policies can under either system benefit certain currency areas at the expense of others. One of the modern occurrences of this idea is the so-called currency war, in which devaluations of national currencies are implemented in order to improve competitiveness in international trade. It is obvious that, within the same rationale, the advantage for one country comes at a disadvantage for competing countries.¹¹ A harmonized monetary policy implemented by cooperating central banks can prevent these harmful side effects on the international level.

Yet, the central question remains: What is the optimal monetary policy to be implemented by cooperating central banks? The fifth and last argument in Smith's list, which is still the most important one for us today, gained attention in the post World War I era. Traditionally, monetary reformers have been occupied with automatic adjustment mechanisms, such as international and interbank clearing systems, that help stabilize financial markets and the monetary system as a whole. After the Great War, however, advocacy of an active and "rational" monetary policy along "scientific" lines of controlling the volume of credit and cash reserves via discount rate setting and open market operations became more and more widespread (Smith, 1990, pp. 189-190). Central banks are the very institutions supposed to implement the policy deemed optimal according to scientific inquiry, and they are indeed indispensable if the optimal policy hinges on a flexible fiat money supply.

The first influential scientific criterion for monetary policy has been the one of price stability advocated most prominently by Irving Fisher (1911, 1923). The money supply according to this

Mali, Niger, Senegal and Togo, as well as the Central African CFA (*Coopération financière en Afrique centrale*) franc used in Cameroon, the Central African Republic, Chad, the Republic of the Congo, Equatorial Guinea, and Gabon, issued by the BCEAO (*Banque Centrale des États de l'Afrique de l'Ouest*) and the BEAC (*Banque des États de l'Afrique Centrale*), respectively, are on a fixed exchange rate of 655.957 units per euro. Lesotho, Namibia, and Swaziland have their currencies on a one-to-one peg to the South African rand. Still other African countries like Djibouti and Eritrea, as well South and Central American countries like Cuba, Panama, and Venezuela, and the Asian metropolis Hong-Kong have their currencies pegged to the US dollar. The Nepalese rupee is on a fixed exchange rate of 1.6 units per Indian rupee.

¹¹A prominent example in recent years has been China. Former US president Obama as well as current president Trump have repeatedly emphasized unfair exchange rate manipulations of the Chinese, which explain their immense trade surplus *vis-à-vis* the US economy. See for example Staiger and Sykes (2010) who quote President-Elect Obama from October 2008 as saying that China's trade surplus is "directly related to its manipulation of its currency's value."

criterion should be expanded at the rate of real economic growth. Smith (1990, [1936], p. 190) pointed out that this criterion “has been suspect in theory and just as unfortunate in practice.” Since then, the successive debonding of money from gold over the turbulence of World War II, the subsequent Bretton Woods system, and the Nixon Shock in August 1971 has given more powers into the hands of central bankers and monetary policy makers. Scientific policy criteria have developed simultaneously. Price stability has not been discarded entirely, at least not the name, but the meaning of the term has been stretched to some degree. Modern central banks, such as the Bank of England and the European Central Bank declare price stability as their goal and define it as a stable rate of price inflation of below but close to 2%.¹² Important lines of thought in modern macroeconomics have thus rationalized even more expansionary monetary policies than traditionally captured under the case for price stability. The criterion of stable price inflation and other rules for monetary policy rely *a fortiori* on the institution of an active central bank that controls a flexible fiat money supply. However, the importance and alleged advantages of this institution have been challenged in alternative approaches to monetary policy analysis, already in the periods studied by Smith, but also in the modern literature.

1.2 The Need for a New *Rationale*

As Leland B. Yeager (1990, p. xviii) pointed out in his preface to the Liberty Fund edition of *The Rationale of Central Banking*, both White (1984, p. 135) and Schwartz (1987) collapsed Smith’s scheme into three separate categories - Currency, Banking, and Free Banking - essentially getting rid of the intersections between the traditional Currency and Banking schools with the free banking position. The reason according to Yeager is that they focused mostly on British and not Continental European controversies.¹³ White and Schwartz thus suggested that both the Banking School position with its emphasis on flexibility and the Currency School position with its emphasis on stability have led to the acceptance of central banks as useful and beneficial institutions. And of course, if the central banking position has not already been the dominant

¹²This is explicitly stated on the ECB’s website: “Price stability is defined as a year-on-year increase in the Harmonised Index of Consumer Prices (HICP) for the euro area of below 2%.” We further read: “The Governing Council clarified in 2003 that in the pursuit of price stability it aims to maintain inflation rates below, but close to, 2% over the medium term.” See <https://www.ecb.europa.eu/mopo/strategy/pricestab/html/index.en.html>.

¹³White did mention von Mises and his student von Hayek (1978a) as intellectual heirs of the free banking tradition in certain respects. Yet, his study was not only geographically restrained to Britain, but also temporally to controversies of the 19th century, which is another reason why these two authors were not discussed in more detail.

view back in the 19th century, it almost certainly has become today. Free banking alternatives have remained exotic thought experiments at best.

Indeed, most of the more modern literature takes the existence of central banks for granted and directly discusses optimal central bank monetary policies with respect to certain aspects of the macroeconomy. These works, implicitly at least, must be seen as arguments in favor of the institution of central banks, although of course not every policy they actually implement. Studies that directly tackle the more fundamental questions of whether central banks in the abstract are beneficial or harmful in the first place, or whether actual central banks have performed successfully or failed, are rare exceptions. In the very recent past, however, probably triggered by the global financial and debt crisis of 2008, studies of this sort have become more numerous and got more attention. They do not exclusively analyze the topic from the point of view of economics but also dive into other areas of sociology, philosophy, history and law.¹⁴

However, a systematic exposition of the prevailing economic arguments for central banks in the modern literature confronting them with the respective counterarguments, that is, a new *Rationale*, is still lacking. The present thesis tries to fill this gap.

It goes without saying that such a study can never be complete or definitive. It is virtually impossible to cover all relevant aspects in due depth. Hence, difficult choices of what to incorporate and what to leave out are made all along the way, some of them resulting from careful consideration and some from personal ignorance. Various readers might evaluate them more or less favorably. In the remainder of this introductory chapter we will explain the choices made and provide a bird's eye overview of the results found.

1.3 Content and Structure of the Thesis

The first particularity of the thesis concerns the classification scheme used. As opposed to Smith's *Rationale*, contributions to modern monetary economics are not classified according to their conclusions of whether to support central banking or not, but rather according to underlying methodological convictions. We will try to look at the methodological basis for any argument in

¹⁴See for example Selgin et al. (2012), who openly raise the question of whether the Federal Reserve System has been a failure and answer in the affirmative. Their analysis is strictly economical. See also Altmiks (2010) and Howden and Salerno (2014) for collections of essays that critically evaluate fundamental aspects of central banking. In these volumes we can find various contributions that go beyond economics proper. More comprehensive discussions along these lines are contained in Hülsmann (2008, 2013), who emphasizes ethical and cultural aspects, and Huerta de Soto (2006) who analyzes the topic, among other things, from a judicial point of view. For a brief overview on the topic of central banks and some criticism, see Israel (2014).

favor or against specific monetary policies or central banks in general.

It may well be the case that we are, on a more general note, overstating the importance of methodology. Indeed, research on methodology has long been hiding in the background of the economics profession, which is a natural phenomenon when a certain approach has become dominant and generally accepted. Yet, the trend has reversed and it seems as if a point in the modern macroeconomic research program has been reached that merits closer attention to methodological foundations. In a sense, the situation is akin to the special case that Carl Menger (1840-1921) identified more than 130 years ago, and that is echoed in modern attempts to support pluralist and alternative approaches to economic science.¹⁵

Nothing illustrates the validity of the claim that we are in such a special case better than the superficial treatment of the famous *Lucas Critique*. Without either empirical support or sufficient theoretical argumentation it is often claimed that the Lucas Critique has been overcome by modern modeling techniques. In this thesis we entertain the view that it has not. Reflections about methodology can help us understand why and they substantiate some of the arguments against conclusions drawn from modern econometric models.

The implications are of course far reaching, but for our topic it means first of all that, within what we refer to as the *instrumentalist-positivist* approach, there are dimensions of the costs and benefits of central bank monetary policy that remain erroneously analyzed or left aside altogether. These include especially elements of the long-term costs and benefits.

Moreover, we regard the emphasis on empirical prediction in econometric modeling precisely

¹⁵Menger (1985, [1883], p. 27) described the special case that demands methodological considerations as follows:

Only in one case, to be sure, do methodological investigations appear to be the most important, the most immediate and the most urgent thing that can be done for the development of a science. It may happen in a field of knowledge, for some reason or other, that accurate feeling for the goals of research coming from the nature of the subject matter has been lost. It may happen that an exaggerated or even decisive significance is attributed to secondary problems of the science. Erroneous methodological principles supported by powerful schools prevail completely and oneness judges all efforts in a field of knowledge. In a word, the progress of a science is blocked because erroneous methodological principles prevail. In this case, to be sure, clarification of methodological problems is the condition of any further progress, and with this the time has come when even those are obligated to enter the quarrel about methods who otherwise would have preferred to apply their powers to the solution of the distinctive problems of their science.

With his *Untersuchungen über die Methode der Socialwissenschaften und der Politischen Oekonomie insbesondere* Carl Menger (1840-1921) started the famous *Methodenstreit* against the German Historical School in the second half of the 19th century. As a matter of course, we do not have the hubris to intend or hope to spark a new *Methodenstreit*, but a focus on methodological differences is thought to be fruitful for reasons outlined below. Moreover, it should be noted that in the modern literature there is already for some time an increasing interest in methodological issues again (Hausman, 2008, p. 2). Some of the very recent publications have been rather critical of modern macroeconomics, and do not lack a good portion of polemic (Romer, 2016).

as such a secondary problem that “an exaggerated or even decisive significance is attributed to” (Menger, 1985, [1883], p. 27) in most of the dominant stream of literature in modern macroeconomics. Empirical prediction is the cornerstone of the instrumentalist-positivist approach covered in the first part of the thesis. Again, the focus on empirical prediction implies that certain aspects of the costs and benefits of central banking are analyzed more thoroughly than others. Concepts that do not readily lend themselves to clear-cut empirical manifestations tend to be ignored or have to be replaced by more or less arbitrary empirical proxies. The problems of moral hazard and systemic risk would be important examples.

There are however other branches of economic thought that can offer an alternative perspective. In the second part of the thesis, we therefore contrast the *instrumentalist-positivist* approach with the *causal-realist* approach, which we find to deal with these crucial aspects of the costs and benefits of central banking in more detail and more satisfactorily. The aim is thus to put together the contributions on both sides to obtain a more comprehensive cost-benefit analysis. We also want to confront some of the opposing claims between and within each side.

1.3.1 Part I: The Instrumentalist-Positivist Approach

Mark Blaug has emphasized the path dependency of the development of economic thought and the interrelatedness of different branches within economics and thereby focused specifically on those branches that we classify under the instrumentalist-positivist approach: “Without Pigou there would be no Keynes; without Keynes no Friedman; without Friedman no Lucas; without Lucas no ...” (Blaug, 1991, pp. x-xi; see also Blaug, 2001, p. 156) The history of modern macroeconomics can and has largely been written as a progression from one branch to the other - from the Keynesian, to the Monetarist, to the New Classical. And sure enough, without Lucas and New Classical economics there would be no New Keynesian DSGE modeling as we know it today.¹⁶

This chronology is quite frequently spiced up by calling the emergence of one branch a

¹⁶The idealized process of development in macroeconomics that we have in mind here is also well described in the following passage:

Macroeconomics is not an exact science but an applied one where ideas, theories, and models are constantly evaluated against the facts, and often modified or rejected [...] Macroeconomics is thus the result of a sustained process of construction, of an interaction between ideas and events. What macroeconomists believe today is the result of an evolutionary process in which they have eliminated those ideas that failed and kept those that appear to explain reality well. (Blanchard (1997) as cited in Snowden and Vane (2005, p. 5))

revolution and the emergence of the next branch the respective counterrevolution (Johnson, 1971; Tobin, 1981; Snowdon and Vane, 1996, 1997; Woodford, 1999), or calling the transition from one branch to the next a fundamental paradigm shift from an old orthodoxy to a new one. Yet, we hold that they are more homogeneous than these spectacular narratives would suggest, and that there has been a rather strong cross-fertilization between them.¹⁷

Take for example the quantity theory of money. Patinkin (1969) argued persuasively that the Monetarist restatement of it (section 3.2.1, p. 100) was an extension of the Keynesian version. Indeed both stand in the Cambridge cash balance tradition going back to Marshall. Another example is the Friedman and Phelps natural-rate hypothesis (section 3.3.1, p. 118) that, although considered to be a central part of Monetarism (Snowdon and Vane, 2005, ch. 4), has also been interpreted as a contribution to Keynesian economics (De Vroey, 2016b, p. 108). A widely recognized Keynesian, on the other hand, considered the hypothesis to be a precursory contribution to New Classical economics (Tobin, 1995). Hoover (1984) makes a particularly strong case for the connectedness of Monetarism and New Classical economics more generally. The kinship of New Classical and New Keynesian economics is quite obvious from their largely identical model framework (Woodford, 2003; Galí, 2008).

The central theme, however, that connects all of these four major branches of modern macroeconomics does not lie in any particular type of model or any commonly accepted set of policy conclusions. The overarching element is one of scientific method. We can identify a common methodological *leitmotif* throughout all of these branches of macroeconomic thought. It is precisely what we call the *instrumentalist-positivist* approach to economics, which we trace back to the birth of modern econometrics in the 1920s. Indeed, one could argue that the combining element between some very influential Keynesians, Monetarists, New Classical and New Keynesian economists, as understood here, is their acceptance of the core postulate of modern econometrics. It is that economics should be transformed into a science “in the strict sense of the word [*dans le sens restreint de ce mot*]” (Frisch, 1957, [1926], p. 79) that follows the same methodological principles as the natural sciences, most notably physics. Hence, they

¹⁷This view is shared by Olivier Blanchard and Stanley Fischer. They start their *Lectures on Macroeconomics* with the following statement:

On the surface, macroeconomics appears to be a field divided among schools, Keynesians, monetarists, new classical, new Keynesian, and no doubt others. Their disagreements, which often appear to be as much about methodology as about results, leave outsiders bewildered and skeptical.

This is not our assessment of the field. Behind the public relations gimmicks and the strong incentives that exist in academia to differentiate products, macroeconomics shares many basic models and views. (Blanchard and Fischer, 1993, p. xi)

share a common way of substantiating their arguments for or against specific monetary policies and central banking more broadly.

We thus devote one chapter to each of these four main branches of modern macroeconomics in part I of the thesis. Chapter 2 starts with a brief historical sketch of the birth of modern econometrics. In particular, we review the writings of Ragnar Frisch who is commonly seen as the originator of econometrics (Bjerkholt, 1995). We then discuss Keynesian economics, whose amalgamation with modern econometrics led to what we call large-scale Keynesian macroeconometrics.

Having outlined these foundations, we go into representative models such as the Klein-Goldberger Model,¹⁸ or the Brookings model, and finally into the conclusions for monetary policy drawn from them. These conclusions essentially developed around the idea of the early Phillips curve that suggested a trade-off between price inflation and unemployment. It suggested a powerful role for monetary policy that could directly influence real economic variables to the benefit of society. By implication, the full potential of monetary policy could only be exploited with a flexible fiat money supply. Some form of monetary authority that controls the production of money would thus be indispensable. However, the naive Keynesian interpretation of the Phillips curve as a stable “menu of choice” for policy makers was subsequently revisited and criticized.

In chapter 3, we go into the contributions of Monetarism. We start again with a discussion of the methodological foundations, more precisely, a review of Milton Friedman’s seminal paper on “The Methodology of Positive Economics”, which we see as congenial to Ragnar Frisch’s conception of modern econometrics. In particular, it reemphasized the central and indeed almost exclusive role of empirical prediction in forming the tentatively accepted body of economic theory. We regard this essay as a prime exposition of the instrumentalist-positivist point of view. A theory in Friedman’s sense is a tool for empirical prediction and does not itself try to describe and explain the world or certain aspects of it realistically or truthfully, according to any other than a purely instrumentalist notion of realism and truth.¹⁹

We explain that the focus on empirical prediction in theorizing leaves the economist essentially with two options. Either theories must be augmented with a range of formal assumptions so as to render possible the generation of precise empirical predictions that can be compared against

¹⁸A mathematical compendium to the Klein-Goldberger Model is provided in appendix A on page 335.

¹⁹In fact, a methodological instrumentalist might argue that there is nothing realistic or true in the world other than correct empirical predictions.

the facts, or one keeps theorizing and formal model building at a minimum and looks directly into the available empirical material in search of regularities that can be used for predictions. We argue that Friedman, especially in his writings on monetary economics in collaboration with Anna J. Schwartz, as discussed in section 3.2, tends to follow the latter path.²⁰

The proper role of central banking in Friedman's view is to diminish volatility in the money supply. The money supply should increase at a constant rate k over time, where k should lie around 3 to 5%. He thus assigned a much more passive role to central banks than did the Keynesians. This is also reflected in the Friedman and Phelps natural-rate hypothesis covered in section 3.3, which postulates long-run neutrality of price inflation and rejects the naive Keynesian interpretation of the Phillips curve. It was argued that monetary policy can only affect real economic variables over the short run. When inflation expectations adapt, the effect vanishes. The expectations-augmented Phillips curve is until today the theoretical benchmark for work on the relationship between price inflation and economic activity. Although it has relativized the power of monetary policy to some extent, it opened the doors for the analysis of optimal monetary policy rules versus political discretion in fooling the public's expectations. After all, if there can be a real economic benefit to central bank monetary policy in the short run, there can be one in the long run as well, if policy continuously reacts optimally.

New Classical economics as discussed in chapter 4 has followed the other path, namely, the one of theorizing under highly formalistic assumptions, to push the analysis further. One can argue that Friedman has paved the way for this line of research by disputing the importance of realistic assumptions in his methodological writings (Mayer, 2009, p. 138; Hands, 2009).²¹

The New Classicals provided a model framework, the so-called dynamic stochastic general equilibrium (DSGE) approach, that postulates representative households and firms that take into consideration each others behavior as well as specified policy rules for their own optimization problems. This framework thus attempts to incorporate all relevant feedback effects. It has developed in response to the Lucas critique, a review of which is given at the beginning of the chapter in section 4.1. We then outline the central building blocks of the DSGE framework and

²⁰It is true, however, that Friedman had various phases. The writings contained in Friedman (2009) are mostly formalistic, that is, of the first type identified above. In the leading essay, he deduced a deflationary money supply rule as theoretically optimal, but completely reversed the finding in "a final schizophrenic note" at the end (Friedman, 2009, pp. 47-48). He argued that for practical, i.e. empirically justified, reasons, it has to be an inflationary rule. One might say that the formal type of reasoning prevalent in the essay was trumped by the empirical type of reasoning we find, for example, in *A Monetary History* (Friedman and Schwartz, 1963a).

²¹Hammond (1990) argued that Friedman rejected the formalism of the New Classicals. This however does not imply that his methodological writings were not instrumental in bringing it about.

finally its implications for monetary policy in sections 4.2 and 4.3, respectively.

The New Classical baseline model²² assigns a rather limited role to monetary policy due to its specific set of auxiliary assumptions, which includes perfect competition as well as fully flexible wages and prices. These assumptions have been relaxed in the New Keynesian adaptation of DSGE modeling, which renders the role of monetary policy more important and allows for Keynesian policy conclusions.²³ The New Keynesian DSGE model “has become in recent years the workhorse for the analysis of monetary policy, fluctuations, and welfare” (Galí, 2008, p. 41).

We cover the New Keynesians in chapter 5. First, we review the so-called New Neoclassical Synthesis to emphasize the close relationship between New Classical and New Keynesian economics in section 5.1, before presenting the characteristic elements of the New Keynesian model in section 5.2. Finally, we present the policy conclusions derived from the model and particularly the New Keynesian version of the Phillips curve in section 5.3.

Many versions of the New Keynesian DSGE model have been developed that provide different ways of analyzing optimal central bank monetary policy rules. These are usually rules for setting central bank interest rates, with respect to some quantitative criterion, typically a maximized social welfare function. The costs of central banking in this framework are mostly understood as *benefits foregone* due to suboptimal policy decisions in the past. Yet, there are typically no autonomous reflections on the actual and potential costs of central banking apart from that.

The claim that the optimal monetary policy relies on a flexible money supply as well as some political authority that actively manages it, implicitly suggests that the benefits of central banking under fiat money exceed the costs. The benefits lie, according to the literature reviewed, most notably in the stimulation of economic activity, i.e. employment and output, as well as the alleged maximization of social welfare. However, it is important to note that the optimum here is always subject to the criteria applied. While there is some controversy over fine details of applying the right criteria, there is widespread agreement about the actual or at least the potential benefits of central banking in the instrumentalist-positivist literature.

We hold that one important reason for avoiding a more comprehensive and direct analysis of the costs of central banking is methodological. Some elements of the costs are simply lacking an unambiguous empirically observable counterpart and thus ultimately elude any strict testing against the facts. Moreover, all developed economies around the world are, as a matter of fact, endowed with central banks that engage in some form of monetary policy, and there is also,

²²A mathematical compendium to the New Classical baseline model can be found in appendix B on page 341.

²³A mathematical compendium to the New Keynesian baseline model can be found in appendix C on page 347.

following the instrumentalist-positivist approach, an understandable bias towards analyzing the *status quo*, or the world as it is, and not a counterfactual scenario of free banking. After all, there cannot possibly be any recent empirical data about such a scenario, and the data from historical episodes of free banking are very old and suffer from vagueness and inaccuracy when compared to the data generated and collected today. It is then an understandable choice to take the default position of central banking.

The comparison between the actual world of central banking and a counterfactual world of free banking is of course riddled with a fair amount of uncertainty and speculation, but it is indispensable for a comprehensive analysis of the topic at hand. Contributions that we classify as *causal-realist* provide many insights that should complement and occasionally challenge the core of modern macroeconomic analysis of central banking.

1.3.2 Part II: The Causal-Realist Approach

While it is indeed impossible to write meaningful histories of Monetarism or New Classical economics without reference to each other or to Keynesianism of either the orthodox or new variety, we can quite confidently regard the *causal-realist* approach within modern economic thought as mostly separated from them, although of course not independent in any strict sense of the word.²⁴ Their weak bond is mostly one of mutual criticism or neglect.

As pointed out above, all instrumentalist-positivist branches of economics accept the core postulates of modern econometrics. The first dividing characteristic is that causal-realist branches do not. However, this provides neither for the former nor the latter group a sufficient demarcation from other approaches not discussed at all in this thesis. In other words, not every school of economic thought that rejects modern econometrics is causal-realist.²⁵ We therefore have to explain in more detail what we mean by the term, which is done at the beginning of part II in chapter 6, section 6.1.

There are, as should be expected, different branches of realist economics. Hands (2001a, p. 53) points out that “many different faces of ‘realism’ are now emerging within the methodological literature.” Mäki (1990a, p. 312), for example, identifies two schools of economic thought that “are obviously amenable to realist interpretation and reconstruction,” namely the Marxian and

²⁴It should also be noted at this point that the classification into the two camps is not absolutely unambiguous. There are as usual overlaps and amalgamations. However, it seems to be the most conducive way of bringing order into our analysis.

²⁵An example would be the Post Keynesian school that has provided very valuable and important criticisms of modern econometrics (Shackle, 1972), but is not considered to stand in the causal-realist tradition.

the Austrian. In this thesis we focus exclusively on the latter, not least for the reason that many of the core contributions of the Austrian School lie in the field of monetary economics. However, it must be noted that the causal-realist approach is also broader than merely Austrian economics, although we consider it to be the best developed and still developing part of it.

As Salerno (2007) points out, the causal-realist approach can be traced back to Carl Menger, who is likewise seen as the founder of Austrian economics, but it includes several other economists from different countries in the times up to World War I, for example, John Bates Clark (1847-1938), Frank A. Fetter (1863-1949) and Herbert J. Davenport (1861-1931) in the United States, or Maurice Block (1816-1901) and Paul Leroi-Beaulieu (1843-1916) in France. The characteristics of their analyses was a focus on “cause-and-effect relationships or *causal* laws [...] that would explain the prices, wages, and interest rates actually observed in *reality*.” (Salerno, 2007) Hence, in contrast to the instrumentalist-positivist approach, theories and models are not merely seen as instruments for *prediction*, but attempt to capture some deeper truth about the real world and serve, first of all, the purpose of *explanation*. It is ultimately not bound to clearly defined and observable empirical magnitudes but can meaningfully deal with abstract and unobservable concepts such as choice, preferences and uncertainty, that are held to be real phenomena. The tradition was in decline after World War I, but is until today held up by the modern Austrian school.

The deep-seated philosophical foundations of Austrian economics have been associated with the work and influence of Franz Brentano (1838-1917) and have thus been traced back to the philosophical realism of Aristotle and the scholastics by, for example, Barry Smith (1994). In his *Austrian Philosophy: The Legacy of Franz Brentano*, he describes the contrast to many other positions, including the instrumentalist-positivist approach in economics, very well:

Many later philosophers of science have embraced a view of science as an essentially predictive enterprise, conceiving the scientific method as bound essentially to what is capable of being expressed numerically and as being concerned primarily with the building up of quantitative ‘models’. For thinkers in the Brentanist tradition, in contrast, empiricism is consistent both with the idea that the scientist may have insight into the structures with which he deals [...], and with the idea that mere description is a scientific enterprise worthy of pursuit, even if such description leads to the conclusion that predictive laws in certain spheres are unobtainable. It is not least for this reason that the Austrian economists will be seen to be allied with

Brentano and his heirs.(Smith, 1994, pp. 300-301)

Theory in the Mengerian sense concerns universal categories - categories that can in principle be exemplified in any economy - and their relationships, which are seen as exact laws, although not empirically or quantitatively exact. In short, theory concerns the *universals* and econometric, empirical, or historical analyses the *particulars*. This position has been outlined in more detail in von Mises (2007, [1957]).

Hence, there is also a very important role to play for empirical analyses and econometrics within the causal-realist approach. However, their contribution is purely descriptive. We defend this position from the point of view of modern Austrian economics in section 6.3. As chance has it, Pawel Ciompa (1867-1913), the Polish economist who actually first used the term econometrics (Ciompa, 1910) some 16 years prior to Frisch, defined it in a way compatible with the causal-realist approach. A brief review of his conception of econometrics is given as it is not discussed in any detail in the existing literature. By extension of Ciompa's conception, econometrics is seen as a tool for accounting and economic history.

In section 6.2, we reformulate the Lucas Critique to provide a justification for our claim that DSGE modeling has not overcome the fundamental problems it identified. As a consequence, many of the conclusions for monetary policy based on DSGE modeling are challenged. A more general critique of positivist economics is presented as well as an explanation of the potential dangers of unjustified reliance on empirical data for inductive purposes. Chapter 6 provides the required groundwork for putting the content of the following chapters 7, 8, and 9 into perspective.

Merely criticizing theories outlined in part I based on methodological considerations would rightfully be seen as destructive. Therefore, chapter 7 provides an outline of the theory of money and monetary policy in the Austrian tradition as an alternative analytical framework. We will discuss questions on which the various branches in part I and the causal-realist approach of modern Austrians agree, as well as others on which they entertain diverging views. For instance, it is not denied by Austrian theory that there is a stimulating effect of monetary expansion in the short run, and that monetary expansion can prevent an economic downturn for some time. However, long-run neutrality is denied.

Austrians insist that there are medium-run and long-run consequences of specific monetary policies as well as the set-up of the monetary system as a whole. Section 7.2 contains an exposition of Austrian Business Cycle Theory that describes an economic adjustment process of some sort, i.e. a downturn, recession or crisis, as the necessary consequence of unsustainable

investment and consumption decisions taken in the course of a monetary expansion. We will also explain the perverse redistributive effects of sustained monetary expansion. Section 7.3 analyzes the impact of the existence of modern central banks on the incentive structure that businesses in financial markets and decision makers in politics face. We explain how modern central banking increases systemic risk and moral hazard.

Chapter 8 can be seen as an addition to the discussion of the Phillips curve scattered through part I of the thesis. We apply insights from chapter 7 to a historical analysis of the empirical relationship between price inflation and unemployment in France, the United Kingdom, Germany, and the United States since the latter half of the 20th century. The descriptive data analysis shows a strong positive link between present price inflation and future unemployment. We argue that the two variables can be put into a causal connection through the political process of interventionism (von Mises, 1977). Price inflation is one immediate consequence of monetary expansion of a sufficient magnitude. Unemployment on the other hand, may drop temporarily due to stimulating effects, but will increase over the medium-run during the adjustment process of the business cycle. However, we argue that there might also be another impact on unemployment that is more long-lived, namely when politics reacts to the unintended consequences of monetary expansion, including economic depressions and redistribution of wealth from bottom to top. Into this historical reconstruction we add elements of Public Choice theory that challenge the view of policy makers as benevolent social planners.

The typical political measures motivated by the consequences of monetary expansion have multiple effects of their own, but they tend to render labor markets less flexible and thus increase unemployment in the long run. This is by no means a theoretical necessity, but rather a historical contingency. Political decisions could have been different. Yet, we argue that they explain part of the observed historical relationship between price inflation and unemployment. Chapter 8 thus contains an example of an econometric analysis in the Ciompanian sense applied to the historical relationship between these two social phenomena.

Chapter 9 contains another example of Ciompanian econometrics applied to cost accounting. Here we take a closer look at an argument for central banking under fiat money that Milton Friedman, among others, has put forward. He argued that fiat money is less costly to produce than a commodity money such as gold. The costs of production could be virtually zero. Hence, one tangible benefit of a central bank controlled fiat money over a commodity standard are lower production costs. Real resources that are saved can be employed elsewhere to the benefit of

society.²⁶

While it is clear that in principle, from a pure cost-of-production standpoint, fiat money can be advantageous, it is questionable whether actual central banks as they exist today are indeed operating at virtually zero costs. We engage in a comprehensive cost accounting exercise for the central bank system of the eurozone as a fair assessment of the question would require.

The accounting scheme incorporates staff costs, administrative costs and various other expenses for housing, energy, maintenance, and various amenities, as well as expenses for the support of research in monetary economics. For the US, White (2005) has shown the intricate link between the Federal Reserve System and research into monetary economics. The situation is very similar in Europe. Various research groups are directly or indirectly financed by the ECB and other central banks. Overall, we can see that the actual costs of money production and closely related services in the eurozone are of course far above zero, but also considerably higher than, for example, the operating costs of leading gold mining companies.

The results of chapter 9 are particularly striking given the fact that cost expenditures for gold production have not actually decreased since the transition to a fiat standard. Not fewer but more real resources have been devoted to gold mining. To be fair, probably less resources are devoted to gold mining today than would have been the case under a gold standard, but still it seems that the total cost-saving effect has been exaggerated.

The final chapter of the thesis contains a summary of the results found in part I and II. It also attempts to provide a synthesis towards a more comprehensive cost-benefit analysis of central banking.

1.4 Contributions and Gaps of the Thesis

In almost every conceivable case it would be pretentious to claim to have delivered a complete treatment of a subject like ours. This thesis is unfortunately no exception. There are numerous gaps to be filled in future research. However, we like to think that it also contains, in between the gaps, some valuable contributions with some innovative character.

The first part of the thesis covers the big players only. We devote one chapter to each of the four main branches of modern macroeconomics, but do not discuss the somewhat more exotic side branches of, for example, disequilibrium theories of Patinkin, Clower, and Leijonhufvud, or

²⁶The argument is indeed much older and goes back at least to David Ricardo's writings on paper money, but probably further (Hollander, 1911).

Non-Walrasian equilibrium models as developed by Barro, Grossman and Malinvaud.²⁷ Chapter 3 on Monetarism focuses almost exclusively on the contributions of Milton Friedman and pushes aside other arguably noteworthy contributors, such as Karl Brunner and Allan Meltzer.

Moreover, it is our impression that the literature on monetary policy and theory has turned away to some extent from the dominant DSGE approach since the economic crisis of 2008. Unconventional monetary policies geared towards financial stability that have actually been implemented by central banks around the world during the past decade have become an important subject of inquiry. We regret not having found the time to go into this most recent stream of literature in depth. This is a gap in the thesis that most urgently demands further consideration in future research.

In the second part, we have a clear focus on Austrian economics and do not engage in any discussion of contributions from modern Marxian economics that could also be classified as causal-realist. This is because the modern Austrian literature frequently studies the more relevant questions for the topic of this thesis, while Marxian economics has a different emphasis, for example, on the process of economic evolution or the origin of economic value.

There is no discussion of other heterodox approaches like, for example, Post Keynesianism. One reason is that our rough classification scheme of instrumentalist-positivist approaches on the one hand and causal-realist approaches on the other hand does not lend itself easily to an unambiguous classification of all modern schools of economic thought. Other reasons are constraints in time and space. While a more comprehensive treatment would have been interesting, our expected marginal benefit of discussing another stream of literature in the present work was lower than the expected costs of such an endeavor.

The first part highlights the overarching and combining element in the four main branches of modern macroeconomics and thus tries to relativize to some extent the often perceived antagonism between them. Our elaborations on methodology help to show that there might be something like a methodological bias towards certain aspects of the costs and benefits of central banking in the dominant literature. Aspects that concern the long-run consequences of monetary policy or that have a more abstract character and lack an unambiguous empirical counterpart are analyzed less satisfactorily, or are entirely ignored. In contrast, the short-run impact on certain empirically measurable quantities are very carefully studied and well understood.

There are four main contributions of the thesis contained in the second part. A discussion of

²⁷For a very informative and recent review of the literature, see De Vroey (2016b, ch. 3, 6 and 7)

Pawel Ciompa's conception of econometrics had been non-existent in the literature before.²⁸ He was the first to use and define the term, incidentally in a way compatible with the causal-realist approach.

The conclusion from our reformulation of the Lucas Critique in section 6.2 of chapter 6, elements of which are published in Israel (2015), is admittedly not new. Cruccolini (2010) has similarly argued that modern DSGE models remain subject to the critique. However, we highlight the connection that can be drawn to Austrian methodology in order to substantiate that claim.

The historical analysis of the price inflation and unemployment relationship in chapter 8, an earlier version of which was published in Israel (2017), elaborates on some of the existing literature in modern Austrian economics that has attempted to rationalize a positively sloped Phillips curve. It adds some important qualifications to existing claims of a theoretically necessary and time and place invariant long-run link.

A cost accounting exercise for a system of central banks, as contained in chapter 9, has to our knowledge not been covered in the literature so far. While it does not add much to our understanding of monetary economics as such, or the economy-wide impact of central bank monetary policy, it covers a rather mundane but all the more important cost element that is often forgotten. In particular, it has found no consideration at all in the dominant literature on central banking, even though historically the cost-saving argument for central bank controlled fiat money has been very important.²⁹

²⁸For a more detailed account, see Israel (2016).

²⁹All the data analyzed in this thesis as well as the source code written to generate the graphical presentations is available upon request. For all the performed data analyses and generated plots, the statistical software *R* has been used: <https://www.r-project.org/>.

Part I

An Analytical Chronology of Modern Instrumentalist-Positivist Approaches to Monetary Policy Analysis

Chapter 2

Large-Scale Keynesian Macroeconometrics

The first important strand of modern macroeconomic policy analysis is referred to as *large-scale Keynesian macroeconometrics*. In section 2.1 the meaning and genesis of the term is explained. A brief overview of the birth of modern econometrics on the methodological side and Keynesian economics on the theoretical side of the broader discipline of economics is presented.

The subsequent section 2.2 describes the set-up of the econometric models in some detail. The Klein-Goldberger and Brookings models as prime examples for this approach are looked at more closely.

In section 2.3, the implications for monetary policy are outlined. The discovery and early development of the Phillips curve as an essential ingredient of the model framework are discussed. The exploitation of the Phillips curve provided a very influential rationale for expansionary monetary policy, and by implication for fiat money and central banks. The early history of the Phillips curve culminates into the important contributions of the Monetarist and New Classical schools. These approaches will be discussed in the subsequent chapters 3 and 4, respectively.

2.1 What is Large-Scale Keynesian Macroeconometrics?

As with every retrospective classification of a particular branch in economic thought it is very difficult to identify and weigh all relevant factors that influenced its formation. Almost by necessity, we must do some injustice to specific individuals, groups of researchers, institutions,

or other lines of economic thought, when we subsume them into that branch for some reason, exclude them for another, or do not even mention them at all for the sake of brevity. However, with some level of confidence it can be argued that what we refer to as large-scale Keynesian macroeconometrics is essentially characterized by the conjuncture of two developments.

On the one hand, the attempt to combine economic theory with mathematical and statistical tools of analysis gained ever more popularity. What we today mean by econometrics, often portrayed as a sub-discipline of economics and statistics, gained acceptance from the 1920s onward. On the other hand, there was the rapid spread of Keynesian economics after the publication of the *General Theory of Employment, Interest, and Money* (Keynes, 1936).

The combination of these two developments, econometrics on the methodological side and Keynesianism on the theoretical side of economics, led to the first influential approach to modern monetary policy analysis that became dominant in the 1950s and 60s.

It goes without saying that not every economist who could be classified as an econometrician or a Keynesian promoted or engaged in large-scale Keynesian macroeconometrics. Instead, it is only a smaller intersection of individuals that can be put into that camp, foremost among which was Lawrence R. Klein (1920-2013), Nobel Laureate in 1980.

Like very few other economists, Klein personifies this approach. He set himself the goal of empirically testing Keynesian economics and considered his large-scale econometric models to be natural extensions of the Keynesian system (De Vroey and Malgrange, 2010, p. 1; Bodkin et al., 1991, pp. 14; Klein, 1966). Hence, this chapter puts special emphasis on his and his collaborators' work. First, however, we will review the two original developments that led to large-scale Keynesian macroeconometrics separately.

2.1.1 The Birth of Modern Econometrics

The term econometrics in the modern sense of the word, or more precisely its French equivalent *économétrie*, has first been used and defined by Norwegian economist Ragnar Frisch (1895-1973) in his seminal paper “Sur un problème d'économie pure”, which was his very first publication in the field of economics (Frisch, 1926).

According to Bjerkholt (1995, p. xiii), who calls Frisch the originator of econometrics,¹ the

¹In fact, it was the Polish economist Pawel Ciompa who first used and defined the German language equivalent *Oekonometrie* sixteen years prior to Frisch (Ciompa, 1910). As we will show in part II (see pp. 229ff.) his conception of econometrics was rather different from the modern conception that originated with Frisch, who admitted in a brief note in his journal *Econometrica* that he was initially not aware of Ciompa's use of the term (Frisch, 1936).

paper “would deserve a place in the history of economics, even for no other reason than the opening sentences” as they coined one of the most prevalent and important terms in the discipline of economics today:

Intermediate between mathematics, statistics, and political economy, we find a new discipline, which, for lack of a better name, may be called *econometrics*.

It is the aim of econometrics to subject abstract laws of theoretical political economy or ‘pure’ economics to experimental and numerical verification, and thus to turn pure economics, as far as possible, into a science in the strict sense of the word.

[own translation, emphasis in the original]²

As this quotation reveals, Frisch did not simply attempt to add a new layer to the existing body of economic theory, but sought much more than that. He called for a genuine transformation of economics into a “science in the strict sense.” The guiding ideal for his scientific vision of econometrics is to be found in the natural sciences, and particularly in physics and astronomy.³ It is therefore, strictly speaking, not correct to think about modern econometrics as a sub-discipline of economics. It must rather be seen as an attempt to fundamentally change its *modus operandi*.

In order to push this transformation further, Frisch has built upon, and was inspired by, the ideas of several renowned economists, among which are Léon Walras (1834-1910), William Stanley Jevons (1835-1882), Alfred Marshall (1842-1924), Vilfredo Pareto (1848-1923), Knut Wicksell (1851-1926), Irving Fisher (1867-1947), and Joseph Schumpeter (1883-1950).⁴ All of

²A complete English translation of the article has been published under the title “On a Problem in Pure Economics” in Chipman (1971, pp. 386-423). The cited passage has been translated from the republication of the French original:

Intermédiaire entre les mathématiques, la statistique et l'économie politique, nous trouvons une discipline nouvelle que l'on peut, faute de mieux, désigner sous le nom de *l'économétrie*.

L'économétrie se pose le but de soumettre les lois abstraites de l'économie politique théorique ou l'économie ‘pure’ à une vérification expérimentale et numériques, et ainsi de constituer, autant que cela est possible, l'économie pure en une science dans le sens restreint de ce mot. (Frisch, 1957, p. 79).

³After Irving Fisher had arranged a visiting professorship from his personal funds, Frisch came to Yale University in the early 1930s. In one of his lectures, he praised astronomy as being one of the most scientific fields, as “astronomical observations are *filled into* the theoretical structure [...] Economic theory has not as yet reached the stage where its fundamental notions are derived from the technique of observations.” (Frisch (1930, ch. 1.1) as cited in Bjerkholt and Dupont (2010, p. 57)) It would be false, however, to reduce Frisch’s ideal conception of econometrics to empiricism and little else, although, this is exactly what this particular statement suggests. In his lectures there are several other references to Newtonian physics and Einstein’s theory of general relativity.

⁴According to Bjerkholt (1995), Frisch’s two great mentors were Marshall and Wicksell (he refers to Frisch (1950, 1952)). Apparently, their books were the only really worn-out ones in his personal library. Frisch credits Marshall for having combined the Walras-Jevons-Menger subjective notion of value with the cost of production viewpoint (Frisch, 1970, p. 16). He specifically refers to Menger as the head of the Austrian economists, but

these economists were, to a greater or lesser extent, and if only occasionally, driven by the attempt at formalizing and quantifying elements of economic theory, such as the concept of utility.⁵ Frisch himself considered the quantification of utility a primary objective of econometrics:

The econometric study that we present is an attempt to realize Jevon's dream [the author refers to the paragraph "Numerical Determination of the Laws of Utility" in the fourth edition of Jevons ([1911] 1965, p. 146)]: measure the variations of the marginal utility of economic goods. We consider in particular the variation of the marginal utility of money. [own translation] (Frisch, 1957, p. 79)

In his Nobel Memorial lecture, delivered after 44 years of an academic career as Norway's leading economist, professor and director of the Institute of Economics at the University of Oslo, he refers again to Jevon's dream of being "able to quantify at least some of the laws and regularities of economics", and claims that, "since the break-through of econometrics – this is not a dream anymore but a reality" (Frisch, 1970, p. 12).

The break-through of modern econometrics and the quantification and mathematization of economics were most strongly linked to the interrelated foundations of two institutions in the 1930s, namely the Econometric Society and the Cowles Commission for Research in Economics, both of which proved to be very successful in the promotion of their common motto "Science is Measurement" among professional economists.

Frisch has effectively done "un peu de propagande" (Frisch, 1970, p. 19) for the establishment of the Econometric Society and its journal *Econometrica* during the 1920s in correspondences with, among others, Joseph Schumpeter, who was visiting and later permanent professor at Harvard University, Irving Fisher of Yale University, François Divisia (1889-1964) of the *École* misspells his first name as Karl instead of Carl. Karl Menger (1902-1985), son of Carl Menger, a mathematician and founding member of the Econometric Society, was probably more after Frisch's fancy. Obviously, Frisch was more drawn towards the formal mathematical presentation in Walras (1874) and Jevons ([1871] 1965), rather than the entirely verbal presentation in Menger ([1871] 2007). For an interesting de-homogenization of these three thinkers, see Jaffé (1976). Frisch studied a French translation of Fisher's dissertation thesis *Mathematical Investigations in the Theory of Value and Prices* during his time in Paris (Bjerkholt and Dupont, 2010, p.28, fn. 10). Fisher himself refers to Jevons' *Theory of Political Economy* as one of two books that had the biggest influence on him (Fisher, 1892, p. 3). Although, Walras and his successor at the University of Lausanne, Pareto, both made important contributions to the mathematical formalization of economic theory in general, and value theory in particular, it is important to note that Pareto rejected a cardinal interpretation of value and utility, but thought of them as being ordinal, and, in fact, based his famous welfare economics on ordinal utility (Aspers, 2001). In this respect, Frisch departed from Pareto. Indeed, listing these economists as intellectual influences is not to suggest that they all would have celebrated Frisch's work in every respect.

⁵Schumpeter may of course be seen as the exception. He was not actively involved in this endeavor in his own writings, but enthusiastically supported the efforts of his mathematically inclined colleagues. Schumpeter, together with Frisch and Fisher, was a founding member of the Econometric Society in 1930 and its president from 1940-1941.

Polytechnique in Paris, Ladislaus von Bortkiewicz (1868-1931) of Berlin University, Arthur L. Bowley (1869-1957) of the London School of Economics, Charles Jordan (1871-1959) of Budapest University, and Eugen Slutsky (1880-1948) of the Central Statistical Directorate of the Soviet Union.⁶

The Econometric Society was eventually founded on December 29 in Cleveland, Ohio, with active support of Irving Fisher and Charles F. Roos (1901-1958), who served as their first president and secretary, respectively. Fisher and Roos forwarded a proposal by Alfred Cowles (1891-1984) to finance the publication of a journal and to “set up a research organization under their auspices” (Christ, 1953, p. 8) to the other members of the society’s council, which included Bowley, Divisia, Frisch, and Schumpeter (Roos, 1933). The European members of the council

⁶Excerpts from his correspondences in the original French, German, and English languages can be found in Frisch (1970, pp. 18ff.). From a letter to Divisia written on September 4, 1926, we can infer that it was Frisch, who came up with the name “Econometrica”, in the style of the biology journal “Biometrika”. He wrote:

Je connais déjà plusieurs économistes-mathématiciens dans différents pays, et j’ai pensé écrire un jour ou l’autre une lettre à chacun d’eux pour avoir leur opinion sur la possibilité d’un périodique, (que dites-vous d’une “Econometrica”?, la soeur du “Biometrika”).

Frisch was also in contact with Jacques Rueff (1896-1978), high ranking civil servant and professor at the Statistical Institute of Paris University, who wrote in the preface to the republication of his essay *Des sciences physiques aux sciences morales*:

Ma méthode - qui n’était que l’application des disciplines scientifiques aux faits économiques - se trouva consacrée par la création, en 1931 [1930], de la Société d’Econométrie. L’événement présenta une importance historique, car malgré de très importantes contributions antérieures, indiscutablement inspirées de préoccupations scientifiques, il marqua l’entrée indiscutée des études économiques dans la science.

C’est en 1929 que Ragnar Frisch, l’illustre économiste mathématicien norvégien, me demanda de l’aider à promouvoir la création d’une association qui réunirait les tenants d’une économie dont les principes seraient constamment confrontés avec les faits, quantitativement exprimés et soumis à leur sanction. Le projet fut discuté à un déjeuner où j’avais convié, avec mon interlocuteur norvégien, François Divisia, qui venait de publier une *‘Economie Rationnelle*. François Divisia représentait avec René Roy et moi la descendance intellectuelle de Clément Colson, dont nous avons tous trois été les élèves à l’Ecole Polytechnique.

Nous donnâmes un accueil enthousiaste au projet de Ragnar Frisch. Il fut convenu que celui-ci partirait pour les États-Unis rallier les concours nécessaires à la constitution d’une “Société Internationale d’Econométrie”. (Rueff, 1969, p. 15)

Although Rueff expressed his sympathy and admiration for Frisch’s project, and called the constitution of the Society a “manifeste émanant,” he was never actively involved in its foundation. Rueff was an ardent promoter of the mathematical and natural scientific approach to economics that the Econometric Society embodied. Yet, he made clear, that this approach can only bear fruits within certain limits:

Le libre arbitre, s’il existe - et aucune science ne saurait être fondée sur une hypothèse aussi discutable que celle qui consisterait à en nier l’existence, - doit impliquer, pour l’individu, la faculté de choisir entre plusieurs voies possibles et, par suite, de modifier à son gré le cours des événements. Par là, il serait vain de chercher à prévoir des successions qui, par leur nature même, doivent être imprévisibles. [...]

Et, cependant, l’expérience nous révèle qu’en de certaines limites des événements humains se prêtent immédiatement à l’analyse scientifique. (Rueff, 1925b, pp. 475-476)

decided to send Frisch to meet and talk to Cowles about his proposal in Colorado Springs. He “became perfectly satisfied, as were Fisher and Roos, that Cowles was sincerely interested in econometric research” (Christ, 1953, p. 9), and in January 1932 the council accepted the offer.

The Econometric Society would sponsor and advise the Cowles Commission for Research in Economics, which was incorporated in September, 1932, and Alfred Cowles would finance the publication of the journal *Econometrica*, the first issue of which appeared in January, 1933. Frisch as editor-in-chief explains the significance of econometrics and quantitative analysis in the editorial note:

[The] emphasis on the quantitative aspect of economic problems has a profound significance. Economic life is a complex network of relationships operating in all directions. Therefore, so long as we confine ourselves to statements in general terms about one economic factor having an “effect” on some other factor, almost any sort of relationship may be selected, postulated as a law, and “explained” by a plausible argument.

Thus, there exist a real danger of advancing statements and conclusions which - although true as tendencies in a very restricted sense - are nevertheless thoroughly inadequate, or even misleading if offered as an explanation of the situation. To use an extreme illustration, they may be just as descriptive as to say that when a man tries to row a boat forward, the boat will be driven backward because of the pressure exerted by his feet. The rowboat situation is not, of course, explained by finding out *that* there exists a pressure in one direction or another, but only by comparing the relative *magnitudes* of a number of pressures and counter-pressures. It is this comparison of magnitudes that gives a real significance to the analysis. Many, if not most, of the situations we have to face in economics are of just this sort. The full usefulness of a large and important group of our economic analyses will come, therefore, only as we succeed in formulating the discussion in quantitative terms. (Frisch, 1933a, pp. 1-2)⁷

Undoubtedly, the problem of multiple factors exerting effects in the same or opposite directions is widespread in economics. Yet, there has been disagreement on the extent to and the exact way in which it can be overcome by quantitative analysis, even among the members

⁷The rowboat example is also used in Frisch (1970, p. 18).

of the Econometric Society. As Schumpeter writes in “The Common Sense of Econometrics” published in the same issue:

What I have to add, by way of comment and amplification, will, I hope, confirm the impression that there is nothing startling or paradoxical about our venture, but that it grows naturally out of the present situation of our science. We do not wish to revive controversies about general questions of ‘method,’ but simply to present and discuss the results of our work. We do not impose any credo - scientific or otherwise -, and we *have* no common credo beyond holding: first, that economics is a science, and secondly, that this science has one very important quantitative aspect. We are no sect. Nor are we a ‘school.’ For all possible differences of opinion on individual problems, which can at all exist among economists, do, and I hope always will, exist among us. (Schumpeter, 1933, p. 5)

The early members of the Econometric Society did not form a homogenous “school” of thought and their approaches at times differed substantially. The degree to which economic concepts could be reformulated in quantitative terms, and be tested empirically, was subject to debate. Although it was generally accepted that there is a quantitative element in economics, its importance was assessed differently. “Many economists had joined the Society at its formation who were not sympathetic to the statistical side of the econometric approach” (Morgan, 1990, p. 121, fn. 18), such as Lionel Robbins (1898-1984), or John Maynard Keynes (1883-1946), whose views will be discussed in subsection 2.1.2.

Especially the research program that Frisch initiated is of great importance for what became large-scale Keynesian macroeconometrics in the post war decades. He himself pushed the quantification of economic concepts and theory further than most other econometricians. He understood that it was a necessary condition for the application of natural scientific methods to economics. These methods require observability and measurability, ideally on cardinal scales. Methods of experimental and numerical verification (or falsification) could not simply be applied to classical economic theory, as the underlying core concepts possess no independent existence in the observable external world, and its conclusions only take the form of qualitative or even counterfactual laws.⁸

⁸On a recent and systematic reinterpretation of economic laws as essentially being of counterfactual nature, see Hülsmann (2003). We will come back to this interpretation in the second part of the thesis. See section 7.2 beginning on page 245.

The first task, therefore, was to redefine economic concepts in terms of variables and indicators that are at least in principle observable and measurable. This would allow the derivation of quantitative economic relationships. Frisch, in the spirit of neoclassical economics, based this redefinition on a mathematical axiomatization of human behavior that has become so widespread that every undergraduate student of economics today learns certain forms of it under the headings of *rational behavior*, *utility maximization* and *homo oeconomicus*. This axiomatization comprises, for example, the well known assumptions of *determination*, *additivity* and *transitivity* of preferences.⁹ Utility or value is not described as an abstract psychic phenomenon, which is ultimately taken as a given, not to be explained further, but rather as a well defined mathematical function of a vector of consumer goods.

Hence, utility is just understood as a mapping from multidimensional bundles of consumer goods, which are observable as they have some form of physical existence, to some cardinal scale of measurement. The critical aspect, of course, is the determination of the mapping. In Frisch's view, the fact that the mapping is merely assumed into existence should not cause too much concern. He was convinced that it was determinable, in principle, through choice questions ("expériences par interrogation") posed to the respective individuals (Bjerkholt and Dupont, 2010, pp. 39-45; Frisch, 1957, p. 81; Frisch, 2013, Lect. 1). He undoubtedly recognized that there were conceptual problems with the idea of inferring cardinal utility measurements from (hypothetical) interview data, and that, in fact, only ordinal rankings could, in principle, be determined. However, the auxiliary assumption of cardinality was often justified "with an appeal to 'everyday experience'" (Bjerkholt and Dupont, 2010, p. 39).

These assumptions, which are extended in similar ways to the production sphere of the economic system, allow for a deductive mathematical derivation of a model framework that contains abstract quantitative relationships between economic variables.

The second step, then, consists in the proper application of statistical-empirical methods in order to bring these abstract quantitative relationships into a concrete form, through the estimation of coefficients based on observed data. This is what Frisch refers to as the combination of the *theoretical-quantitative* (mathematical model framework) and the *empirical-quantitative* (statistical estimation methods) in Frisch (1933a, p. 1). In Frisch ([1933] 2013, Lect. 1), he describes the general approach as follows:

⁹For a complete list of the axioms outlined in Frisch's Poincaré lectures (Frisch, 2013, [1933], Lect. 1), see Bjerkholt and Dupont (2010, p. 42, Table 2).

The attempt at quantification in econometrics comprises two aspects of equal importance. First, we have the axiomatic aspect, i.e. an abstract approach which consists in establishing as far as possible logical and quantitative definitions and to construct from the definitions a quantitative theory of economic relations. Then we have the statistical aspect, here we use empirical data. We try to fill the boxes of abstract quantitative relationships with real numerical data. We try hard to show how the theoretical laws manifest themselves at present in this or that industry or for this or that consumption category, etc. The true unification of these quantitative elements is the foundation for econometrics.

Thus, Frisch (1936, p. 95) defines econometrics as the “unification of economic theory, statistics, and mathematics”. At the heart of this unification are the underlying axioms and assumptions that comprise the mathematical model framework. These assumptions can be altered to the discretion of the econometrician, the only ultimate restriction being the laws of formal logic.

It is of course indispensable, to ask on which grounds one set of underlying assumptions can be declared superior to another set of assumptions. At best, they only provide approximations to the real world, but how could their plausibility be judged? According to Frisch ([1933] 2013, Lect. 1) it is “by the subsequent agreement of the consequences of the axioms with reality that we can judge the plausibility of them.” This view anticipates the instrumentalist methodology of Friedman (1953b), which will be discussed in detail in the next chapter of this thesis. It is the primary indicator of the positivistic element in modern econometrics.

Moreover, Frisch formulated and emphasized the important distinction between micro-dynamic modeling, in which only specific actors, firms, or sectors are analyzed, and macro-dynamic modeling, in which the economy as a whole is considered. His celebrated “Rocking Horse Model” was one of the first mathematical macro-dynamic models of the business cycle (Frisch, 1933b).¹⁰ It has laid out the basic theoretical-quantitative framework for much of the

¹⁰ Most interestingly, it has recently been shown that the “Rocking Horse Never Rocked” (Zambelli, 2007). It was considered the great achievement of Frisch to have derived a cyclical or oscillatory return to equilibrium after a shock from a simple linear model. However, the solutions to his model, for all economically relevant parameter configurations, turn out to be monotonic and not oscillatory, which numerous reviewers have overlooked. Among them were Tinbergen (1935), Hicks (1950), Arrow (1960), Samuelson (1974), and Klein (1998) (see Zambelli, 2007, p. 147, fn. 5). That this grave error has not been found earlier is a curious fact. Zambelli (2007, p. 162) concludes his paper with thought-provoking, though ultimately unanswerable, questions:

Whether the development of mathematical business cycle analysis and that of mathematical economics would have been substantially different is impossible to assess. Would Frisch have played the

macroeconomic business cycle research that followed in the 1930s. During and after the Great Depression this area of economic research was of particular interest.

It was Dutchman Jan Tinbergen (1903-1994), a close friend and collaborator of Frisch, who brought this framework to its first major empirical-quantitative application (Morgan, 1990, ch. 4; Gilbert and Qin, 2005, p. 4). Following a survey of business cycle theories conducted by Gottfried von Haberler (1937), the League of Nations commissioned an empirical test of these different theories, which Tinbergen undertook in a two-volume treatise targeted towards the US economy (Tinbergen, 1939a,b).¹¹ Already this early empirical modeling attempt contained important implications for monetary policy. Tinbergen compared the likely results of six policy programs and found that a currency devaluation would be the most conducive to solving the unemployment problem of the Great Depression (Tinbergen, 1959, p. 83).

In the first volume of Tinbergen's work on US economic cycles, we can find among other things some general explanations on the methodological role of econometric analysis that have in fact evoked a rather critical reception among some of his peers.¹² Tinbergen clarified Frisch's concept of verification. He held that economic theories could in principle only be verified empirically in a rather limited sense. Yet, he argued that they could be falsified or at least shown to be incomplete:

The part which the statistician can play in this process of analysis must not be misunderstood. The theories which he submits to examination are handed over to him by the economist, and with the economist the responsibility for them must remain; for no statistical test can prove a theory to be correct. It can, indeed, prove that theory to be incorrect, or at least incomplete, by showing that it does not cover a particular set of facts: but, even if one theory appears to be in accordance with

same role in defining research directions in economic theory? Would he have gotten the same support from the Cowles and Rockefeller foundations? Would he have been able to support Tinbergen's League of Nations' project in the same way as he did? Would he have had the same impact on his contemporaries as he did? Would research on nonlinear business cycle theory have been different? These are all conjectural questions that imply conjectural answers. But one can certainly agree that subsequent works on business cycles would have been written, at least in some of their parts, differently.

¹¹Tinbergen's model of the US economy has heavily drawn from Frisch's "Rocking Horse Model", his own earlier survey on empirical business cycle research (Tinbergen, 1935) as well as the macroeconomic model of the Netherlands, known as the "Dutch Model" (Morgan, 1990, pp. 102ff.), that he developed on request from the Dutch Economic Association. The latter was originally published in Dutch language in 1936 and later revised and republished in English (Tinbergen, 1937, 1959).

¹²The review essay by Keynes (1939) published in *The Economic Journal* had probably the biggest impact. We discuss his criticisms in the following subsection 2.1.2.

the facts, it is still possible that there is another theory, also in accordance with the facts, which is the 'true' one, as may be shown by new facts or further theoretical investigations. Thus the sense in which the statistician can provide 'verification' of a theory is a limited one. (Tinbergen, 1939a, p. 12)

In the second volume, Tinbergen presented one of the first large-scale macroeconomic models that would become an important source of inspiration for post war modeling attempts. He is very careful not to draw too strong conclusions from the statistical analysis. His main claims are that monetary factors have not contributed much to the Great Depression (Tinbergen, 1939b, p. 185), that underconsumption theories of the business cycle provide better explanations than overinvestment theories (p. 190), and most importantly, without going into specific recommendations, that adequate policies could have prevented or at least eased the depression (p. 193). This is a stronger and more important claim than it seems at first glance, as it is a step away from the view that the business cycle is an inevitable periodic phenomenon with causes ultimately out of reach, such as for example the sunspot theory of Jevons (1884), and the Venus theory of Moore (1914, 1923).¹³

Of the numerous works by the early members of the Econometric Society, Frisch's theoretical contributions and Tinbergen's applications had arguably the biggest impact on the broader discipline of economics. That they jointly won the first Nobel Memorial Prize in economics in 1969, is a strong indicator for this claim. In any way, their work constitutes the first pillar of what became large-scale Keynesian macroeconometrics - the first dominant approach of modern economic policy analysis that flourished under the support of the Cowles Commission after World War II.

2.1.2 Keynesian Economics

The second pillar on which the large-scale models of the post war decades were built is Keynesian economics. It is important to note again that Keynes himself cannot be considered a large-scale Keynesian macroeconomist. He did not engage in this kind of model building and theory testing. In fact, he had at best an ambiguous if not outright critical view on econometrics and mathematical economics in general, and Tinbergen's work in particular. Hence, we will first review Keynes's famous critique of Tinbergen as well as his views on mathematical economics,

¹³These earlier works can be considered precursors to econometric attempts at explaining the business cycle. For a concise summary of the two theories in their relation to later econometric work, see Morgan (1990, pp. 18ff.).

before we recapitulate the central ideas and conclusions from his *General Theory*. Then, we will briefly discuss the IS-LM formalization of Keynes's theory, which has become the flagship of Keynesian economics and the baseline model for Lawrence Klein and his collaborators.

Keynes's Critique of Econometrics

Keynes's attitude towards mathematical economics and econometrics has been widely discussed (e.g. Patinkin, 1976; Stone, 1980; Leeson, 1998; Louçã, 1999; Morgan, 1990, pp. 121ff.; Garrone and Marchionatti, 2004; Keuzenkamp, 2004, pp. 73ff.). The fact that he "was less interested than most people in keeping up an appearance of consistency" (Stone, 1980, p. 55) has not facilitated the debate about his role in the development of econometrics. Some consider him to be a "benefactor of econometrics" (Stone, 1980, p. 88), and argue that there is "no evidence for regarding Keynes as a critic and an opponent of econometric work *per se*" (Garrone and Marchionatti, 2004, p. 24). Others have taken his harsh critique of Tinbergen (1939a) as sufficient evidence to regard him as an "*a priori* anti-econometrician", which is probably an overstatement.¹⁴

As often is the case, the answer may lie somewhere in between, depending on what elements of econometrics are actually discussed. Patinkin (1976, p. 1104) calls Keynes a "casual empiricist", willing to rely on econometric analysis for illustrative purposes and when it fits his preconceptions.¹⁵ However, Keynes (1939) obviously rejected the inductive element of testing

¹⁴Garrone and Marchionatti (2004, p. 1) claim this to be the long prevailing view on Keynes without providing direct references. They then argue against this view. It is indeed very difficult to distill a mainstream view on that question. The work in which Keynes could most confidently be said to follow an *a priori* approach is his *Treatise on Probability* (Keynes, 1921), where he advocates a conception of probability understood as "degrees of rational belief" (p. 2), which is purely based on logic. Here, Keynes is opposed to the frequentist interpretation of probability later developed among others by Richard von Mises (1928b). This latter conception is empirical in nature. Interestingly, Ludwig von Mises, the most outspoken advocate of *a priori* economics, would adopt his brother's frequency theory of probability (von Mises, 1998, pp. 106ff.) without referencing to his work. Hence, in the field of probability theory, one might say that John Maynard Keynes was more of an *a priori* theorist than Ludwig von Mises. When it comes to Keynes's broader views, however, including economics in general, there seems to be widespread disagreement. O'Donnel (1990) classifies Keynes as a rationalist. Littleboy and Mehta (1983) classify him as an empiricist. Those economist who are inclined to econometrics and consider themselves to be Keynesian to some extent, have usually looked at Keynes's critique of econometrics with some contempt or even ridicule. Klein (1951, p. 450) wrote:

[Keynes's] review of Tinbergen's celebrated study for the League of Nations was one of his sorriest professional performances. Many econometricians have remarked on Keynes's review with the comment that he simply did not understand the methods he was criticizing and failed to see at what Tinbergen was aiming, a type of comment Keynes frequently used to characterize his own critics.

Stone (1980, p. 61) calls the review "a model of testiness and perverseness." Other economists have acknowledged the validity of Keynes's critique and argue that it remains relevant to this day (Hendry, 1980; Rowley, 1988; McAleer, 1994).

¹⁵Patinkin (1976, p. 1103) provides an excerpt from a letter that Tinbergen has written to him on May 25, 1976.

business cycle theories contained in Tinbergen's work and pointed out that the statistical material has to satisfy certain conditions for his econometric methods to be applicable. These conditions are not sufficiently discussed or simply ignored, and the methods are applied even though they are manifestly not satisfied. Keynes arranged his critique along the following six points.

The first point refers to the *completeness* of the list of causal factors considered in the analysis. If certain causal factors have been identified beforehand, Tinbergen claims to be able to quantify their relative impact on the business cycle. Keynes points out that not only must the causality of the factors be determined beforehand, but also *all* of the causal factors must be taken into account:

For example, suppose three factors are taken into account, it is not enough that these should be in fact *veræ causæ*; there must be no other significant factor. If there is a further factor, not taken account of, then the method is not able to discover the relative quantitative importance of the first three. (Keynes, 1939, p. 560)

This is so because the effect of the other significant factor could be falsely attributed to the factors taken account of and thereby distort the measurement. This would today be referred to as *identification bias* and is one of the most fundamental problems for any econometric inquiry.¹⁶

Next, all of the relevant factors have to be measurable, in principle, and the econometrician needs "adequate statistical knowledge of their measure" (p. 560) in practice.

If it is necessary that all the significant factors should be measurable, this is very important. For it withdraws from the operation of the method all those economic problems where political, social and psychological factors, including such things as government policy, the progress of invention and the state of expectation, may be significant. In particular, it is inapplicable to the problem of the Business Cycle. (p. 561)

Tinbergen tells the following anecdote about Keynes which illustrates the point beautifully:

In the Statistical Office of the Netherlands we had attempted to estimate a number of substitution price elasticities for imports. We did find quite a few cases where it was around the famous figure of 2 which Keynes assumed in his study about the transfer capacity of Germany [(Keynes, 1929, p. 5)]. When I told him about our results I thought he would be glad that his assumption had shown to be true. What he said, however, was: isn't it nice that you found the correct figure?

¹⁶There is another possibility under which, in principle, a quantification of the relative importance of the causal factors would be possible, namely when the factors that are not taken account of exert no independent effect on the business cycle for the period during which the data are collected. Usually however, this cannot be guaranteed in economics.

The third point, after *completeness* and *measurability*, concerns the *independence* of the causal factors. Keynes argues that when the factors are not independent, “we lay ourselves open to the extra-ordinarily difficult and deceptive complications of “spurious” correlation.” Moreover,

Prof. Tinbergen is concerned with “sequence analysis”; he is dealing with non-simultaneous events and time-lags. What happens if the phenomenon under investigation itself reacts on the factors by which we are explaining it? For example, when he investigates the fluctuations of investment, Prof. Tinbergen makes them depend on the fluctuations of profit. But what happens if the fluctuations of profit partly depend (as, indeed, they clearly do) on the fluctuations of investment? Prof. Tinbergen mentions the difficulty in a general way in a footnote to p. 17, where he says, without further discussion, that “one has to be careful.” But is he? (p. 561)

In Keynes’s view he is not. In practice, “Tinbergen seems to be entirely indifferent whether or not his basic factors are independent of one another.” (p. 562) This is problematic as it might produce what is commonly referred to as *simultaneous equation bias*.

The next point, which attacks the assumed *linearity* of all the functional relationships in Tinbergen’s model, is one of less fundamental importance today as modern computers have rendered the treatment of nonlinear functions much more feasible. It would still be a crude assumption for most applications, but it has become much easier to avoid.¹⁷

The fifth problem that Keynes identifies is Tinbergen’s treatment of *trends and time-lags* that are added to his equations:

To the best of my understanding, Prof. Tinbergen is not presented with his time-lags, as he is with his qualitative analysis, by his economist friends, but invents them for himself. This he seems to do by some sort of trial-and-error method. That is to say, he fidgets about until he finds a time-lag which does not fit in too badly with the theory he is testing and with the general presuppositions of his method. (p. 565)

The selection of trends is equally arbitrary argues Keynes. Tinbergen is aware of the problem and even admits at one instant in a footnote that “the trend chosen [...] may be somewhat biased

¹⁷The problem of assuming linear relationships where there actually are nonlinear relationships also falls under the category of *identification bias*. Keynes comments on the exclusive use of linear relationships rather bluntly:

But it is a very drastic and usually improbable postulate to suppose that all economic forces are of this character, producing independent changes in the phenomenon under investigation which are directly proportional to the changes in themselves; indeed, it is ridiculous. (p. 564)

by the fact that the period starts with a boom year and ends with a slump year.” (Tinbergen, 1939a, p. 47) Yet, Keynes laments: “But he is not disturbed, since he has persuaded himself, if I follow him correctly, that it does not really make any difference what trend line you take.” (Keynes, 1939, p. 566) It surely does and it is a problem that should be kept in mind, since arbitrary choices of trends and time-lags can help coat serious deficiencies of the econometric model through *overfitting*.

In the last section of his review, Keynes criticizes the inductive aim of the study in general and calls Tinbergen’s work a “singularly unpromising project in the present state of our knowledge.” (p. 567) Although Tinbergen is very careful in the formulation of his conclusions and “appears to be solely concerned with statistical description”, the “ultimate goal” outlined in the preface “is surely an inductive one.” Keynes asks: “If the method cannot prove or disprove a qualitative theory, and if it cannot give a quantitative guide to the future, is it worth while? For, assuredly, it is not a very lucid way of describing the past.” (p. 566) Here, Keynes makes clear that from an epistemological point of view Tinbergen’s work is unproblematic as a “piece of historical curve fitting and description”, but that any inductive claims “with reference to the future as well as the past” are unjustified for the above reasons.¹⁸

Keynes’s critique caught much attention and sparked off a lively debate about econometric methodology not least because of its rather snarky tone. At one point he comments on Tinbergen’s approach:

It becomes like those puzzles for children where you write down your age, multiply, add this and that, subtract something else, and eventually end up with the number of the Beast in Revelation. (p. 562)

These cynical remarks are characteristic of Keynes and should not be taken as proof for his rejection of econometrics in general. He made an oft-cited and similarly disparaging comment on the general use of mathematics in economics in his *General Theory*:

It is a great fault of symbolic pseudo-mathematical methods of formalising a system of economic analysis, such as we shall set down in section VI of this chapter, that they expressly assume strict independence between the factors involved and lose all their cogency and authority if this hypothesis is disallowed; whereas, in ordinary

¹⁸The problems indicated by Keynes are valid and important. His discussion is however not exhaustive. There are other points to make, which are more fundamental and encompass Keynes’s critique. They will be discussed in the second part of the study. See chapter 6, in particular, section 6.2.2.

discourse, where we are not blindly manipulating but know all the time what we are doing and what the words mean, we can keep ‘at the back of our heads’ the necessary reserves and qualifications and the adjustments which we shall have to make later on, in a way in which we cannot keep complicated partial differentials ‘at the back’ of several pages of algebra which assume that they all vanish. Too large a proportion of recent ‘mathematical’ economics are merely concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols. (Keynes, 1936, p. 297-298)

Nonetheless, as indicated in the quote, Keynes himself provided a mathematical representation of at least parts of his system in the very same chapter that contains the above quotation (Keynes, 1936, ch. 21, section VI) and also elsewhere (Keynes, 1936, ch. 20, section I). His critique of mathematics may thus just be seen as an apology for applying it himself (Patinkin, 1976, p. 1094). So his attitude towards mathematical economics in the *General Theory* is ambivalent, but in total the work was at its time “among the more mathematical” (Patinkin, 1976, p. 1094), and so was Keynes’s earlier two-volume *Treatise on Money* (Keynes, 1958, 1960), originally published in 1930. And indeed, Keynes engaged in a sort of rudimentary econometric estimation of his investment multiplier based on data provided by Simon S. Kuznets (1901-1985) for US national income (Kuznets, 1934). Without providing much guidance on how he calculated his estimate and showing some doubts about the accuracy of the data, Keynes states that “the multiplier seems to have been less than 3 and probably fairly stable in the neighbourhood of 2.5” (Keynes, 1936, p. 118) for the US from 1925 to 1933.

Stone (1980) gathers some evidence that Keynes’s attitude towards econometrics had become more favorable by the end of his life, and states that “all his asperity towards econometrics seems to have evaporated.” (p. 63) Keynes wrote in private correspondence to Alfred Cowles, who wanted him to become president of the Econometric Society: “whilst I am interested in econometric work and have done something at it at different times in my life, I have not recently written anything significant or important along these lines, which make me feel a little bit of an impostor.” He nonetheless would accept the honors in 1944 and was president of the Society until 1945. And in another letter to Cowles on July 23, 1945, Keynes praised Tinbergen: “[T]here is no-one more gifted or delightful or for whose work one could be more anxious to give every possible scope and opportunity.” (Stone, 1980, p. 63)

However, it seems as if Keynes was especially interested in the descriptive statistical and econometric work on national accounting and its application to economic history and the measurement of national wealth. There really is no evidence that he had ever changed his mind on the possibility of empirically testing economic theory. It is precisely with respect to national accounting, as advanced by Kuznets in the US and Colin Clark (1905-1989) in the UK (e.g. Clark, 1932, 1938), that Stone (1980, p. 62) makes his strong claim that “there is no doubt that in its day Keynes’s book [*The General Theory*] had done probably more than any other to encourage the systematic estimation of national accounts magnitudes and the construction of econometric models.”

The General Theory

Keynes’s *magnum opus* has probably caught more attention than any other 20th century economics book, although it is widely regarded as an extremely cumbersome and difficult read. Paul A. Samuelson (1915-2009) famously stated shortly after Keynes’s death that “it is a badly written book, poorly organized; any layman who, beguiled by the author’s previous reputation, bought the book was cheated of his 5 shillings. It is not well suited for classroom use. [...] It is arrogant, bad-tempered, polemical, and not overly-generous in its acknowledgments”, and nonetheless he calls it “in short [...] a work of genius.” (Samuelson, 1946, p. 190) He also admitted that he himself and “no one else in Cambridge, Massachusetts, really knew what it was about for some 12 to 18 months after its publication. Indeed, until the appearance of the mathematical models of Meade, Lange, Hicks, and Harrod there is reason to believe that Keynes himself did not truly understand his own analysis.” (p. 188)

However, what needs to be emphasized about the work, according to Samuelson, are not the confusions and contradictions when it comes to technical details, but the basis for the “new system” (p. 190) of thinking about the economy as a whole that it outlined. Samuelson among others helped to specify and formulate this system more clearly. Indeed, one of the book’s characteristic elements is the reasoning in terms of macroeconomic aggregates: “This book [...] has evolved into what is primarily a study of the forces which determine changes in the scale of output and employment as a whole” (Keynes, 1936, p. vii). In this sense it has contributed to the split between micro and macro analysis,¹⁹ and its publication is therefore often regarded

¹⁹In chapter 21, Keynes (1936, p. 293) expressed this perceived dichotomy between micro and macro clearly. He also points out that monetary theory is necessary in the analysis of the macro system. Indeed, modern macroeconomics has become predominantly occupied with monetary theory and policy analysis:

as the beginning of modern macroeconomics (De Vroey and Malgrange, 2011), a historical classification that is shared in this thesis. It initiated the “Keynesian Revolution” and founded what is sometimes referred to as the “New Economics.”²⁰

In order to understand the gist of the book and its importance for the topic of this thesis, we will briefly discuss some of its key concepts and their relation to monetary policy and central banking - of course, without claiming completeness. The presentation is necessarily selective, but tries to focus on the essentials. As one important conclusion, we will see that Keynes has provided a rationale for the Phillips curve trade-off between price inflation and unemployment more than 20 years before it became popular under this name.²¹

The first crucial element in the *General Theory* is the concept of *effective demand*, which Keynes simply defined as the demand at the point of intersection of an aggregate demand and an aggregate supply function, both of which are understood as functions of the number of people employed (Keynes, 1936, p. 25). Hence, Keynes established right from the beginning a link between *demand* and employment, even if at first only terminologically, since the point of intersection at which demand and supply are equal could, with the same plausibility, also be associated with something like *effective supply*. Yet, Keynes conceived of demand as being the driving force of the economy, which becomes clear throughout the book, and particularly in the paragraphs that immediately follow his definition of effective demand. He ridicules Say’s law into meaning “Supply creates its own Demand”, a view that he claims “underlies all orthodox economic theory”, that is, the “classical doctrine”. Stated in this way it admittedly merits no refutation, but Keynes is not shy of triumphantly providing his.²²

The division of economics between the theory of value and distribution on the one hand and the theory of money on the other hand is, I think, a false division. The right dichotomy is, I suggest, between the theory of the individual industry or firm and of the rewards and the distribution between different uses of a given quantity of resources on the one hand, and the theory of output and employment *as a whole* on the other hand. So long as we limit ourselves to the study of the individual industry or firm on the assumption that the aggregate quantity of employed resources is constant, and, provisionally, that the conditions of other industries or firms are unchanged, it is true that we are not concerned with the significant characteristics of money. But as soon as we pass to the problem of what determines output and employment as a whole, we require the complete theory of a monetary economy.

Interestingly, it was again Ragnar Frisch who first used the term *macroeconomics* in lectures. It was probably Erik Lindahl (1891-1960) who first used it in print (De Vroey and Hoover, 2004, p. 2, fn. 1). Schumpeter (2006, p. 278) also points out that the term *macroanalysis* is owed to Frisch.

²⁰See for example Harris ([1947] 2010) for a collection of essays that generally praise the impact of the “New Economics”, or Hazlitt (1959) for a comprehensive critique thereof. See also Tobin (1974) for a somewhat less resolute reassessment. Schumpeter ([1954] 2006, p. 215) emphasized the emergence of a veritable intellectual movement out of Keynes’s oeuvre and compares the “fidelity of the orthodox Keynesians to the message of Keynes” with that of the Marxists to the message of Marx and that of the Physiocrats to the message of Quesnay.

²¹For a brief history of the early Phillips curve see section 2.3 on page 80.

²²A much more detailed and critical discussion can be found in Hazlitt (1959, ch. 3) and Hazlitt ([1960] 1995),

Keynes's mutation of Say's law is in fact an interesting illustration of the problems with aggregation. If one is inclined to think in terms of the aggregates of demand and supply only, Say's law can neither correctly be understood, nor restated. It can only preserve its meaning in terms of individual supplies of goods, namely in the sense that the supply of a specific good embodies the demand for many other goods. In other words, in an economic system that is characterized by the division of labor specific goods are produced by certain individuals, groups or firms in excess of their own needs, precisely in order to be able to trade and thus demand other goods.²³

In the next step, Keynes provides a bird's-eye overview of his framework, and explains that the point of intersection of the aggregate demand and supply functions, and hence effective demand and the level of employment, are determined by the *propensity to consume* and the *inducement to invest* (p. 27). He explains that the "psychology of the community" is such that the propensity to consume is below unity, so that the output produced for any given level of employment will exceed consumption. "Thus, to justify any given amount of employment there must be an amount of current investment sufficient to absorb the excess of total output over what the community chooses to consume when employment is at the given level." Otherwise, the level of employment would be unsustainable and would have to shrink.

Hence, given the propensity to consume and the inducement to invest there is "only one level of employment consistent with equilibrium", that is equality between aggregate demand and supply. The central claim of the *General Theory* is that

there is no reason in general for expecting [this level] to be equal to full employment.

in particular the chapter "Lord Keynes and Say's Law" (pp. 315-321) by von Mises.

²³This reformulation of Say's law comes much closer to the original meaning expressed in the *Traité d'économie politique* originally published in 1803. In chapter 15 of the second edition (1841), we read:

Il est bon de remarquer qu'un produit terminé offre, dès cet instant, un débouché à d'autres produits pour tout le montant de sa valeur. En effet, lorsque le dernier producteur a terminé un produit, son plus grand désir est de le vendre, pour que la valeur de ce produit ne chôme pas entre ses mans. Mais il n'est pas moins empressé de se défaire de l'argent que lui procure sa vente, pour que la valeur de l'argent ne chôme pas non plus. Or, on ne peut se défaire de son argent qu'en demandant à acheter un produit quelconque. On voit donc que le fait seul de la formation d'un produit ouvre, dès l'instant même, un débouché à d'autres produits.

C'est pour cela qu'une bonne récolte n'est pas seulement favorable aux cultivateurs, et qu'elle l'est en même temps aux marchands de tous les autres produits. On achète davantage toutes les fois qu'on recueille davantage. Une mauvaise récolte, au contraire, nuit à toutes les ventes. Il en est de même des récoltes faites par les arts et le commerce. Une branche de commerce qui prospère fournit de quoi acheter, et procure conséquemment des ventes à tous les autres commerces ; et d'un autre côté, quand une partie des manufactures ou des genres de commerce devient languissante, la plupart des autres en souffrent. (Say, 2011, p. 89)

The effective demand associated with full employment is a special case, only realised when the propensity to consume and the inducement to invest stand in a particular relationship to one another. This particular relationship, which corresponds to the assumptions of the classical theory, is in a sense an optimum relationship. But it can only exist when, *by accident or design*, current investment provides an amount of demand just equal to the excess of the aggregate supply price of the output resulting from full employment over what the community will choose to spend on consumption when it is fully employed.” [emphasis added] (p. 28)

Keynes thus postulates the possibility of an equilibrium state with what he calls *involuntary unemployment*,²⁴ or put differently, he denies any necessary economic forces that would push the system towards full employment. These forces, if existent, are either accidental, for example fluctuations in the psychological moods of the community, or accomplished by design, that is by political design, but they are not inherent in the market process.²⁵ Ensuring full employment, therefore, requires fiscal and monetary policy interventions in order to increase consumption

²⁴It goes without saying that involuntary unemployment in the common sense of the word is a reality that no economist has ever denied. Economists adhering to what Keynes refers to as the classical doctrine, however, would interpret its existence as a deviation from equilibrium that is only transitory in an unhampered market. Keynes argues that it can be perpetual and consistent with equilibrium.

Without going into too much detail, it is interesting to note that Keynes chose to employ a rather confusing classification scheme for unemployment. He defined *involuntary unemployment*, *voluntary unemployment*, as well as *frictional unemployment*. Hazlitt (1959, pp. 15-16) comments:

Here is a classification that would trouble any logician. Unemployment must be either voluntary or involuntary. Surely these two categories exhaust the possibilities. There is no room for a third category. “Frictional” unemployment must be either voluntary or involuntary. In practice it is likely to be made up of a little of each. “Frictional” unemployment may be involuntary through illness, disability, failure of the firm, [...] “Frictional” unemployment may be voluntary because a family has moved to a new place, because a man has relinquished an old job in the hope of getting a better one [...] Such unemployment is the result of a *decision*, good or bad, on the part of the man who is unemployed. “Friction,” though a traditional term, is perhaps not the most fortunate metaphor to describe it.

²⁵Keynes did not engage in a logical analysis and refutation of the “classical” theory, which he seemed to reduce entirely to the writings of Alfred Marshall and Arthur C. Pigou (1877-1959). Yet, exactly this would have been expected from a strict “*a priori* anti-econometrician.” Instead, he either ignored important aspects of it or tended to appeal to popular sentiment, as is shown in the following passage:

Obviously, however, if the classical theory is only applicable to the case of full employment, it is fallacious to apply it to the problems of involuntary unemployment - if there be such a thing (and who will deny it?). The classical theorists resemble Euclidean geometers in a non-Euclidean world who, discovering that in experience straight lines apparently parallel often meet, rebuke the lines for not keeping straight - as the only remedy for the unfortunate collisions which are occurring. Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required today in economics. We need to [...] work out the behaviour of a system in which involuntary unemployment in the strict sense is possible. (Keynes, 1936, pp. 16-17)

and stimulate investment in such a way that a level of effective demand consistent with full employment is brought about.

There are two other important features of the Keynesian system that lead to this conclusion, both shall be explained briefly in the following. First, Keynes dwelt on the dichotomy between saving and investment. He claims that the decision to save, and thus to abstain from consumption today, is not equivalent to investment. In other words, a diminished demand for consumption goods will not necessarily be offset by an increased demand for investment goods, and as a result effective demand and employment might decrease due to savings. Chapter 16 contains a summary of this idea:

[S]ince the expectation of consumption is the only *raison d'être* of employment, there should be nothing paradoxical in the conclusion that a diminished propensity to consume has *cet. par.* a depressing effect on employment.

The trouble arises, therefore, because the act of saving implies, not a substitution for present consumption of some specific additional consumption which requires for its preparation just as much immediate economic activity as would have been required by present consumption equal in value to the sum saved, but a desire for 'wealth' as such, that is for a potentiality of consuming an unspecified article at an unspecified time. The absurd, though almost universal, idea that an act of individual saving is just as good for effective demand as an act of individual consumption, has been fostered by the fallacy, much more specious than the conclusion derived from it, that an increased desire to hold wealth, being much the same thing as an increased desire to hold investments, must, by increasing the demand for investments, provide a stimulus to their production; so that current investment is promoted by individual saving to the same extent as present consumption is diminished. (Keynes, 1936, p. 211)

Keynes insists: "It is of this fallacy that it is most difficult to disabuse men's minds" (p. 212). This brings us to the second feature, which must be understood as the part of Keynes's proposed solution that is of particular importance for this thesis, namely, his treatment of interest rates as a purely monetary phenomenon. Interest rates in the Keynesian view should be pushed down through monetary expansion in order to ensure that the inducement to invest effectively offsets

any possible decline in the propensity to consume. Keynes explains the determination of interest rates as follows:

[T]he rate of interest at any time, being the reward for parting with liquidity, is a measure of the unwillingness of those who possess money to part with their liquid control over it. The rate of interest is not the 'price' which brings into equilibrium the demand for resources to invest with the readiness to abstain from present consumption. It is the 'price' which equilibrates the desire to hold wealth in the form of cash with the available quantity of cash; - which implies that if the rate of interest were lower, i.e. if the reward for parting with cash were diminished, the aggregate amount of cash which the public would wish to hold would exceed the available supply, and that if the rate of interest were raised, there would be a surplus of cash which no one would be willing to hold. If this explanation is correct, the quantity of money is the other factor, which, in conjunction with liquidity-preference, determines the actual rate of interest in given circumstances. (p. 167)

Hence, the extent to which a given increase in the money supply is capable of reducing the interest rate within the Keynesian framework depends on the *liquidity preference* schedule of the community, a concept akin to the demand for money.

The stimulating effect of lower interest rates on investment is then contingent on the schedule of the *marginal efficiency of capital*, a concept that Keynes introduced in chapter 11. It is defined "as being equal to that rate of discount which would make the present value of the series of annuities given by the returns expected from the capital-asset during its life just equal to its supply price [i.e. replacement cost]" (p. 135).²⁶ Hence, investments are stimulated up to the point at which the marginal efficiency of capital reaches the lowered interest rate. Investment

²⁶Keynes explained that this concept corresponds to Fisher's "rate of return over cost" (Fisher, 1930). This, however, has been disputed and identified as "an error propagated by a careless statement" (Alchian, 1955, p. 938). It is relatively easy to recognize this error, since Fisher defined his concept as a crossover rate with respect to two different capital assets: "This hypothetical rate of interest which if used in calculating the present worth of the two options compared would equalize them or their differences (cost and return) may be called the rate of return over cost." (Fisher, 1930, p. 155) Keynes on the other hand defined his marginal efficiency of capital with respect to the cost and return of one and the same capital asset. This means, however, that Keynes's concept can only be seen as identical to Fisher's concept in a very special case, namely when we consider one of the two investment options (assets) to be refraining from investment (no asset at all, that is, cost and return are zero). In that case, in order to find Fisher's rate of return over cost, we would look for the discount rate that renders the present value of the actual asset equal to zero, which corresponds to Keynes's marginal efficiency of capital.

It is not unlikely that other readers would try to argue differently based on other passages of the *General Theory*. However, the concept of marginal efficiency of capital is a good example for the unnecessary confusions, complications and contradictions that Keynes provoked in his book through the use of changing or ambiguous definitions of technical terms (see Hazlitt, 1959, pp. 156ff.).

projects become relatively more profitable the further the interest rate is initially pushed below the marginal efficiency of capital. Accordingly, in cases where the interest rate lies above the marginal efficiency of capital, investments are depressed.²⁷ In fact, Keynes's theory of the trade cycle (chapter 22) is entirely based on the relative fluctuations of the marginal efficiency of capital to the rate of interest.

Thus, an increase in the money supply lowers interest rates depending on the liquidity preference schedule.²⁸ Lower interest rates stimulate investments depending on the schedule of the marginal efficiency of capital. The last element in the chain that determines the impact on effective demand and hence on employment is the *investment multiplier*, which indicates by how much aggregate income will rise due to an initial increase in investment. It is based on the idea that an investment constitutes income for another party, which then is partly spent and constitutes income for still another party, and so on (pp. 113 ff.). This is the basic system that Keynes lays out, indeed with various qualifications and complications.²⁹

The latter, however, do not alter the central conclusion for monetary policy in fighting unemployment: An increase in the supply of money will tend to increase effective demand, "and the increase in effective demand will, generally speaking, spend itself partly in increasing the quantity of employment and partly in raising the level of prices." (p. 296) Thus the *General Theory* provides a rationale for the Phillips curve trade-off between price inflation and unemployment, which Keynes referred to as a "generalised Quantity Theory of Money" (p. 285). This analysis must be taken as an argument for active management of the money supply, that is, central banking, and, more specifically, for central banking under a fiat money standard.

In contrast to the classical theory of employment, which "has been accustomed to rest the supposedly self-adjusting character of the economic system on an assumed fluidity of money-wages", Keynes (1936, p. 257) assumes money wages to be rigid in some form or another.³⁰

²⁷Another way in which monetary policies may exert an influence in Keynes's framework is through their impact on the expectation of the future value of money. He argued that when the value of money is expected to increase (price deflation), the marginal efficiency of capital today diminishes and so investments are held back and employment tends to decrease. Likewise, if the value of money is expected to fall (price inflation), the marginal efficiency of capital today increases and investment projects are undertaken and employment increases.

²⁸Keynes (1936, p. 207) mentions the theoretical possibility of "absolute" liquidity preference, in which case the "monetary authority would have lost effective control over the rate of interest." He also writes: "But whilst this limiting case might become practically important in future, I know of no example of it hitherto." This is the problem known as *liquidity trap* and has been widely discussed (Boianovsky, 2004). It is surprising that Hicks (1937) calls the liquidity trap scenario characteristic of the Keynesian analysis.

²⁹This is the "Keynesian system" that Schumpeter (2006, p. 278) calls the "leading system of Monetary Analysis of today," that is, of the 1940s.

³⁰The assumption of nominal rigidity is also characteristic for the New Keynesian DSGE approach discussed in chapter 5 of this thesis.

The economy left alone will thus not necessarily solve the problem of unemployment. The repercussions of the Great Depression with high unemployment rates throughout the US and Europe added to the success of Keynes's theory, since it presented a promising solution. A number of his intellectual disciples formalized the theory into the well known IS-LM model that has become important in macroeconomic research for some decades and standard material in university teaching until today.

The IS-LM Formalization

Colander (2004, p. 305) calls IS-LM analysis a "creature of pedagogy," and to some extent this is certainly true, especially nowadays. It was the attempt, initiated by John R. Hicks (1904-1989),³¹ to put Keynes's *General Theory* into simple mathematical formulas and graphical representations, and thereby make its central message clearer and more accessible. On the other hand, it was more than mere pedagogy. As De Vroey and Hoover (2004, p. 3) put it: "In effect, it became the organizing theoretical apparatus of the emerging discipline of macroeconomics." The IS-LM model was also the basic module for large-scale Keynesian macroeconometrics and hence for empirical monetary policy analysis in the 1950s and 60s.

Hicks (1937) reduced the *General Theory* to three equations from which two sets of equilibria are derived. The first equation sets an exogenous money supply, M , equal to the liquidity preference L , defined as a downward sloping function of the interest rate i and an upward sloping function of aggregate income I , that is, $M = L(I, i)$. All combinations of i and I that satisfy this equation form an upward sloping curve in the (i, I) -diagram, which Hicks called the LL-curve.

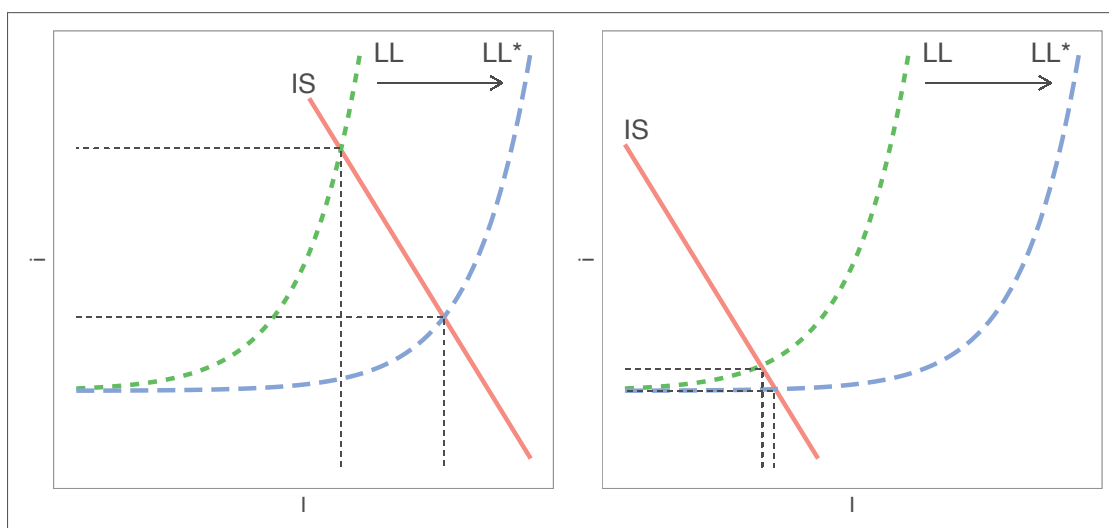
From the other two equations, those combinations of i and I are derived that equilibrate savings and investments. Investments are negatively related to interest rates and savings are positively related to aggregate income. Hence, a downward sloping IS-curve in (i, I) -diagram can be derived. The intersection of the IS and LL curves corresponds to general equilibrium and determines aggregate income I and hence employment and the interest rate i . Both curves are illustrated in Figure 2.1.

The specific shapes and positions of the curves influence the policy conclusions. In particular, Hicks (1937, p. 154) pointed out that:

³¹Hicks's model was one of several works presented at a meeting of the Econometric Society in Oxford in September 1936. James Meade and Roy Harrod contributed two different versions which turned out to be less successful than Hicks's. Two other early attempts to formulate the *General Theory* in mathematical terms can be found in Champernowne (1936) and Reddaway (1936).

This brings us to what, from many points of view, is the most important thing in Mr. Keynes' book. It is not only possible to show that a given supply of money determines a certain relation between Income and interest (which we have expressed by the curve LL); it is also possible to say something about the shape of the curve. It will probably tend to be nearly horizontal on the left, and nearly vertical on the right. This is because there is (1) some minimum below which the rate of interest is unlikely to go, and (though Mr. Keynes does not stress this) there is (2) a maximum to the level of income which can possibly be financed with a given amount of money. If we like we can think of the curve as approaching these limits asymptotically.

Figure 2.1: Two scenarios in the IS-LL (IS-LM) model: effective monetary expansion (left panel); ineffective monetary expansion (right panel)



Now if the shape of the LL-curve is as described above and the IS-curve is close to the right side of the diagram as shown in the left panel of Figure 2.1, then monetary policy can have an effect within the framework of this model. If the money supply is increased, the LL-curve shifts to the right and aggregate income and employment increase. Yet, if the IS-curve lies on the left as in the right panel of Figure 2.1, an increase in the money supply has no effect as the interest rate cannot be pushed down significantly with a rightward shift of the LL-curve. Hicks (1937, p. 155) concludes: "So the General Theory of Employment is the Economics of Depression." In the latter case monetary policy would have almost no effect, and fiscal policies would have to step in by shifting the IS-curve instead through an increase in public spending.

Franco Modigliani (1918-2003) elaborated on Hicks's work and clarified the distinction between the classical model, which would be associated with fully flexible prices and wages, and the Keynesian model, which assumes nominal rigidities, especially downward wage rigidity. Modigliani (1944) developed the standard version of the model (De Vroey, 2000; Rubin, 2004). It was subsequently through the works of Alvin H. Hansen (1887-1975) that the IS-LL model was renamed into IS-LM (investment, saving - liquidity preference, money supply) (Hansen, 1949, 1953).

The IS-LM model was developed further in different directions. Extended to the case of an open economy, it led to the Mundell-Fleming model (Fleming, 1962; Mundell, 1963). Another version that incorporates the labor market more specifically is known as AD-AS (aggregate demand - aggregate supply) model (Blanchard, 1997). There were also versions that explicitly added the credit market (Larson, 2003). The IS-LM model is still taught in introductory courses at the university-level, although it plays practically no role anymore in graduate studies or theoretical and empirical macroeconomic research. Yet, until the 1970s it was widely used. And it was precisely an extended IS-LM formalization to which the Frisch-Tinbergen approach of econometric model building was applied by the end of the 1940s. This conjunction of econometrics and Keynesianism resulted in the development of what we call large-scale Keynesian macroeconomic models. Even though Keynes himself might have looked at these models with grief, his work was inspirational: "It now seems clear that *The General Theory* was a tremendous stimulus, not only to macroeconomic theory in general, but to macroeconomic model-building in particular." (Bodkin et al., 1991, p. 18)

2.2 A Description of Large-Scale Keynesian

Macroeconometric Models

In the previous section we have roughly traced the early histories of the two developments that led to the first leading approach in modern economic policy analysis. From the 1940s onward their histories have to a significant extent become one. The synthesis of Keynesianism and econometrics is suitably labeled large-scale Keynesian macroeconometrics.

One of the most influential representatives of this approach is Nobel laureate Lawrence R. Klein (1980), who was Paul A. Samuelson's first doctoral student at the Massachusetts Institute of Technology, himself a Nobel laureate (1970) and a devoted follower of Keynesian

economics. Out of Klein's doctoral dissertation emerged the book *The Keynesian Revolution* (Klein, 1950a).³² Klein's later work on large-scale macroeconomic modeling was seen as a natural extension and elaboration of the Keynesian system. He himself and two co-authors stated in retrospect that "a number of the concepts of Keynes's *General Theory* appeared to cry out for empirical verification (or refutation)" (Bodkin et al., 1991, p.19), and this "empirical extension became his life's work." (De Vroey and Malgrange, 2010, p. 1)

It was Jacob Marschak (1898-1977), director of the Cowles Commission from 1943 until 1955, who hired Klein after his dissertation for developing a new macroeconomic model of the postwar American economy. As Klein himself put it: "Marschak prevailed on me to drop all other job search activities and join the staff at Cowles to develop what he said the country needed desperately - a new "Tinbergen" model of the US economy" (Klein, 1991, p. 108). He continues: "When he recruited me, it was explicitly to prepare model specifications according to received economic theory, using both microeconomics, macroeconomics, and aggregation or index theory to bridge the gap between them" (Klein, 1991, p. 109).

Monograph no. 11 of the Commission was Klein's *Economic Fluctuations in the United States, 1921-1941* (Klein, 1950b). The book contained three models, the last of which was of particular significance. It was the first in a series of models to be developed over the 50s and 60s, among which were the Klein-Goldberger Model (Klein and Goldberger, 1955), the Wharton Models (Evans, 1963; Klein, 1964; Evans and Klein, 1967; Haitovsky et al., 1974), and the Brookings Models (Duesenberry et al., 1965; Fromm and Taubman, 1968; Duesenberry et al., 1969; Fromm and Klein, 1975). Klein was involved in all of them.

In this section we describe two of these models in some detail. First, an overview of the general setup of the models is given and then we look at some of the specifics of the Klein-Goldberger Model and the Brookings Model, and particularly the conclusions for central bank policy that were drawn from them.

2.2.1 The Standard Setup: An Augmented IS-LM Model

Webb (2005, p. 24) points out that traditional macroeconomic models of the 1960s were essentially Keynesian, "since their basic design takes as given the idea that prices fail to clear markets, at least in the short run." They are elaborations "of the textbook IS-LM model aug-

³²For a brief overview of Klein's professional career and the impact of his work see for example Visco (2014), and some of the references cited therein, in particular the autobiographical essays Klein (1980, 1986, 1992).

mented with a Phillips curve.”³³ Klein essentially attempted to provide empirical content for extended versions of the theoretical models that have been developed by Hicks (1937) and Modigliani (1944). In Frisch’s terminology, he thus added the empirical-quantitative element to the theoretical-quantitative IS-LM framework.

The general idea of how to construct these kinds of models is well described in Modigliani (1944, p. 46):

As a first step in the analysis, we must set up a system of equations describing the relation between the variables to be analyzed. In doing this we are at once confronted with a difficult choice between rigor and convenience; the only rigorous procedure is to set up a complete “Walrasian” system and to determine the equilibrium prices and quantities of each good [...]. The alternative is to work with a reduced system: we must then be satisfied with the rather vague notions of “physical output,” “investment,” “price level,” etc.

Hence, the models were “Walrasian” only in the limited sense that they were systems of a specific number of simultaneous equations, which were capable of determining an equal number of endogenous variables representing the entire economy on an aggregated level.³⁴ As a minimum these models contain an aggregate consumption function (C_t), an aggregate investment function (I_t), and a money demand (liquidity preference) function (M_t), such as the following:³⁵

$$C_t = \alpha_1 + \beta_{11}(Y_t - T_t) + \epsilon_{1t},$$

$$I_t = \alpha_2 + \beta_{21}(R_t - \pi_{t+1}^e) + \epsilon_{2t},$$

³³See also Visco (2014, p. 613) who argues that the theoretical framework in Lawrence Klein’s work was essentially Keynesian, and Colander (2004, p. 307) who mentions the fundamental role that the IS-LM framework had played for empirical model building in the 1960s. The latter writes:

In the 1960s, the IS-LM model was not only a stepping-stone to theoretical macro, it was also a stepping-stone to empirical macro and the large econometric models that were then the center of advanced macroeconomic forecasting and policy analysis. When students learned IS-LM in the 1960s, they were learning a simple example of the much larger econometric models, which had thousands, rather than tens, of equations, but otherwise had the same structure.

³⁴See also Klein (2000, p. 151), who explicitly draws the connection between Ragnar Frisch’s work in macroeconomics and Walras’s work in microeconomics:

Ragnar Frisch, Michal Kalecki and a few others did for macroeconomics what Leon Walras and Vilfredo Pareto did for microeconomics; they formulated systems of equal numbers of equations and (endogenous) variables that would be able to determine the latter as solutions to simultaneous equations.

³⁵The following model comes from Webb (2005, p. 24).

$$M_t = \alpha_3 + \beta_{31}Y_t + \beta_{32}R_t + \epsilon_{3t},$$

where Y_t and T_t are the real GDP and tax payments at point t , respectively. Consumption at point t is thus defined as a function of real disposable income $Y_t - T_t$. Investment is a function of the expected real interest rate, that is, the nominal interest rate R_t minus the expected inflation rate for $t + 1$, denoted π_{t+1}^e . Money demand in this example is understood as a function of real GDP and nominal interest rates.

This basic system is typically extended with a number of identities and equations that describe how certain variables are related to each other, price inflation expectations are formed and, most importantly, establish a type of Phillips curve relationship. In a typical Keynesian model there would be two equations for the latter purpose, one that relates unemployment to wage inflation and another that relates wage inflation to price inflation (Webb, 2005, p. 24, fn. 2).³⁶

Moreover, the equations of the basic model can each be disaggregated into several sub-equations. Consumption can be separated into consumption of durables and non-durables, or consumption in various different sectors of the economy, such as manufacturing, agriculture, industrial output, etc. Investment can be separated into investments in different sectors as well. This disaggregation can in principle provide more exact and detailed results, but was not always feasible in practice, due, for example, to a lack of data or restrictions of computational power.

Large-scale Keynesian macroeconometrics was made possible through systematic public data collection and enormous technological advances in computing capacity. The American technology and consulting corporation IBM, for example, was involved in several research projects and provided machine time and technological support in the processing of macro data and estimation of equations (Klein, 1964, p.11). The models have gradually grown in size and detail and incorporated more and more variables and equations, as will be seen in the following

³⁶Today, a more modern version of the Phillips curve is typically used. It relates price inflation to a measure of the discrepancy between actual GDP and potential GDP, such as:

$$\pi_t = \alpha_4 + \beta_{41} \frac{Y_t}{Y_t^p} + \epsilon_{4t}.$$

A brief presentation and discussion of the New Keynesian Phillips curve is presented in chapter 5 on page 196. Expected price inflation was typically modeled as a function of current and past price inflation, known as adapted expectations. An easy example is the following:

$$\pi_{t+1}^e = \theta_{51}\pi_t + \theta_{52}\pi_{t-1}.$$

Finally, as the last equation of the model some version of the famous national accounting identity is used, the most basic of which is:

$$Y_t = C_t + I_t + G_t.$$

examples that are presented in chronological order according to their date of appearance.

2.2.2 The Klein-Goldberger Model

Lawrence Klein, while hired by the Cowles Commission from 1944 until 1947, which was during that time based at the University of Chicago, developed three models in his *Economic Fluctuations in the United States, 1921-1941* published only a few years later (Klein, 1950a). The third model proved to be of particular importance as a forerunner of Klein and Goldberger (1955). The main drawbacks of the model were that it still employed rather basic econometric methods and that the rough and intermittent data that were available and used in the estimation procedures did not extend to the post-war economy. It therefore was like Tinbergen's models only a pioneering effort towards the eventual development of models that could be used effectively for post war economic planning, forecasting and policy analysis.

At the University of Michigan, where Klein worked from 1949 until 1954, he developed such a model in collaboration with Arthur S. Goldberger (1930-2009). The Klein-Goldberger Model, although today labeled "medium size," is the first prime example of large-scale Keynesian macroeconometric modeling and it was itself not considered to be a "once-and-for-all" effort (Klein and Goldberger, 1955, p. 1; Bodkin et al., 1991, p. 57). As Klein and two co-authors describe it in retrospect:

It was presented as a part of a more continuous programme in which new data, reformulations and extrapolations were constantly being studied. The model consisted of 15 structural equations, five identities and five tax-transfer auxiliary relationships. [...] In the genealogy of macroeconometric models, no other model has left such a vast legacy of style and flavour as the Klein-Goldberger Model. (Bodkin et al., 1991, p. 57)

We will first look into the setup and estimation of the model and then summarize its main applications and conclusions. It should be kept in mind, however, that the model was not exclusively built for monetary policy analysis. Almost to the contrary, an emphasis was put on fiscal policies, especially taxation schemes and government expenditure. The emphasis shifted to some extent with the discovery and popularization of the Phillips curve towards the end of the 1950s and early 60s. The Klein-Goldberger Model is nonetheless of importance for our purposes

as it illustrates very well how these types of models were built and on what basis general policy conclusions were drawn.

Model Setup and Estimation

The Klein-Goldberger model contains a consumption function and an investment function, just like the basic model outlined in the previous subsection. Their specifications, however, are much more complex. So, aggregate consumption is not simply a function of aggregate disposable income, $Y - T$, but income sources are disaggregated into three different categories: wage income, which itself is separated into private and public employee wage income, W_1 and W_2 , respectively; farm income, which is separated into farm income minus farm subsidies on the one hand and farm subsidies on the other, A_1 and A_2 , respectively; as well as non-wage non-farm income, P . All three forms of income are taxed differently, leading to the respective tax amounts T_W , T_A and T_P .

Consequently, three kinds of disposable incomes are incorporated as explanatory variables of aggregate consumption in year t , C_t : disposable employee compensation, $(W_1 + W_2 - T_W)_t$, disposable farm income, $(A_1 + A_2 - T_A)_t$, and disposable non-wage non-farm income, $(P - S_P - T_P)_t$, where S_P denotes corporate savings. Moreover, the number of persons in the US, N_P , liquid assets (currency, bank deposits, saving and loan shares, and U.S. government bonds) held by persons at the end of the previous year (lagged by one year), $(L_1)_{t-1}$, as well as consumption of the previous year C_{t-1} serve as explanatory variables. The consumption function of the Klein-Goldberger model is thus given by:

$$C_t = \alpha_0 + \alpha_1(W_1 + W_2 - T_W)_t + \alpha_2(P - T_P - S_P)_t + \alpha_3(A_1 + A_2 - T_A)_t + \alpha_4C_{t-1} + \alpha_5(L_1)_{t-1} + \alpha_6(N_P)_t + u_{1,t}, \quad (2.1)$$

where $u_{1,t}$ denotes a random error term.

Similarly, aggregate investment in the Klein-Goldberger model is not simply a function of interest rates and price inflation expectations, $R - \pi^e$, but of disposable non-wage income plus capital consumption charges (depreciation, D), both of the current and the previous year, $(P - T_P + A_1 + A_2 - T_A + D)_{t/t-1}$, as well as the end-of-year liquid assets held by enterprises lagged by one year, $(L_2)_{t-1}$, the average yield on corporate bonds during the previous year, $(i_L)_{t-1}$ and the end-of-year stock of private capital of the previous year, K_{t-1} :

$$I_t = \beta_0 + \beta_1(P - T_p + A_1 + A_2 - T_A + D)_t + \beta_2(P - T_p + A_1 + A_2 - T_A + D)_{t-1} + \beta_3(i_L)_{t-1} + \beta_4K_{t-1} + \beta_5(L_2)_{t-1} + u_{2,t} \quad (2.2)$$

As opposed to only two coefficients in the basic model outlined above, the consumption and investment functions in the Klein-Goldberger model contain seven and six coefficients, respectively.

The aggregate money demand or liquidity preference function is separated into a household liquidity preference equation and a business liquidity preference equation. The model also contains a corporate savings equation, a relation between corporate profits and non-wage non-farm income, a depreciation equation, a demand for labor equation, a production function, a labor market adjustment equation, an import demand function, an agricultural income determination equation, a relation between agricultural and non-agricultural prices, a relation between short and long term interest rates, and a money market adjustment equation.

In total, the model contains 15 structural equations. Their specifications are indeed in most cases justified on a mix of both empirical and theoretical grounds. Yet, at times the justification is purely empirical as, for example, in the case of their proposed relation between short (average yield on short-term commercial paper, i_S) and long-term interest rates (average yield on corporate bonds, i_L) (Klein and Goldberger, 1955, pp. 28-29). The authors postulate a relation between short-term rates in year t and lagged long-term rates in $t - 3$ and $t - 5$:

$$(i_L)_t = \xi_0 + \xi_1(i_S)_{t-3} + \xi_2(i_S)_{t-5} + u_{14,t}. \quad (2.3)$$

From a theoretical point of view, it is fair to call this choice arbitrary, but as Klein and Goldberger state themselves their specification “does not necessarily imply that actual short rates are a function of past long rates” (p. 29). Nor does it imply that they are indeed a function of past long rates three and five, and not one, two or four, years ago. This choice is driven by an instrumentalist methodology as indicated by (Frisch, 2013). It simply provides a better fit than any other feasible specification to the data at hand.

Five auxiliary equations that take the form of definitions or accounting identities complement the system of 20 equations with 20 endogenous variables and 19 exogenous variables.³⁷ For

³⁷A comprehensive list of all variables and estimated equations of the Klein-Goldberger model can be found in Appendix A. It should be noted that for the actual empirical estimation of the model (Klein and Goldberger, 1955, ch. IV, pp. 42-70) several changes in the equations presented in the theoretical setup of the model (Klein and Goldberger, 1955, ch. III, pp. 4-29). For example, the average yield on corporate bonds i_L , as well as disposable

the first estimation of the model, Klein and Goldberger used annual data covering the periods of 1929-1941 and 1946-1950, summing up to only 18 observations, which is a major limitation of the study. In the revised version, which was published along with the original version in the 1955 monograph, observations for 1951 and 1952 have been added to the data set.

In order to estimate the coefficients the *limited information maximum likelihood method* as explained among others in Koopmans (1950) or Hood and Koopmans (1953) was applied. Instead of estimating the coefficients of all equations simultaneously, this approach allows for a separate estimation of structural equations or selected subgroups thereof. It thus strongly reduces the computational power required to solve the problem.³⁸

Applications and Conclusions

The estimation of model coefficients is one problem, which is easier to solve as advances in computational power are made. The application of a fitted model is another one. Klein and his collaborators were totally aware of the fact that a relatively good fit to the sample data does not provide any assurance for the direct applicability of the model to questions relevant for economic policy. One would first have to compare the model predictions for periods not used in the estimation itself to the actual outcomes observed in those periods in order to gain a sense of the robustness and reliability of the model. As Klein and Goldberger (1955, p. 72) put it:

The severest test of any theory is that of its ability to predict. Our equation system presents a theory of economic behavior in the aggregate. We have fitted the model to the sample, and although it may be an achievement to find a structural system which does fit the observed facts, we cannot be satisfied with the performance of the system solely with reference to the sample data.

And indeed the model was constantly adjusted and revised in light of newly gathered data. The published versions available in print were usually already outdated by the time they appeared on the market. Traditional macroeconomic modeling was very much an ongoing work in progress, as Klein and Goldberger (1955, p. vii) note in their introduction to the first published version of their model:

non-wage income plus depreciation of the current year, $(P - T_p + A_1 + A_2 - T_A + D)_t$, have been removed from the aggregate investment function as their impact was found insignificant, reducing the number of estimated coefficients to four.

³⁸For a brief explanation of the method, see Appendix A, page 338.

After having spent more than three years at work on the econometric model construction described in this book, we now find ourselves outdated by the basic revisions of the national income accounts made by the U.S. Department of Commerce in mid-summer 1954.

Nonetheless, the performance of the Klein-Goldberger model in *ex ante* forecasting was regarded as very strong, especially as compared to the alternative approaches, labeled “naive” small-scale models. Goldberger (1959) systematically studied the model’s predictions for the years 1953, 1954 and 1955 and came to the conclusion that it was better than any available alternative. It almost always predicted correctly the direction of change for any important macroeconomic variable. Interestingly, the main error in the forecasts for 1953 lay in overestimating price inflation, while forecasts for growth in real activity and employment were fairly accurate. The overall performance led to an enthusiastic use of large-scale models in policy simulations with the ultimate goal of informing political authorities about the relative costs and benefits of different public policy mixes.

Goldberger (1959) analyzed various multiplier effects within the model framework for fiscal policy measures, such as increases in government expenditure or increases in corporate and wage taxes. In order to investigate its cyclical properties and its capability of reproducing observed business cycles, Adelman and Adelman (1959) subjected the model to different kinds of shocks and simulated the model’s reaction functions. After having appropriately chosen the starting conditions from observed macro data, they introduced, among other things, random errors into each of the estimated equations that were drawn from a normal distribution with zero mean and variance corresponding to the size of the residuals observed in the sample. The model generated three to four year cyclical fluctuations in consumption and gross national product and their overall properties “appeared to be reasonably realistic” (Bodkin et al., 1991, p. 74). The average expansion and contraction phases in the post-war US cycles were 2.1 and 1.8 years, respectively. On average, the Klein-Goldberger model generated expansions of 2.6 and contractions of 1.5 years.³⁹

³⁹Adelman and Adelman (1959, pp. 614-616) conclude:

All in all, it would appear that there is a remarkable correspondence between the characteristics of fluctuations generated by the superposition of random shocks upon the Klein-Goldberger system and those of the business cycles which actually occur in the United States economy. The resemblance is not restricted to qualitative parallelism, but is, indeed, quantitative, in the sense that the duration of the cycle, the relative length of the expansion and contraction phases, and the degree of clustering of peaks and troughs are all in numerical agreement (within the accuracy of measurement) with empirical evidence

In terms of monetary policy analysis the conclusions were rather limited. However, some results suggested by the model were taken to be robust and highly relevant. Some nine years after its initial publication, namely in Klein and Bodkin (1964), one central conclusion has been drawn on the basis of the Klein-Goldberger model, namely, that price stability and high-level employment are conflicting policy goals (e.g. Bodkin et al., 1991, pp. 67-68). This conclusion might have been pushed partly by the popularization of the Phillips curve in the early 1960s, which will be discussed in more detail in section 2.3. It is of utmost importance to understand the perceived role of central banks and monetary policy. It suggests that there are real economic benefits to be gained in terms of economic activity, employment and growth if monetary policy authorities forsake the goal of price stability and are willing to go into the direction of more expansionary monetary policies.

In fact, a very similar relationship to the Phillips curve was already used in the initial formulation of the Klein-Goldberger model. The labour market adjustment relationship in equation A.8, which can be found in the model description in appendix A, encapsulates an inverse relationship between the unemployment rate and wage growth in nominal terms, as well as a wage-price lag, that is, price inflation of any given period positively affects wage growth one period ahead. In this sense, the model incorporated an element of wage rigidity.

The Klein-Goldberger model was only one in a series of models, sometimes geared towards more specific goals, sometimes larger, more general and detailed. For example, Valavanis-Vail (1955) developed a growth model for the US over the much larger period of 1869-1953. The model of Duesenberry et al. (1960) was specifically constructed for the analysis of economic crises. It tried to capture the structure of the US economy during recessions (Bodkin et al., 1991, pp. 78-80). However, it was with the construction of the Brookings model that major steps forward in large-scale macroeconometrics and policy analysis were made. We will discuss the results in the following.

2.2.3 The Brookings Model and Beyond

We have selected the Klein-Goldberger model for a more detailed exposition since it is comparatively small and much easier to handle in a summary like ours. As computing power and data gathering advanced at a rapid pace, the models grew larger, became less aggregated and more complex. The Brookings-S.S.R.C. model already contained more than 150 structural equations

and some 75 identities in its original formulation (Klein and Fromm, 1965, p. 348), as opposed to only 15 equations and 5 identities in the Klein-Goldberger model. The Brookings model has been presented to the public in multiple publications and as previous models was adjusted and revised regularly (Duesenberry et al., 1965; Fromm and Taubman, 1968; Duesenberry et al., 1969; Fromm, 1971; Fromm and Klein, 1975). It was also expanded quite considerably over time. Some versions of the model contained up to 400 equations, and so it is seen in retrospect as “truly the first large-scale macroeconomic model” (Bodkin et al., 1991, p. 88). It was highly regarded internationally as an enormous effort towards more reliable forecasting and comparative public policy analyses.⁴⁰

The Brookings model builders used seasonally adjusted quarterly data, instead of annual data, originally for the period of 1949 to 1960, for their estimations, which was regarded as a great advantage by the National Bureau of Economic Research, especially for the study of business cycle fluctuations and stabilization policies.⁴¹ They made a considerable effort in order to integrate the monetary and real spheres of the economy. Some 30 equations were specified to that end, including equations representing the demand for currency, demand and time deposits, the rate on time deposits, the term structure of interest rates, reserve holdings by commercial banks, household borrowing, and many more.

The choice of exogenous variables made the model predestined for policy simulations. Key tools of fiscal and monetary policy were integrated as exogenous factors into the numerous equations in order to render an evaluation of different policy mixes feasible. The model was

⁴⁰For example, Åberg (1967) suggested that the Brookings model, because of its sheer size and detail, and since it was built “by 25 of the most prominent economists in the U.S.” (p. 79) and hence benefited from their expertise in various subfields, has far surpassed earlier macroeconomic models. In fact, the author called the latter “a fantastic game in which an arbitrary theory in combination with rather inaccurate statistics could create practically any result.” (p. 79) Not so the Brookings model. Babeau (1966, p. 687) wrote the following lines in his discussion of the model in the *Revue économique*:

Si, malgré le nombre des équations, le modèle global est, grâce à l'utilisation de machines électroniques, aussi maniable que ses auteurs le disent, on peut penser que la politique économique américaine devrait trouver là un moyen commode de tester les effets des éventuelles mesures envisagées. En attendant les chercheurs français, pour leur propre compte, ne méditeront pas sans profit les solutions formalisées apportées par leurs collègues d'outre-Atlantique aux multiples problèmes de prévision abordés dans cette publication qui fera date.

⁴¹Two earlier examples of large-scale models that employed quarterly data were Klein (1964), a direct descendant of the Klein-Goldberger model, and Liu (1963). Both models were somewhat larger and less aggregated than the Klein-Goldberger model, but still much smaller than the Brookings model. While Klein (1964) reiterated the same labour market adjustment specification as the Klein-Goldberger model and thus had some relevance for monetary policy, Liu's model was directly used for simulations of the effects of different fiscal and monetary policies. He found that the implementation of a countercyclical interest rate policy would have a destabilizing effect, but added a caveat. According to him, the model was still too simplistic for any reliable policy advice. The Brookings model was more detailed and larger by orders of magnitude.

built precisely with the aim of informing and improving short-term stabilization policies (Bodkin et al., 1991, p. 96). The model performed well in *ex-post* forecasting over the period from 1961 to 1964 as shown in (Fromm and Taubman, 1968) and so the authors deemed it suitable for counterfactual policy simulations and used the model as a basis for evaluating the relative benefits and costs different policy measures.

Among other things, Fromm and Taubman (1968) simulated the effects of increases in government expenditures in different sectors of the economy, an increase in government employment, a decrease in the federal income tax, a decrease in the federal income tax in combination with expansionary monetary policies, and expansionary monetary policies in isolation. More specifically, they simulated a reduction of legal reserve requirements on demand deposits from 14.9% to 13.9%, and an increase of reserves of commercial banks through open market operations. Several conclusions were drawn in the study.

First, the monetary policy measures had the greatest impact on real investments, while the largest increase in real GNP resulted from increased government spending on construction and nondurables. Income tax cuts had the biggest impact on consumption. Second, all stimulative policies had a decreasing impact on the rate of price inflation, which Bodkin et al. (1991, p. 104) call “somewhat surprising”. Interestingly, as pointed out above, an error of the earlier Klein-Goldberger model was the overestimation of price inflation. With the benefit of hindsight we know today that the later models were severely underestimating price inflation rates in the 1970s. The third policy conclusion relevant for us was the following. Changes in monetary policy would generally have to be relatively substantial in order to have a notable impact, especially during economic downturns and recessions (Fromm and Taubman, 1968, p. 49).

Moreover, Fromm and Taubman (1968) engaged in welfare analysis in order to compare the different policies. They used three different utility functions (Bodkin et al., 1991, pp. 105-106):

- a linear utility function: $U_t = \sum_{i=1}^6 \beta_i X_i$,
- a Cobb-Douglas utility function: $U_t = \prod_{i=1}^6 X_i^{\beta_i}$, and
- a constant-elasticity-of-substitution (CES) utility function: $U_t = \sum_{i=1}^6 \beta_i X_i^\delta$,

where U_t is a utility index for period t , calculated from the six ($i = 1, 2, \dots, 6$) magnitudes deemed to be important for social welfare by the authors. They include personal consumption expenditure, government expenditure, government budget surplus, gross private domestic investment other than inventory investment, the reciprocal of the unemployment rate, and the reciprocal

of the GNP implicit price deflator. The authors used two different sets of weights. First, all magnitudes were weighted equally, $\beta_i = 1, \forall i \in [1, 6]$. Second, the expenditure magnitudes were weighted according to their average share in real GNP, the government budget surplus by 0.1, and the reciprocals of the unemployment rate and the GNP deflator by 0.5, respectively. The authors calculated a utility index over a time horizon, m , with different time preference rates, r ($r = 0.04, 0.06, 0.08, 0.10$), as follows:⁴²

$$U = \sum_{t=1}^m \frac{U_t}{(1+r)^t}.$$

Except for one specification of the CES utility function, the authors found that the monetary policy measures were welfare maximizing. Next came government expenditure policies. Federal income tax cuts performed poorest with respect to the utility index. The authors argued that part of the result is explained by the fact that government expenditures decrease the budget surplus or increase the deficit, while monetary policies do not.

We should mention at least a couple of the caveats that Fromm and Taubman (1968) added to their analysis, demonstrating awareness of some of the shortcomings. They pointed out that there might be important disruption costs due to discretionary monetary policies which might not be sufficiently taken into account by the model, and that there might actually be less uncertainty involved in fiscal policy as it could be directed to specific industries. Finally they emphasize some other limitations of their approach:

Aside from any questions of the validity and accuracy of the model, neither the multiplier nor the utility functions (even if augmented with additional arguments endogenous to the model) reflect the many other considerations which impinge on a policy choice. Such issues as the degree of government intervention, intergroup and interregional inequities, implications for long-run resource allocation and other social costs and benefits which are difficult to quantify must be taken into account. (Fromm and Taubman (1968, p. 123) as cited in Bodkin et al. (1991, p. 106))

The last point in the above quote seems to be of particular importance given the general methodological outlook of the approach. Indeed, the quantification of certain social costs and benefits constitutes a fundamental limit. Another, arguably even more fundamental one, would

⁴²For the CES utility function, δ was set equal to $-1, -0.5, 0.5$, and 2 , successively. The detailed results can be found in Fromm and Taubman (1968, pp. 119-122).

be the justification of any measure, index or weight given to these aspects, or to other actually quantifiable ones, for that matter, as well as their integration into some functional form that allows for the evaluation and comparison of overall social welfare.

Moreover, the difficulties in assessing the “implications for *long-run* resource allocation” should catch our attention. Since here is, regardless of all doubts concerning measurability and intersubjective utility comparisons, a limitation inherent in the positivistic approach advocated by Ragnar Frisch and his followers. It holds predictability and forecasting performance as central criteria for the validity or usefulness of a theory or a model.⁴³

Forecasting turns out to be an easier task for the short term. The further in the future the forecasting horizon, the larger become forecasting errors and uncertainty about the accuracy of the predictions. Following the approach of Adelman and Adelman (1959), Nagar (1969) analyzed the dynamic properties of the Brookings model and evaluated its capability of reproducing post-war US business fluctuations. He came to the conclusion that the model reproduced “cyclical behavior extremely well early in the simulation period and less well as the prediction period is lengthened. [...] In other words, better predictions of 1957-58 and 1960-61 [two mild post-war recessions] would have been observed if initial conditions closer to those dates had been selected” Nagar (1969, pp. 443-444).

We can draw an extremely important conclusion from this fundamental problem in forecasting the behavior of complex social systems. The short-run consequences of any given public policy measure tend to be evaluated relatively well following the instrumentalist-positivist approach, whereas long-run consequences are either neglected or remain much more uncertain. Quite naturally, since relatively robust forecasting successes will be achieved more frequently for the near future, following this approach generates a bias towards analyzing the short-term consequences. A scientist, who is primarily focused on predictive power, will tend to feel more comfortable about assessing the short-term rather than the long-term consequences.

Active maintenance of the Brookings model ended in 1972 (Bodkin et al., 1991, p. 108). However, it inspired many other large-scale models developed and used throughout the 1970s and 80s, such as the MIT-Penn-S.S.R.C. model under the leadership of professors Albert Ando from the University of Pennsylvania and Franco Modigliani from MIT, or the Wharton model initiated by the Wharton Econometric Forecasting Associates, Inc, an organization founded by

⁴³It was in fact Friedman (1953b) who has provided the most influential formulation of this methodological stance. Although, he was in disagreement on some points with the proponents of large-scale macroeconomic modeling, the underlying methodological basis was in that respect identical. We will discuss Friedman’s seminal essay in some detail in section 3.1 in the next chapter.

Lawrence Klein at the University of Pennsylvania in 1969. Over the same period the large-scale macroeconomic approach came increasingly under criticism, not least for a rather poor forecasting performance of price inflation rates during the stagflation.

Early prominent critics were Monetarists like Karl Brunner⁴⁴ and New Classical economists like Robert Lucas.⁴⁵ Besides some very important methodological considerations, one of the central building blocks of the Keynesian large-scale models that reflects a substantial part of the repercussions of monetary changes on the real economy, namely, the Phillips curve, has been attacked specifically, at least in its naive interpretation. Before we enter into these criticisms, we will review its early history in the last section of this chapter.

2.3 Political Implications: The Early History of the Phillips Curve

In section 2.1.2, we have seen that Keynes provided a rationale for the Phillips curve in the *General Theory* two decades before it gained its popularity. It is therefore not surprising that the Keynesian macroeconometricians of the post World War II era immediately incorporated Phillip's empirical work into their models, in particular the Brookings and Wharton models. As pointed out above, the Klein-Goldberger model incorporated a similar Phillips-type relationship even before the Phillips curve was a common term in macroeconomics. Yet, it is indeed surprising that Irving Fisher's brief theoretical and empirical investigation of the link between inflation and unemployment published ten years prior to the *General Theory*, caught so little attention (Fisher, 1973, [1926]). As a conjectural explanation one might take this historical fact as evidence for the success of Keynesian macroeconomics, which was by the end of the 1950s sufficiently widespread so that Phillip's paper got a warmer reception. Not only was the finding consistent with Keynesian theory, the mere empirical result that it contained had also higher weight from the perspective of positivistic econometrics. Back in the 1920s, before the foundation of the Econometric Society another methodological approach might still have been dominant.

⁴⁴In the preface to an edited volume, Brunner (1972, p. iv) wrote that "econometric practice has evolved into numerology analogous in some respects to astrology." In the same volume Basman (1972a,b) criticized the Brookings model for not delivering sufficiently sharp empirical hypotheses that could be tested in practice. He also asked provocatively whether the Brookings model was "number mysticism?"

⁴⁵We will discuss his famous Lucas critique in section 4.1 in chapter 4 on the New Classical contributions to our topic.

2.3.1 Fisher Discovered the Phillips Curve

Fisher analyzed data from the United States at the beginning of the 20th century and went even so far as to postulate a causal relationship:

But as the economic analysis already cited certainly indicates a causal relationship between inflation and employment or deflation and unemployment, it seems reasonable to conclude that what the charts show is largely, if not mostly, a genuine and straightforward causal relationship; that the ups and downs of employment are the effects, in large measure, of the rises and falls of prices, due in turn to the inflation and deflation of money and credit. (Fisher, 1973, [1926], p. 502)

Notice that Fisher still used the terms inflation and deflation in the traditional sense, meaning expansion and contraction of the supply of money and credit, respectively (von Mises, 1990b, p. 115), instead of mere increases or decreases of some price index, which would be the commonly accepted definition today (Salerno, 2010, p. 424). Although it might be considered a useful shortcut, as price inflation has become the primary monetary policy target and monetary inflation, if only large enough, ultimately leads to price inflation, this shift in the definition amounts to a conflation of the aim sought and the means applied. Fisher proposed inflation and deflation of the money supply as the means to counterbalance decreases or increases in the price level, and hence to attain a stable purchasing power. In order to avoid semantic confusion, throughout the thesis rises and falls in the price level will be referred to as price inflation and price deflation, respectively.

Maybe Fisher's work was just published too early for the great success. Keynes too had some relationship of this kind in mind when he formulated his policy recommendations in an open letter to president Roosevelt in response to the Great Depression in the 1930s, although he also resolutely emphasized the complementary role of fiscal policy in order to stabilize and improve macroeconomic conditions (Keynes, 1933). Yet, Fisher's article remained widely unrecognized, and the relationship was not named after him, but more than 30 years later after Phillips.

2.3.2 The Popularization of a Rushed Job

In 1958, Alban W. Phillips (1914-1975) of the London School of Economics published an empirical study on the relationship between the rate at which nominal wages change and the rate of unemployment for the United Kingdom from 1861 to 1957. The statistical evidence collected

in his study suggests an inverse relationship, that is, unemployment tended to be relatively low during periods in which wages rose quickly. Phillips did not draw any political conclusions from his finding and merely hinted at an unemployment-price inflation relationship. By subtracting the long-term productivity growth from the rate of change of nominal wages, which is assumed to correspond to the rate of price inflation, Phillips (1958, p. 299) concluded that for “a stable level of product prices the associated level of unemployment would be a little under $2\frac{1}{2}$ per cent.”

It is interesting to note that Phillips himself might not have been convinced of the quality of his paper and did not intend to publish it in this form. It was James Meade, a colleague at LSE, who pushed for the publication (Sleeman, 2011). In fact, Phillips often commented on his paper dismissively, as “a very crude attempt”, a “quick and dirty job”, a work “just done in a weekend”, and as a “rushed job” (Sleeman, 2011, p. 233). Phillips also refused to comment on later papers on the topic or write a follow-up paper himself. There is evidence that Phillips was reluctant to publish his famous 1958 article.

However, only two years later Samuelson and Solow replaced the rate of change of money wages by the rate of price inflation with lasting impact (Samuelson and Solow, 1960). They popularized the empirical finding and explored its political implications. Assuming a causal relationship, just like Fisher did back in the 1920s, they argued that expansionary monetary policy would lead to lower unemployment rates. With their contribution the idea and the term of the Phillips curve was popularized and encouraged a lively intellectual debate. Gordon (2011, p. 13) describes its immense influence as follows:

So widely read and discussed was the Samuelson-Solow article that the term “PC” [Phillips curve] entered the language of macroeconomics almost immediately and soon became a lynchpin of the large-scale macroeconometric models which were the focus of research activity in the 1960s.

Samuelson and Solow investigated data for the U.S. from the turn of the century to the 1950s and found that the relationship did not hold during the two world wars and the Great Depression in the 1930s. During the three remaining periods, namely, before World War I, from the end of World War I until the end of the 1920s, and after World War II, they identified an empirical relationship between price inflation and unemployment that very much resembles Phillips’ results. In addition, they point to the possibility of a shift of the Phillips curve:

What is most interesting is the strong suggestion that the relation, such as it is, has shifted upward slightly but noticeably in the forties and fifties. On the one hand, the first decade of the century and the twenties seem to fit the same pattern. [...] [W]age increases equal to the productivity increase of 2 to 3 per cent per year is the normal pattern at about 3 per cent unemployment. This is not so terribly different from Phillips' results for the U.K. [...] On the other hand, from 1946 to the present [1960] [...] it would take more like 8 per cent unemployment to keep money wages from rising. And they would rise at 2 to 3 per cent per year with 5 or 6 per cent of the labor force unemployed. (Samuelson and Solow, 1960, p. 189)

The authors assume a long-run productivity growth of 2 to 3 per cent. Hence, under the further assumption that the rate of price inflation corresponds to the rate of change of nominal wages minus productivity growth, we would have stable prices if wages rise at 2 to 3 per cent.⁴⁶ For the analysis of Samuelson and Solow, this means that prior to World War I prices had been stable at 3 per cent unemployment. After World War II the zero inflation unemployment rate had risen to 5 to 6 per cent.

Samuelson and Solow (1960, p. 193) represent the Phillips curve, whatever its position may be, as a “menu of choice”, suggesting a trade-off that could be exploited by political authorities. The position of the curve in turn is determined by the institutional environment, that is, factors like the power of trade and labor unions, or labor laws. Subsequently, this alleged trade-off has found its way into political debates in various countries. In the case of the United States, for example,

[t]he policy advisors of the Kennedy and Johnson administrations, led by Walter Heller with support roles by Robert Solow and James Tobin, argued that the previous Republican administration had chosen a point too far south-east [high unemployment and low inflation] along the PC trade-off, and that it was time to 'get the country moving again' by moving to the north-west [low unemployment and high inflation]. (Gordon, 2011, pp. 15-16)

In 1972, German “Superminister” - minister of economic affairs, finance and defense - Helmut Schmidt of the Social Democratic Party famously stated that he would rather have 5

⁴⁶In general, as described in Blanchard and Fischer (1993, pp. 542-543), in order to arrive at an unemployment-price inflation relationship, a markup equation that connects price and wage developments is needed as an intermediate step. More precisely, prices are assumed to be a markup over unit labor costs, which are defined by wage rates and labor productivity. In this context, see also Tobin (1972a) to whom the authors refer.

percent price inflation than 5 percent unemployment. According to former state secretary Otto Schlecht, Schmidt knew full well that this statement was technically false, but he thought it was politically necessary.⁴⁷ It is no surprise that he was aware of the technical falsehood, since research conducted after Samuelson and Solow (1960) has shown that the relationship between unemployment and inflation is far from being a mechanistic and stable trade-off.

These contributions are most notably linked to the Monetarist School, which will be discussed in the following chapter, and the New Classical School, particularly their important methodological critique. This will be the subject of chapter 4.

2.3.3 Implications

Much has been written on the Phillips curve and a lot of it may be exaggerated or simply false. Forder (2014) has presented a thorough reconsideration of the impact of Phillips's work on the broader discipline of macroeconomics, arguing that it has been vastly overstated in the literature on the history of modern macroeconomic thought. He calls the success story of the Phillips curve a myth. Indeed, there have been authors who gave the Phillips curve a rather hostile reception. Some even ridiculed it.⁴⁸

One historical fact that Forder presents in order to downgrade its influence is that large-scale macroeconomic models, such as the Klein-Goldberger model, have incorporated Phillips-type relationships already before the publication of the famous paper in 1958. That is certainly true and it even strengthens our claim here.

The point is that what is meant with the term Phillips curve is usually much more than Phillips's curve as such. It has become a short-cut for designating empirical regularities and relationships between changes in monetary quantities and real quantities, most notably unemployment and in more recent times aggregate output. It also covers formal theoretical advances intended to illuminate the relationship between monetary and real magnitudes.

Understood in such a broad sense, the Phillips curve is indeed what transports the bulk of the implications for the analysis of the costs and benefits of active monetary policy and central bank-

⁴⁷In response to a critical remark by Schlecht, Schmidt said: "Daß dies fachlich falsch ist, weiß ich selbst. Aber Sie können mir nicht raten, was ich auf einer Wahlveranstaltung vor zehntausend Ruhrkumpeln in der Dortmunder Westfalenhalle zu sagen für politisch zweckmäßig halte." (Schlecht, 1996) [That it is false I know full well, but you can't tell me what I should say in front of ten thousand laborers in Dortmund on an election rally, if I deem something else politically necessary. (own translation)]

⁴⁸The British economist Guy Routh, for example, suggested that Phillips's plot resembled an ostrich (Routh, 1986). According to Forder, Routh was the first to use the term "Phillips curve" in written form (Routh, 1959) and he was entirely mocking and rejecting the work. In Forder's account, this was not an uncommon position among economists at the time.

ing. The aggregated macroeconomic models of the day suggested that expansionary monetary policies could stimulate real economic activity and employment based on the available historical data of the time. These models suggested politically exploitable Phillips-type relationships, not necessarily in only one simple curve or equation, but in systems of estimated equations.

The costs of monetary policy from that perspective are the real economic slowdowns and downturns that could have been effectively countered, if not completely prevented, had an active and more expansionary central bank policy been implemented at the right point in time. This was first suggested in an econometric study by Tinbergen (1939a,b) and later elaborated further in the series of large-scale models published in the 1950s, 60s and 70s under the immense influence of Lawrence Klein.

These substantive developments in modern macroeconomics are independent of the story that is told about Phillips's paper and its influence, which might well be false on a number of counts. In fact, they are completely independent of how the paper was received and handled by the profession. As De Vroey (2016a) has emphasized, there is a difference between the Phillips curve story and the Phillips curve theory. The latter is what we might call the Phillips curve in the broader sense. And it is what we have in mind here.

The next important contribution in that regard has been delivered by Milton Friedman. Before we go into his substantive contributions to the topic, however, we introduce the Monetarist approach in order to highlight its methodological kinship to the Keynesian analysis presented above.

Chapter 3

Monetarism

The subject of this chapter is the Monetarist approach to monetary theory and policy. With respect to the underlying views on economics as a science, “in the strict sense,” it is similar to the Keynesian approach (again, not necessarily the one of Keynes himself) which we dealt with in the previous chapter. Both share a distinctly positivistic methodology. There are, however, also undeniable and important differences in their methods that have to be emphasized.

In section 3.1 we focus on one of the most important writings in the methodology of economics in the 20th century, Milton Friedman’s (1912-2006) “The Methodology of Positive Economics” (Friedman, 1953b). Friedman, Nobel laureate in 1976, elaborated on Frisch’s view, although without reference, that economic theories or models are to be judged in light of their predictive power. This view has become dominant to this day.

From the methodological foundations we move on to the actual theories and modeling attempts of the Monetarists in section 3.2. We draw again heavily on the works of Friedman himself as well as some of his collaborators.¹ Two important differences to the Keynesian approach should become apparent. First, whereas we have labeled large-scale Keynesian models as “Walrasian” in a loose sense because they look at the economy as a whole, the Monetarist contributions were more in line with the Marshallian idea of breaking up the complex problems of economics into smaller sub-problems. Second, Monetarist models are, in a sense, less formal. At times they lack an explicit mathematical-quantitative formulation.

In section 3.3 the historical sketch of the Phillips curve that closed the previous chapter is

¹It was Brunner (1968) who came up with the term Monetarism. It should however be mentioned that there is not one homogeneous doctrine of Monetarism, and we therefore do not claim to present one here - or rather, we do not claim to present the only doctrine of Monetarism. As Mayer (1978, p. i) points out, Monetarism “has no *General Theory*.” Our analysis is thus selective and will focus primarily on the contributions of Milton Friedman, who is widely regarded as the founder and most important representative of Monetarism.

continued. One of the most important theoretical contributions of the Monetarist school was the natural-rate hypothesis that suggested an alternative interpretation of the Phillips curve based on *adaptive expectations*. From the expectations-augmented Phillips curve emerged the prevailing view that the trade-off between price inflation and unemployment is a short-run phenomenon which vanishes in the long run. We will also look at some of the empirical literature that attempted to test the natural-rate hypothesis.

3.1 The Methodology of Positive Economics

Friedman's seminal article on "The Methodology of Positive Economics" has been called the "the most influential work on economic methodology of the twentieth century" (Hausman, 2008, p. 33).² It picked up and strengthened the central claim of the early modern econometricians that predictive power is the decisive quality criterion for theoretical propositions and models, and turned it into the predominant methodological view in the field of economics. In so doing, it shifted the focus further away from the realism³ of underlying assumptions.

We will discuss Friedman's essay and some related issues in three subsections. First, the underlying distinction of economics into a positive and a normative science, and the particular importance of the former are explained. Next, positive economics and its elements are looked at more closely. Finally, we present Friedman's justification for his instrumentalist methodology.

²Mäki (2009a, p. 47) wrote: "There is no doubt that this short essay of forty pages became the most cited, the most influential, and the most controversial piece of methodological writing in twentieth-century economics."

There have of course been important influences on Friedman's methodological views. Hirsch and De Marchi (1990) argued that Wesley C. Mitchell had an impact on him when he worked at the National Bureau of Economic Research. They also argued that there are similarities to the pragmatism of John Dewey (cf. also Mäki, 2009a, p. 56). De Marchi (1988, p. 7) pointed out that Friedman was obviously influenced by Karl Popper when he linked positive economics "to the Popperian ideal of science as conjecture and refutation" although he was not a Popperian in his view. Indeed, Friedman himself met Karl Popper at the very first meeting of the Mont Pèlerin Society in 1947 as he recalled in an interview (Levy, 1992). They had "some long discussions" about methodology in the natural and social sciences that "played a not negligible role" for his later essay. In another interview, when asked about his famous essay on methodology, Friedman said that he "found out that [his] views were very similar to Karl Popper's" and that he "followed his writings in a sort of a vague way, but not very seriously" (Snowdon and Vane, 2005, p. 210). Popper himself apparently did not think too highly of Friedman's methodological ideas. According to a personal anecdote by his long-time research assistant, Jeremy Shearmur, Popper was in the audience for a speech on methodology by Friedman at Stanford. Popper said to Shearmur: "He's not very good, is he?" It was Dr. David Gordon, senior fellow of the Mises Institute in Auburn, AL, a friend of Shearmur, who told this anecdote in private correspondence.

³The term "realisticness" has been suggested by Mäki (1989) instead of realism to avoid confusions. The term "realisticness" would refer to a set of characteristics of theories and their assumptions and the term "realism" to a family of philosophical doctrines. In our discussion of Friedman's essay we will stick to the word "realism" as it was the term that the author used himself. In section 6.1 we will discuss realism as a philosophical foundation for economics.

3.1.1 The Distinction between Normative and Positive Economics

By reference to John Neville Keynes's work on economic methodology, *The Scope and Method of Political Economy*, that makes a threefold distinction of economics into a *positive science*, a *normative science*, and an *art*, Friedman (1953b, p. 3) emphasized the particular importance of economics as a positive science. The positive science of economics as a "body of systematized knowledge about what is" may be seen as the very foundation for the art of economics, that is, the development of policy rules to attain certain ends derived from the normative science of economics.⁴

As Friedman (1953b, p. 4) pointed out, "[p]ositive economics is in principle independent of any particular ethical position or normative judgments." According to him it tries to achieve a "system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances." On the other hand, any policy conclusion, say, advocacy of the implementation of a certain regulatory law to attain a specified end, relies on these generalizations and cannot be drawn independently. It is possible, however, that two individuals who agree on the consequences of such a regulatory law, disagree on their desirability. One advocates the law, the other does not. Settlement of this kind of disagreement, if at all possible, would fall under the scope of the separate normative science of economics.

Interestingly, Friedman held that the differences in opinion about the desirability of the undisputed consequences of certain political measures, that is "fundamental differences in basic values," are "differences about which men can ultimately only fight" (p. 5). Indeed, this view of the subjectivity of value judgments, and that they have to be separated from objective scientific inquiry, is the dominant one in the modern social sciences.⁵ Yet, it begs the question in what way the normative aspects of economics can then be dealt with scientifically at all. Under the premise of subjectivity there seems to be very little room for a *normative science* of economics.⁶

⁴For the sake of clarity, it is worthwhile quoting the relevant passage in Neville Keynes's book here:

As the terms are here used, a positive science may be defined as a body of systematized knowledge concerning what is; [...] a *normative* or *regulative* science as a body of systematized knowledge relating to criteria of what ought to be, and concerned therefore with the ideal as distinguished from the actual; [...] an art as a system of rules for the attainment of a given end. [...] The object of a positive science is the establishment of uniformities, of a normative science the determination of ideals, of an art the formulation of *precepts*. (Keynes, 1890, p. 22)

⁵This is so at least since the influential work of Max Weber on the philosophy of sciences. See for example his essays *Die 'Objektivität' sozialwissenschaftlicher und sozialpolitischer Erkenntnis* and *Der Sinn der "Wertfreiheit" der soziologischen und ökonomischen Wissenschaften* in Weber (1922).

⁶One might say that the very idea of the subjective nature of value judgments is the one and only objective scientific insight from the normative realm - the be-all and end-all of the normative science of economics, so to

Friedman believed that most disagreements on economic policy stem from diverging views on what the actual consequences of certain political measures would be:

I venture the judgment, however, that currently in the Western world, and especially in the United States, differences about economic policy among disinterested citizens derive predominantly from different predictions about the economic consequences of taking action – differences that in principle can be eliminated by the progress of positive economics [...]. (p. 5)

On the question of what kind of consequences are desirable or not, there seems to be widespread agreement in his opinion. And even if there was disagreement, one could “only fight” over it. Naturally then, his emphasis lay on *positive economics* that has to be developed and improved in order to settle disagreements. In fact, Friedman pointed out that his judgment about the major source of disagreement is also only hypothetical and needs “to be accepted or rejected on the basis of empirical evidence” like every non-tautological “positive” statement.

If this judgment is valid, it means that a consensus on “correct” economic policy depends much less on the progress of normative economics proper than on the progress of a positive economics yielding conclusions that are, and deserve to be, widely accepted. It means also that a major reason for distinguishing positive economics sharply from normative economics is precisely the contribution that can thereby be made to agreement about policy. (pp. 6-7)

He mentioned the example of minimum wage legislation and argued that there is underlying agreement on the policy goal, namely achieving a “living wage” for everybody. Very few people would deny the desirability of such an outcome. There is, however, disagreement on whether the measure actually serves the purpose. It could be solved by positive economics.⁷

speaking. But then we must recognize that this claim is of the same kind as the rest of scientific economics, namely positive. The distinction between positive and normative science would then not be very helpful. This, of course, is not an indication of a fundamental problem in Friedman’s system, but rather a hint at a slight misuse of terms. There would not really be a *normative science* of economics, only normative judgments that are occasionally incorporated into an otherwise positive science of economics.

⁷An interesting observation can be made in Friedman’s brief summary of the position of minimum wage law proponents:

Proponents believe (predict) that legal minimum wages diminish poverty by raising the wages of those receiving less than the minimum wage as well as of some receiving more than the minimum wage without any counterbalancing increase in the number of people entirely unemployed or employed less advantageously than they otherwise would be.

3.1.2 Positive Economics

Positive economics tells us something about the real world *as it is*, or ideally and more precisely, in Friedman's view, about the real world *as it will be*. Predictive power is seen as the cornerstone of all progress in positive economics, which essentially splits into two elements:

The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed. Such a theory is, in general, a complex intermixture of two elements. In part, it is a "language" designed to promote "systematic and organized methods of reasoning." [...] ^[8] In part, it is a body of substantive hypotheses designed to abstract essential features of complex reality. (Friedman, 1953b, p. 7)

Friedman held that economic theory seen as a *language* is merely a set of conventions and tautologies and has no "substantive content." It is a tool for the systematization and classification of empirical facts, that is a "filing system." The usefulness of such a system is judged "partly on logical, partly on factual, considerations." (p. 7) It is important that categories and definitions are sufficiently clear, exhaustive, and consistent. They should avoid ambiguities. The various empirical factors and phenomena that are of interest should ideally be biuniquely assignable to the categories and terms of the filing system. In other words, the categories and terms should have clear and "meaningful empirical counterparts." The set-up of the filing system itself is therefore guided by what is empirically observed.

The elements of the filing system can be combined in a purely definitional or tautological way. But Friedman emphasized that "economic theory must be more than a structure of tautologies if it is to be able to predict and not merely describe the consequences of action; if it is to be something different from disguised mathematics." (pp. 11-12) Hence, the elements of the filing system or the language of positive economics must be used in a way that hypotheses are formulated that relate to the observable external world in the future, for example the path or state of a certain

Obviously this statement involves a comparison between a state that *is* and a state that otherwise *could have been*, that is, a counterfactual state. But only the facts and not the counterfactuals can be investigated empirically. It is rather unclear how Friedman's positivistic methodology outlined below could, even in principle, cope with these kinds of claims, and hence solve the disagreement. This is a problem that we have already briefly mentioned in the previous chapter. It will be discussed in more detail in the second part of the thesis.

Moreover, we can see that this statement at the very least involves one value judgment which is implied in the use of the words "without any counterbalancing increase." The question is of course what qualifies as a counterbalancing increase and what does not? The example is therefore rather ironic as an illustration of a clear cut between positive and normative economics.

⁸Friedman quotes from Alfred Marshall's *The Present Position of Economics* (Marshall, 1885, p. 37), who referred to the "economic organon" that "introduces systematic and organized methods of reasoning."

observable economic variable. These hypotheses are formulated in accordance with empirical evidence of the past, and are rejected, accepted or refined on the basis of empirical evidence gathered in the future. As Friedman pointed out, “they must be well enough defined so that observation can show them to be wrong” (p. 13). In other words, they must be falsifiable. As long as they are supported by the empirical evidence and are not falsified they remain part of the tentative body of positive economics.

In 2000, Friedman described this process vividly in a television interview, in which he commented on his book *A Theory of the Consumption Function* (Friedman, 1957). He himself considered it to be his best book in “technical”, or let us say positive, economics:

[The book] is a nice complete whole. It started with an empirical contradiction - data that weren't consistent with one another. It presented a hypothesis [the permanent income hypothesis] to explain the contradiction. Out of that hypothesis it drew implications capable of being contradicted by further evidence. It analyzed the further evidence that there was and found that it was consistent with the hypothesis. And the hypothesis is by now part of standard economics. (Friedman, 2000)

Friedman acknowledged that there are certain problems involved in the process of hypotheses building and testing that are of great importance in the social sciences. In particular, the observations against which a model or hypothesis is tested are almost always uncontrolled, which means that there is a multitude of variables that potentially play a role but cannot be held constant. As a consequence it is “difficult to produce dramatic and clear-cut evidence to justify the acceptance of tentative hypotheses.” (Friedman, 1953b, p. 40) Yet, he continued:

Reliance on uncontrolled experience does not affect the fundamental methodological principle that a hypothesis can be tested only by the conformity of its implications or predictions with observable phenomena; but it does render the task of testing hypotheses more difficult and gives greater scope for confusion about the methodological principles involved. More than other scientists, social scientists need to be self-conscious about their methodology. (p. 40)

Friedman also acknowledged the fact that in economics, which deals with the “interrelations of human beings, [...] the investigator is himself part of the subject matter being investigated” (p. 4), but denies that this constitutes an important distinction to the natural sciences. Although it

renders objectivity of evidence even harder to obtain, for Friedman, the fundamental methodological principle is the same both in economics and the natural sciences. It is the conformity of a positive theory's predictions to observable phenomena and empirical evidence that is important, not the realism or "realisticness" (Mäki, 1989) of underlying assumptions and premises.

3.1.3 Realism of Assumptions versus Predictive Power

Friedman completely rejected the idea that the realism of underlying assumptions of a theory is relevant or even vitally important - an idea that had been held dear among others by John Maynard Keynes who criticized the "accepted classical theory of economics" precisely for "its tacit assumptions [that] are seldom or never satisfied, with the result that it cannot solve the economic problems of the actual world" (Keynes, 1936, p. 378; see also Crotty, 2011). In Friedman's eyes, there is no meaningful test of assumptions that does not end up being a test of the implications of the assumptions, and he even claimed that

[t]his widely held view [Keynes's view] is fundamentally wrong and productive of much mischief. [...] [It] produces a misdirection of much intellectual effort devoted to the development of positive economics, and impedes the attainment of consensus on tentative hypotheses in positive economics. (Friedman, 1953b, p. 14)

Friedman claimed that particularly significant hypotheses and theories tend to have assumptions that are "wildly inaccurate descriptive representations of reality," and as a general rule of thumb "the more significant a theory, the more unrealistic the assumptions" (p. 14). Why is that? In answering this question Friedman was running in circles by just reiterating that predictive power is the most important attribute to an economic hypothesis or theory, and that in order to attain predictive power, realistic assumptions are not necessary. In fact, the criterion of predictive power in combination with the principle of Occam's razor demands that the economist goes for the unrealistic, but simpler assumptions. Hence, the answer that Friedman provided is rather self-confirming. It simply lies in his own judgment about what makes a theory *significant*.

It is interesting to note that his judgment must be considered normative. It clearly is no positive scientific statement to say that a theory ought to have predictive power. In a sense Friedman put the cart before the horse. At first it must be clarified whether scientific prediction is at all possible in economics, and if so in which way? Otherwise he might be chasing an

unobtainable target, which could justly be considered a “misdirection of much intellectual effort.” However, these questions would have to be dealt with in a completely different way.⁹

A theory, according to Friedman, is significant when it is simple and capable of making relatively accurate predictions. It is simple when it is based on as little information as possible. It therefore has to be selective and must abstract from complex reality. He claimed that “to be important, [...] a theory must be descriptively false in its assumptions,” or put differently, a theory must always be understood as an “engine” to analyze the real world, but not as a “photographic reproduction” thereof (Friedman, 1953b, p. 35).¹⁰ It is merely a tool or instrument that the economist employs and does not in itself claim to be true. Hence,

the relevant question to ask about the “assumptions” of a theory is not whether they are descriptively “realistic,” for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently

⁹The causal-realist approach to economics explored in the second part of this thesis can shed some light on these preliminary problems. See in particular chapter 6.

¹⁰Here, once more, Friedman makes a reference to the work of Alfred Marshall, in which we can indeed find precursory elements of Friedman’s view. For example, in his *Principles*, Marshall presents a monistic interpretation according to which both natural and social sciences follow the same fundamental principle, the only essential difference being their degree of exactness. He presented the fundamental principle as follows:

Those physical sciences, which have progressed most beyond the points to which they were brought by the brilliant genius of the Greeks, are not all of them strictly speaking “exact sciences.” But they all aim at exactness. That is they all aim at precipitating the result of a multitude of observations into provisional statements, which are sufficiently definite to be brought under test by other observations of nature. These statements, when first put forth, seldom claim a high authority. But after they have been tested by many independent observations, and especially after they have been applied successfully in the prediction of coming events, or of the results of new experiments, they graduate as laws. A science progresses by increasing the number and exactness of its laws; by submitting them to tests of ever increasing severity; and by enlarging their scope till a single broad law contains and supersedes a number of narrower laws, which have been shown to be special instances of it. (Marshall, 1890, pp. 23-24)

Marshall then acknowledged that the social sciences are less “exact” than the natural sciences, just like some elements within the natural sciences are less exact than others. The difference between social and natural sciences therefore is in degree and not in kind. He contrasted economics to the relatively simple theory of gravitation in astronomy and compared it to the more complicated and less accurate theory of the tides:

Now there are no economic tendencies which act as steadily and can be measured as exactly as gravitation can: and consequently there are no laws of economics which can be compared for precision with the law of gravitation. (p. 24) [...] The laws of economics are to be compared with the laws of the tides, rather than with the simple and exact law of gravitation. (p. 25)

Consequently, Marshall held that in economics the “term ‘law’ means [...] nothing more than a general proposition or statement of tendencies, more or less certain, more or less definite.” He thus, slightly more pronounced than Friedman, advised caution: “Many such statements are made in every science: but we do not, indeed we can not, give to all of them a formal character and name them as laws.” (p. 25) But both see no fundamental difference to the natural sciences.

accurate predictions. The two supposedly independent tests thus reduce to one test.
(p. 15)

Friedman was not perfectly clear on the distinction between “descriptively false” and “incomplete” assumptions. Presumably, a set of assumptions could be incomplete or selective in that it does not cover all aspects and variables that are related to the phenomena that are studied, but can still be “true” or “realistic” in what it states about the aspects and variables that are taken account of. Friedman used the terms interchangeably suggesting that neither should be cause for concern, or rather, that a significant theory will always be both anyway, descriptively false and selective. Especially with his emphasis on the latter, that is, his emphasis on partial analysis, he is following Alfred Marshall.

Marshall explained the breaking up of complex problems into component parts as a dictate of common sense in the *The Present Position of Economics*:

The only resources we have for dealing with social problems as a whole lie in the judgment of common sense. For the present, and for a long time to come, that must be the final arbiter. Economic theory does not claim to displace it from its supreme authority, nor to interfere with the manner nor even the order of its work, but only to assist it in one part of its work. For common sense does not deal with a complex problem as a whole. Its first step is to break the problem up into its several parts; it then discusses one set of considerations after another, and finally it sums up and gives its conclusions. The fact which Comte seems to have ignored is that the human mind has no other method of inquiry than this; that a complex problem is broken up into its component parts, less methodically indeed but no less completely by common sense than by formal analysis. When it is thus broken up each separate part offers a foot-hold to treatment by a special scientific organon, if there be one ready.
(Marshall, 1885, pp. 35-36)

And within every component part the guiding criterion for the development of the “organon” is predictive power.

Friedman provided a couple of examples to illustrate his point. First, he refers to the acceleration of a body in free fall in a vacuum under a uniform gravitational field. The hypothesis that the object would accelerate at a constant factor g is generally accepted. An object dropped in a vacuum near the surface of the earth for instance is supposed to accelerate at $g = 9.81m/s^2$

(or $32.2ft/s^2$). Given that the object starts from rest, that is with an initial velocity of zero, one obtains the following formula for the distance traveled after t seconds: $\frac{1}{2}gt^2$. Friedman stated that if one were to apply this hypothesis or theory to “a compact ball dropped from a building”, this would be “equivalent to saying that a ball so dropped behaves *as if* it were falling in a vacuum” (p. 16).

Trying to test this hypothesis by looking at whether the actual air pressure is “sufficiently close” to zero as in a vacuum is pointless according to Friedman as there is no way of determining what “sufficiently close” means without reference to the actual accuracy of the prediction and the desired accuracy of the inquirer. This is correct as far as it goes, but “testing assumptions” is in this case a rather irrelevant notion anyway. In Friedman’s presentation of this particular example, a somewhat artificial possibility of testing the assumption of a vacuum is created. Choosing a working definition of vacuum, let us say some state of close to zero air pressure, it becomes possible to check whether a vacuum is actually there or not - and of course it is not - but put differently, the assumption is simply that the compact ball accelerates at $9.81m/s^2$. How would one test this assumption in itself?

We would somehow have to know the correct acceleration factor already in order to do that. But then there would be no need for scientific inquiry in the first place. Hence, as Friedman elaborates at length there is really no way of testing this kind of assumption without actually looking at the derived prediction and comparing it to the actual observations. And this is certainly so in most cases where the assumptions of a theory directly imply exact observable outcomes.

However, Friedman did not show any recognition of the fact that there is at least one other assumption, in a way more fundamental, that underlies his methodology and that has to be evaluated differently as it ultimately eludes the realm of observable phenomena. It is assumed that under the same and idealized conditions the same constant acceleration factor always holds, which of course is not very controversial in this particular case.¹¹

¹¹In the second part of this thesis, we will see that whether or not exact observable outcomes can in principle be predicted depends on whether or not the *constancy principle* with respect to the observed phenomena holds, which is in itself an assumption implicitly underlying but not testable within Friedman’s methodology (see section 6.2.2, pp. 224ff.). Moreover, we will see that, within the causal-realist approach to economics, theories are formulated based on the assumption of human action, that is the explicit rejection of empirical constancy between variables that are contingent on action. It will be argued that the weight and significance of such theories must be evaluated by the plausibility and logical justification of their underlying core assumption of rejecting constancy, as well as the empirical correctness of certain auxiliary assumptions in specific cases where these theories are applied. Testing assumptions broadly conceived becomes meaningful here.

Musgrave (1981) made a similar point arguing that the realism of at least some assumptions is important and must be evaluated independently from prediction, although his classification into negligibility, domain and heuristic assumptions is not very useful for us. Another influential critique can be found in Hausman (1992, ch. 9). For a brief discussion of both see (Hands, 2001b, pp. 57-60).

Similar issues arise for Friedman's other examples. He went on to discuss a slightly more complex problem from the natural sciences, namely the density of leaves on a tree, an example also used in (Alchian, 1950). Friedman argued that it would practically make no difference whether one assumed that "each leaf deliberately sought to maximize the amount of sunlight it receives" (p. 19) or instead assumed a "purely passive adaptation to external circumstances" (p. 20) as the predictions from either theory would largely be the same. It is true, whether we call it *deliberate action* or *passive adaptation* does not really matter. In fact, it would merely constitute a semantic difference as long as the explanatory variables, i.e. intensity and angle of sunlight, position of tree, etc., are hypothetically combined in ways that produce identical quantitative predictions with respect to the density of leaves. However, the decisive assumptions are once again implicit and not discussed by Friedman. In putting the observable variables in a fixed quantitative relationship, and testing and adjusting this relationship based on further observations, one assumes that such a relationship really exists, that it is constant over time, and that it is specifiable to a sufficient degree of precision - in other words, that the same conditions always lead to the same density pattern, and that we have the means to specify these conditions sufficiently accurately.

Friedman eventually elaborated on two examples involving human behavior: predicting the shots of a professional Billiard player, also used in Friedman and Savage (1948, p. 298), and the hypothesis of firms maximizing expected returns. He again argued that it would not be important whether the professional Billiard player really "knew the complicated mathematical formulas that would give the optimum directions of travel, [...] could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated" (p. 21) or not. These assumptions would presumably yield very good predictions of the player's actual shots.

Likewise, firms may not actually use the formulas of economic models to maximize returns, but "the repeated failure of its implications to be contradicted" provides an "important body of evidence" (p. 22) for the hypothesis. Friedman was not specific here and just claimed that the evidence is "scattered in numerous memorandums, articles, and monographs concerned primarily with specific concrete problems rather than with submitting the hypothesis to test" (pp. 22-23). And interestingly, for this hypothesis he also referred to another kind of evidence that is completely unrelated to its predictive power, and is even contrary to the point he wanted to make. He argued that the maximizing-returns hypothesis would be rendered plausible by appropriately summarizing the "conditions for survival" in the "process of natural selection" (p. 22) on the

market. This latter argument, if anything, looks more like “testing the assumption” than looking at predictions derived from it.

This last finding may be seen as indicative of a certain difficulty in properly applying the instrumentalist-positivist methodology to the complex problems of economics. Usually economic hypotheses, like the maximizing-returns hypothesis, do not imply concrete and exact predictions that can be defined and exactly specified in terms of observable variables. Yet, if one feels inclined to regard predictive power as the most important, indeed, the only relevant criterion in evaluating a theory, as Friedman did, there are essentially two options. One could either incorporate a certain number of further assumptions that tend to be highly unrealistic to render empirically testable specification possible. Or instead one could keep the formal theoretical work to a minimum and search directly for statistical regularities in the empirical data, from which one then formulates testable hypotheses. Both can be found in the work on monetary policy evaluation by the Monetarists.¹²

Finally we have to note that some economists have suggested that there is a substantial difference between Friedman’s actual methodology and his stated ideal for positive economics.¹³ There might of course be some differences or inconsistencies between the methodology outlined in Friedman (1953b) and the actual methodology followed in his other works. Yet, arguing that they differ on a fundamental level strikes me as too strong a claim. This would imply that Friedman did either not understand his writings on methodology or did not take them seriously. A third option would be that he simply changed his mind. But then, he never formally corrected his views expressed in the 1953 essay, and in fact explicitly approved of them even 50 years later (Mäki, 2009a, p. 60, fn. 1). The claim that the methodology of Friedman (1953b) is fundamentally different from Friedman’s methodology leaves much room for interpretation, but we feel inclined to reject it. Instead, we think that the 1953 essay contains the methodological guidelines that he followed in his later works - possible inconsistencies, which are an interesting research topic in themselves, notwithstanding.

¹²In his work on monetary economics Friedman rather followed the latter path. As Hoover (1984, p. 67) points out: “Friedman is, in his own view, primarily an empirical economist, who uses a few deeply held principles to sift through facts in search of predictions.”

¹³Mäki (2009a, p. 52), for example, states:

A good account of F53 [(Friedman, 1953a)] may not be a good account of Friedman’s methodology, in some cases not even part of it. Indeed, *Friedman’s methodology* and the *methodology of F53* are not one and the same thing, even though the two are likely to be connected. That they are not the same thing is exemplified, among other things, by incongruences between the dictates of F53 and Friedman’s statements and practices elsewhere, such as unrealisticness of assumptions considered irrelevant here, but relevant there.

In particular, his emphasis of predictive failure in rejecting the Keynesian theory, as well as lengthy empirical treatments in search of empirical regularities in monetary and macroeconomic data are indications that Friedman remained loyal to *The Methodology of Positive Economics*.

3.2 Monetarist Monetary Policy Analysis

Analyzing the effects of central bank monetary policy is a complex problem with multiple layers. The traditional Keynesian approach to overcome the problem of interdependence was to build comprehensive models of the entire economy that attempted to capture all relevant effects of policy interventions on macroeconomic target variables. These models were of course not exclusively geared towards monetary policy analysis, which is one reason for their comprehensiveness. Yet, another and probably more important reason is the conviction that one part of the economy could be analyzed properly only in relation to all other parts. The basic IS-LM setup was thus a “Walrasian” type of model for the whole macroeconomy (Modigliani, 1944, p. 46).

In that regard the Monetarist approach was distinctly different.¹⁴ It was Marshallian (Boumans, 2016), as the frequent references in Friedman’s writings would indicate. As emphasized in the previous section, Monetarists did not attempt to model the entire economy as detailed as possible, but quite to the contrary, deliberately sought a practical demarcation and selection of parts from the complex whole that are to be analyzed separately as a first step.

This is of course not to argue that Marshall and later Friedman and the Monetarists did not recognize the interdependence of different parts of the economy. The emphasis on the need for partition and a partial perspective on the whole may rather be seen as a matter of practicality and a more or less direct corollary of their instrumentalist methodological views. Economic theory is seen as an engine to analyze specific problems that is judged by the accuracy of its predictions, not as a comprehensive structure or a schematic reproduction of the actual economy

¹⁴The most vivid, albeit somewhat derogatory, description of the difference between Keynesians and Monetarists in this regard that we could find is in Thomas Mayer’s *The Structure of Monetarism*:

We live in a world too complex for our intellectual apparatus. We must therefore do either of two things. One is to take account of a great many factors at the cost of being able to see their interrelations only in a vague, clouded way. The other is to simplify drastically, and to look at only a few factors. Along these lines one can classify economists into “cloud makers” and into “oversimplifiers”, to use two derogatory terms. Using this dichotomy the Keynesian is a cloud maker while the monetarist is an oversimplifier. (Mayer, 1978, p. 41)

that is judged among other things by its generality and mathematical elegance (Friedman, 1949; Friedman, 1955; Hoover, 1984, pp. 64-66).

Monetarism thus focused specifically on the role of the money stock as a policy tool.¹⁵ In Friedman's work we generally find single equation analyses. A case in point is his restatement of the quantity theory of money that will be discussed in the following section 3.2.1. We then proceed to the connection between the monetary and the real economy in section 3.2.2, before presenting the famous Friedman k-percent rule for monetary policy and its empirical justification in section 3.2.3. Among other things, the latter contains a review of Friedman's empirical work in collaboration with Anna J. Schwartz (1915-2012).

3.2.1 The Quantity Theory of Money

Modern monetary theory has been occupied with the optimization of various macroeconomic variables. One common target has been the price level. Fisher (1923) famously stated that the business cycle was largely a "dance of the dollar", suggesting that a stable price level brought about through adequate monetary policy could prevent a substantial part of economic fluctuations.¹⁶ In "The Optimum Quantity of Money", originally published in 1969, Friedman tackled the problem indirectly and claimed that while focusing on the quantity of money instead

¹⁵That Monetarists specialized in the area of monetary economics has led to them being criticized for ignoring other aspects of the economy. Walter W. Heller (1915-1987), for example, who was chairman of the Council of Economic Advisers during the Kennedy and Johnson administrations in the US, criticized Friedman in a famous debate at New York University for overemphasizing the role of monetary policy and neglecting the importance of fiscal policy (Friedman and Heller, 1969). However, one cannot possibly argue that he would ignore the fiscal side completely. As Friedman himself points out in his rebuttal:

I am in favor of expenditure decreases from a long-range point of view because I think that the U. S. federal budget is too large compared to what we're getting for it. We're not getting our money's worth out of it. And, therefore, I would like to see government spending brought down. (Friedman and Heller, 1969, p. 74)

Indeed, Friedman and several of his disciples are well known for this position and it does not really represent a neglect of the importance of fiscal policy, but rather a different assessment of the effects of fiscal policy, that is, a disagreement in positive economics one might say. According to Friedman it is in fact very important that fiscal policy is kept restrained so as not to hamper the productive and innovative forces of the private sector (Congdon, 2011). As we will see below, Friedman also assigned a rather restricted role to monetary policy.

¹⁶In the paper in which he anticipated the Phillips curve, mentioned in section 2.3.1 of the previous chapter, Fisher took the same line of argument and concluded:

In short, facts and theory both indicate that in the "dance of the dollar" we have the key, or at any rate a very important key, to the major fluctuations in employment. If this conclusion be sound, we have in our power, as a means of substantially preventing unemployment, the stabilisation of the purchasing power of the dollar, pound, franc, lira, mark, crown, and any other monetary units. (Fisher, 1973, [1926], p. 502)

of the price level or its rate of change “a definitive answer” (Friedman, 2009, p. 2) can be given as to what constitutes an optimal monetary policy.¹⁷

Central to his analysis is the quantity theory of money. Friedman (2009, p. 51) argued that it had fallen into disrepute after the Great Depression, but that the University of Chicago had been one of the rare centers that kept it alive throughout the 1930s and 40s. His version of the theory, for example in “The Quantity Theory of Money: A Restatement” (Friedman, 2009, ch. 2, pp. 51-67) borrowed directly or indirectly from the theories developed by Henry Simons (1899-1946), Lloyd Mints (1888-1989), Frank Knight (1885-1972) and Jacob Viner (1892-1970). It is in his own words not an “atrophied and rigid caricature” that its opponents often refer to, but

a more subtle and relevant version, one in which the quantity theory was connected and integrated with general price theory and became a flexible and sensitive tool for interpreting movements in aggregate economic activity and for developing relevant policy prescriptions. (Friedman, 2009, pp. 51-52)

As the first important point Friedman clarified that the quantity theory of money is above all a theory of the demand for money.¹⁸ It is not in itself a theory of “output, or of money income, or of the price level” (Friedman, 2009, p. 52). For such a broader theory, specifications of the money stock as well as other variables would be needed too. Yet, in a system of central banking under fiat money, there is, in a sense, no need for an endogenous theory of the money stock, that is, more precisely, a theory of the quantity of base money or what Monetarists often refer to as high-powered money. It can be seen as an exogenous variable or as a policy tool, if there are

¹⁷In this theoretical essay, which stands in contrast to his voluminous empirical work on the subject, he even came to the conclusion that a contraction of the quantity of money at a constant rate would be efficient if it leads to price deflation in the order of the real interest rate and a nominal interest rate of zero. He thereby abandoned the Fisherian ideal of price stability in this particular work. This result is known as the Friedman rule (De Vroey, 2016b, p. 69; Schmitt-Grohé and Uribe, 2011) and should not be confused with the more famous k percent rule discussed in section 3.2.3.

¹⁸On this point see also Friedman (1987, p. 3):

Changes in prices and nominal income can be produced either by changes in the real balances that people wish to hold or by changes in the nominal balances available for them to hold. Indeed, it is a tautology, summarized in the famous quantity equations, that all changes in nominal income can be attributed to one or the other - just as a change in the price of any good can always be attributed to a change in either demand or supply. The quantity theory is not, however, this tautology. On an analytical level, it has long been an analysis of the factors determining the quantity of money that the community wishes to hold; on an empirical level, it has increasingly become the generalization that changes in desired real balances (in the demand for money) tend to proceed slowly and gradually or to be the result of events set in train by prior changes in supply, whereas, in contrast, substantial changes in the supply of nominal balances can and frequently do occur independently of any changes in demand. The conclusion is that substantial changes in prices or nominal income are almost always the result of changes in the nominal supply of money.

no special arrangements like an exchange rate peg. Hence, we would gain much more from a suitable theory of the demand side alone than might be expected, especially for the analysis of the modern monetary system that prevailed since 1971.

Next, the Monetarist quantity theory¹⁹ distinguishes between two functions of money and thus two different sources of the demand for money.²⁰ To an “ultimate wealth-holding unit” money is just one particular form of holding wealth. To a productive enterprise it is a capital or producer’s good (Friedman, 2009, p. 52; Friedman and Schwartz, 1982, pp. 37-41; Laidler, 1985; Friedman, 1987, p. 12). Hence, the theory of the demand for money is seen as a part of capital theory.

For an individual wealth-holder the demand for money as a specific form of wealth among several can, according to Friedman, be expressed in the following form or something similar:

$$M^d = P f(y, w, R_M^e, R_B^e, R_E^e, u), \quad (3.1)$$

where M^d and P denote the nominal money demand and the price level, respectively. Dividing by P , we would obtain an expression of the real money demand M^d/P in terms of the representative basket of goods used in the construction of the price level. Variable y denotes the individual’s income at constant prices, and w signifies the fraction of wealth in non-human form. R_M^e , R_B^e , and R_E^e denote expected rates of return on money (M), fixed valued securities or bonds (B), and physical assets or equity (E), respectively. Some clarifications are in order.

The total wealth of an individual is the budget constraint in deciding how much money as a fraction of wealth should be held. The individual’s income y in equation 3.1 serves as an index of total wealth as estimates of the latter are rarely available (Friedman, 1987, p. 11). Friedman recognized the deficiencies involved as income estimates are ‘subject to erratic year-to-year fluctuations.’ He argued that the concept of permanent income developed in the more general theory of consumption (Friedman, 1957) would be more promising but is itself difficult to specify for empirical studies (Friedman, 2009, ch. 6, pp. 111-139).

Friedman separated wealth into non-human and human forms, the latter being akin to the concept of human capital. Conversion of one form into the other is rather limited. Indeed, the

¹⁹This phrase should be taken with a grain of salt. Friedman himself wrote that there is no fixed dogma of the Monetarist or Chicago quantity theory. When using this phrase we refer to the formal statement of the theory that Friedman proposed.

²⁰This insight is neither a unique nor original idea of the Monetarist school. See for example Rothbard (2009a, ch. 2, pp. 137-142) for a particularly clear discussion of the “total demand to hold” a good that separates into “exchange-demand” and “reservation-demand”. Rothbard referred to the Wicksteed (1933, vol. I, pp. 213-238; vol. II, pp. 493-526, and pp. 784-788) and Boulding (1941, pp. 51-80).

earning capacity of an individual stems from his or her human wealth or capital, but the latter cannot directly be sold and bought on the market in contrast to non-human wealth. Hence, Friedman considered the fraction of wealth in non-human form, w , a potentially important explanatory variable for the demand for money (Friedman, 1987, p. 11). Again, it is approximated as the fraction of income derived from non-human forms of wealth or property, that is, capital income.

The role of the different expected rates of return is comparable to the role of marginal utilities of goods in general consumption theory. The rate of return on money, R_M^e , is compared to the rate of return of alternative forms of wealth such as bonds, R_B^e , and equity, R_E^e . The higher the rate of return on money relative to the other rates of return, the higher the demand for money as a form of wealth. As for marginal utilities in consumption analysis there will be a tendency towards the equalization of the rates of return according to Friedman.

This, however, seems to be a claim that can hardly be defended on empirical grounds. And also from a theoretical perspective, just like in consumption theory, it seems more adequate to think of an equalization of the marginal utilities of the different forms of wealth - their rates of return being only one of the determining factors. For example, a Keynesian economist might rightly argue that bonds are bought precisely because of a higher rate of return, that compensates for a loss in liquidity, which is another factor determining the marginal utility of a certain form of wealth.

Moreover, Friedman acknowledged the potential impact of other factors and included the portmanteau symbol u to denote them. Among other things he mentioned the role of the expected stability of the economic system. Liquidity is valued higher under instability and hence the demand for money in times of wars or crises, for example, tends to be higher. Price inflation as another important factor is incorporated in the expected rates of return. More specifically, it sometimes serves as a proxy for the rate of return on physical assets, R_E^e (Friedman, 1987, p. 12).

Accepting some simplifying assumptions, such as ignoring possible distributional effects for the variables y and w over the whole population, one can shift from the individual money demand function in equation 3.1 to an aggregated per capita money demand function. One would then just interpret y and w as per capita income and per capita capital income, respectively, and M^d as per capita money holdings. Friedman explained the difference of the two interpretations of equation 3.1 as follows:

Although the mathematical equation may be the same, its significance is very different for the individual wealth-holder and the community as a whole. For the individual, all the variables in the equation other than his own income and the disposition of his portfolio are outside his control. He takes them, as well as the structure of monetary institutions, as given, and adjusts his nominal balances accordingly. For the community as a whole, the situation is very different. In general, the nominal quantity of money available to be held is fixed and what adjusts are the variables on the right-hand side of the equation, including an implicit underlying variable, the structure of monetary institutions, which, in the longer run, at least, adjusts itself to the tastes and preferences of the holders of money. A dramatic example is provided by the restructuring of the financial system in the US in the 1970s and 1980s. (Friedman, 1987, p. 13)²¹

In the aggregate, the amount of money held does not really adjust, as it is determined exogenously. Instead the variables on the right-hand side adjust to any disequilibrating changes. Before discussing these adjustments, however, the second source of demand needs brief consideration.

In the Monetarist view, for a productive enterprise money qualifies as a producer's good and is demanded for its productive services. Although much less empirical work has been done on this aspect of the demand for money due to lack of data, Friedman argued that most of the variables deemed relevant for the money demand of the wealth-holder also play a role for the productive enterprise. The rate of returns are "highly relevant to business enterprises" as they "determine the net cost of holding money balances." (Friedman, 1987, p. 13)

There is no budget constraint in the same way as there is for the wealth holding individual, but a similar variable, y , that represents the scale or size of the enterprise might be important. Friedman suggested for example total transactions, net income, or net worth as potential proxies. However, the distinction between income from human and non-human wealth is probably not very important for a business enterprise.

The portmanteau variable u incorporates expectations about the future stability of the economy, which are also very likely to be important for businesses as they affect the expected need

²¹The last assertion from the quoted passage is as it seems an application of the more general case for the free market order to the more specific case of the financial system as satisfying consumer preferences. It is however an astonishing claim by Friedman, champion of laissez-faire in many areas (Friedman, 1962; Friedman and Friedman, 1980), to interpret the restructuring of the financial system in the 1970s and 80s as akin to the restructuring of any private production sector in response to shifts in consumer preferences. After all money production has not been private before the 1970s nor thereafter.

for cash balances. Other factors may have to be added that are important for businesses but not necessarily for wealth-holders. Yet, equation 3.1 as it stands, ignoring w , could be interpreted as representing the money demand of a productive enterprise, and thus - keeping w and “with even more serious qualifications about the ambiguities introduced by aggregation” (Friedman, 1987, p. 14) - it can be seen as an aggregated per capita demand for money both for wealth-holding as well as production if we “broaden our interpretation of u .” (Friedman, 2009, p. 60)

Hence, using equation 3.1 and multiplying it by N , the total population number, and equating it to the money supply, M^s , Friedman developed a generalized quantity equation in the Cambridge cash balance tradition:²²

$$M^s = NP f(y, w, R_M^e, R_B^e, R_E^e, u) \quad (3.2)$$

Any change in the money supply is transmitted to the right-hand side variables in some way. Yet, as Friedman himself pointed out:

A frequent criticism of the quantity theory is that its proponents do not specify the transmission mechanism between a change in M^s and the offsetting changes in other variables, that they rely on a black box connecting the input - the nominal quantity of money - and the output - effects on prices and quantities. (Friedman, 1987, p. 14)

The claim that Monetarism relies on a black-box connection between real and nominal magnitudes seems to be justified to some extent. Yet, in Friedman’s eyes, and very much in line with his methodological writings, this does not constitute an important criticism. From the instrumentalist-positivist point of view it is irrelevant how the causal chain unfolds in detail as long as the postulated empirical relationship between changes in the money stock and changes of the right-hand side variables holds.

There are nonetheless some, but by no means comprehensive, discussions of the transmission mechanism in Friedman’s writings. They are the subject of the next section.

3.2.2 The Connection to the Real Economy

Friedman (1987) pointed out several times that changes in the money stock are likely to have an

²²The classical quantity theory of money that is associated with Fisher (1911) saw money exclusively as a medium of exchange and was a theory concerning the supply of money. The Cambridge cash balance approach associated with Pigou (1917) and Keynes (1924) saw money as a store of value or a form of wealth, which is demanded as such.

initial effect on income and the rates of return, but that its long-run effect is predominantly on the price level. A sustained increase in the money stock may thus lead to sustained price inflation as well as changes in real economic variables.

In his 1958 essay “The Supply of Money and Changes in Prices and Output” (republished in Friedman, 2009, ch. 9, pp. 171-187), he asked the central question around which much of modern macroeconomic analyses of the costs and benefits of monetary policy have entwined: “Is a rising price level favorable or unfavorable to rapid growth in output?” And although he clearly stated that “[n]o conclusive answer can be given” (p. 172), he presented some empirical evidence suggesting the former. In fact, by combining a positive link between rising prices and economic growth with the quantity theory of money, Friedman’s essay indicated a positive relationship between monetary expansion and economic growth.

He argued that this connection is particularly pronounced in the short-run dynamics of the economy over the course of the business cycle:

Over the cycle, prices and output tend to move together - both tend to rise during expansions and to fall during contractions. Both are part of the cyclical process and anything, including a monetary change, that promotes a vigorous expansion is likely to promote a vigorous rise in both and conversely. (Friedman, 2009, pp. 181-182)

Only a few lines later, Friedman clarified that the connection has its limits. Monetary changes that exceed a certain order of magnitude, that is, extreme monetary expansions or contractions, would both be detrimental, since they would lead to wasteful and inefficient use of resources as seen in periods of hyperinflation or during the price deflation of the Great Depression from 1929-1933. The relationship between monetary policy and real economic activity must therefore be seen as a rather narrow one.

Moreover, Friedman pointed out that the long-run analysis is less clear. He held that output over a longer time horizon is first of all determined by real economic factors. This is when “money and price changes play their parts as the supporting cast” (Friedman, 2009, p. 182). But there are diverging views on the additional impact of sustained moderate price increases on real economic activity. Friedman very briefly sketched four alternative explanations for a beneficial impact that had been, and still are to some extent, advocated by different economists, especially Keynesians.

First, the necessary reallocation of resources in the market depends on changes in relative prices and wages, which are more easily brought about, so it is argued, when there is a general

upwards pressure on the price level. Second, others have argued that costs and wages tend to be stickier than other prices. Hence, an upwards pressure on prices can increase profit margins and production. Third, another view holds that there tends to be a general upward pressure on labor costs due to labor unions. If prices are not pushed upwards employment and production would diminish. According to the last explanation offered interest rates are particularly slow in adjusting to price increases. Entrepreneurs would therefore have an incentive to borrow and invest more than otherwise.

Friedman then moved on to mention some of the opposite positions, relativizing the alleged benefits and even emphasizing possibly detrimental effects of sustained price inflation:

In opposition to this view, it has been argued that generally rising prices reduce the pressure on enterprises to be efficient, stimulate speculative relative to industrial activity, reduce the incentives for individuals to save, and make it more difficult to maintain the appropriate structure of relative prices, since individual prices have to change in order to stay the same relative to others. Furthermore, it is argued that once it becomes widely recognized that prices are rising, the advantages cited in the preceding paragraph will disappear: escalator clauses or their economic equivalent will eliminate the stickiness of prices and wages and the greater stickiness of wages than of prices; strong unions will increase still further their wage demands to allow for price increases; and interest rates will rise to allow for the price rise. If the advantages are to be obtained, the rate of price rise will have to be accelerated and there is no stopping place short of runaway inflation. (Friedman, 2009, p. 183)

Following his positivistic research paradigm, Friedman did not engage in a more detailed theoretical discussion to evaluate and weigh the opposing claims, but looked at some of the available empirical material that, as so often, does not provide clear-cut evidence for any of these positions.

In fact, Friedman and Schwartz (1963b, p. 55) admitted that they “have little confidence in [their] knowledge of the transmission mechanism, except in such broad and vague terms as to constitute little more than an impressionistic representation rather than an engineering blueprint.” This confession of ignorance explains their relatively crude empirical analysis that usually remains on a very broad and aggregated level. In an interview, when asked about different approaches to time series analysis, Friedman stated:

I think the major issue is how broad the evidence is on which you rest your case. Some of the modern approaches involve mining and exploring a single body of evidence all within itself. When you try to apply statistical tests of significance, you never know how many degrees of freedom you have because you're taking the best out of many tries. I believe that you have a more secure basis if, instead of relying on extremely sophisticated analysis of a small fixed body of data, you rely on cruder analysis of a much broader and wider body of data, which will include widely different circumstances. (Taylor, 2001, pp. 121-122)

Note that fellow Monetarists Brunner and Meltzer (1974) criticized Friedman for the lack of a solid theoretical foundation. They wrote: "Friedman's statement of monetary theory does not seem to us an adequate underpinning for monetary theory or a particularly useful basis for empirical work" (p. 65). More precisely, they lament the absence of financial and credit markets, interest rates, and relative prices. This critique is of great importance as it has been leveled against Friedman's theory after the publication of his "A Theoretical Framework for Monetary Analysis" (Friedman, 1970b), which was an attempt to embed the central Monetarist view into a Keynesian IS-LM framework. One important conclusion from this article was that the differences between the Keynesian and Monetarist view were not necessarily of theoretical but rather empirical nature.

Criticizing Friedman's underlying theoretical framework for a lack of sophistication thus amounted to a critique of the Keynesian theory more broadly. In that regard we can interpret Brunner and Meltzer (1974) as contributors to the transition from Keynesianism and Monetarism to New Classical Economics, which we will discuss in the next chapter.²³

Friedman (1970b) offered a theoretical synthesis of both the Keynesian and the Monetarist positions (Friedman, 1972; Gordon, 1974; Tobin, 1972b, 1981) and showed that their difference lay essentially in a couple of open questions he thought to be answerable empirically:²⁴

How important are changes in the supply of money compared with changes in the demand for money? Are transactions variables or asset variables most important in determining the demand for money? How elastic is the demand for money with

²³On the close connection between New Classical Economics and Monetarism, especially of the Brunner and Meltzer type, see Hoover (1984).

²⁴It was precisely the contribution of the New Classical economists to show that generalized answers to these questions are not simply found empirically, but require theoretical groundwork. See in particular section 4.1, pp. 126ff.

respect to interest rates? With respect to the rate of change in prices? When changes in demand or supply occur that produce discrepancies between the quantity of money that the public holds and the quantity it desires to hold, how rapidly do these discrepancies tend to be eliminated? Does the adjustment impinge mostly on prices or mostly on quantities? Is the adjustment process cyclical or asymptotic? Is the adjustment to sharp changes over short periods different in kind or only in degree from the adjustment to slower changes over longer periods? How long does it take for people to alter their anticipations in light of experience? (Friedman, 1970b, p. 234)

Monetarist typically hold that the demand for money is relatively stable and that the adjustments to discrepancies between the quantity of money held by the public and the desired quantity impinges mostly on prices, at least when the changes occur slowly over longer periods. And they have provided extensive empirical studies to support these views, most notably in Friedman and Schwartz (1963a,b).

In their view, however, a detailed investigation of all the above questions, although desirable would quickly overload the empirical analysis. They tried to keep it broad and manageable, always focused on a relatively small number of aggregated variables. In the following section, we will go into their empirical studies.

3.2.3 The k-Percent Rule

The first important publication that contained an outline of what Friedman considered to be an optimal monetary policy is *A Program for Monetary Stability*, a series of four lectures given in 1959 at Fordham University (Friedman, 1960). In a first step, Friedman makes an argument why government should be involved in the production of money, that is, why there should be something like monetary policy in the first place. This argument is of particular importance for an analysis of the costs and benefits of central banking. Friedman argued that under a traditional commodity standard a substantial part of the productive forces of the economy will be directed towards the production of money. So he estimated that under a pure commodity standard the US in 1960 would have devoted “about $2\frac{1}{2}\%$ of its annual product or about \$8 billion a year to produce directly or indirectly through foreign trade additional amounts of the monetary commodity” (Friedman, 1960, p. 5), which from a societal point of view can be regarded as a waste of resources if there are less expensive ways of providing a medium of exchange.

Friedman argued that there are thus incentives to find cheaper ways of providing a medium of exchange, which has led to the introduction of “fiduciary elements into the monetary system” (p. 6). A purely fiduciary currency, or a fiat money system, would reduce the costs of production for money to a minimum of virtually zero.²⁵

Obviously, if the money production under these conditions is in the hand of competing private banks, there would be strong incentives for any individual bank to expand the quantity of money to the point where its exchange value equals the marginal costs of production. Hence, in such a system, central banks would be necessary in order to monopolize and control the production of money. The obvious question is then how to control the production of money optimally?

First, in the spirit of the Chicago Plan,²⁶ Friedman asked for 100% reserve requirements on demand deposits, which would erase one important source of instability from the monetary system, namely the money creation of commercial banks on top of the base money creation of central banks.²⁷ In the most extreme cases such an arrangement would prevent “liquidity crises involving bank runs, banking panics, [and] suspension of convertibility of deposits into currency” (Friedman, 1960, p. 67).

From the point of view of Monetarism, which aims at the control the money stock as a policy tool, this is a perfectly consistent and plausible proposal as it would eliminate the possibility of money creation by any non-central-bank institution. It would thereby give full control over the quantity of money in the economy to central banks, and simplify the discussion of what

²⁵In chapter 9, pp. 297ff., we will engage in a cost accounting analysis for the central bank system of the euro area. It shows that there are still substantial costs involved in implementing and maintaining the system. Although it is in principle conceivable that the costs of fiat money production are next to nothing, the historical record of the euro system tells a different story. Moreover, this argument ignores a very important aspect as it is not only the costs of production that matter, but also the quality of the product and its wider implications. See section 7.3, pp. 257ff., for a detailed discussion. It is important to note at this point that Friedman has changed his mind on this topic in his very last publication with Anna Schwartz (Friedman and Schwartz, 1986, p. 58).

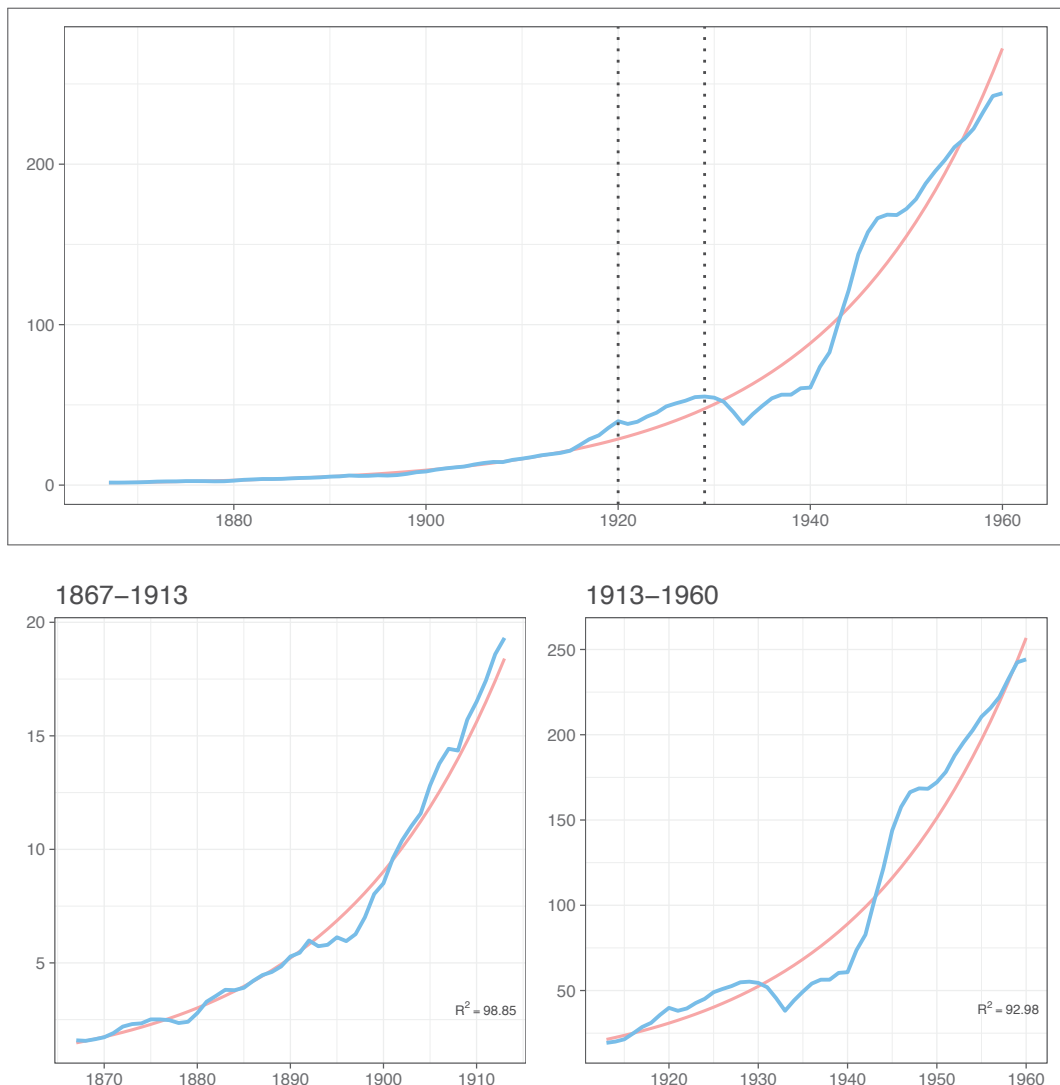
²⁶For a historical review of the Chicago Plan see Phillips (1994), and also Benes and Kumhof (2012, pp. 17-19) for a brief summary. It goes back to the ideas of Nobel Prize winning chemist Frederick Soddy (1926) that were picked up by Frank Knight (1927). In 1933, in the midst of the Great Depression, Knight (1933) and Simons (1933) addressed two memoranda to president Roosevelt outlining a plan to implement a 100% reserve system, the same year that John M. Keynes published an open letter to the president (Keynes, 1933). Irving Fisher (1935, 1936) also supported the plan.

²⁷Indeed, the critique of fractional reserve banking has a longer tradition in economics, spanning from the late scholastics of Salamanca (Huerta de Soto, 2006, pp. 83-97), the Currency School (Ricardo, 1824), to the Austrian (von Mises, 1953) and the Chicago Schools. Note however, that the question of full vs. fractional reserves is in principle independent of whether or not a fiat money standard is deemed desirable. One could advocate a commodity standard and 100% reserves as von Mises (1953) or a fiat standard with 100% reserves as Friedman did, or either standard with fractional reserves for that matter. The recent *Vollgeld* initiative in Switzerland and Germany (Gocht, 2011; Huber and Robertson, 2008; Huber, 2014; Mayer and Huber, 2014) is very much in the spirit of the Chicago proposal. In France, the 1988 Nobel prize winning economist Maurice Allais (1911-2010) made similar suggestions to reform the financial system (Allais, 1967, 1999a,b,c). Also Keynesian economist James Tobin suggested a 100% reserve system (Tobin, 1985).

monetary aggregate to focus on substantially. The only relevant measure would be high-powered money, that is, M_1 , which would be identical to M_0 .

Second, Friedman made a suggestion of how to change the money stock over time, known as the k-percent rule, which relied heavily on earlier (Friedman, 2009, [1958], ch. 9) and ongoing historical research on macroeconomic aggregates from after the Civil War until 1960, published in the celebrated *Monetary History of the United States* (Friedman and Schwartz, 1963a), and further elaborated on in later works (Friedman and Schwartz, 1963b, 1970, 1982).

Figure 3.1: Estimates of the money stock from 1867-1960 in billions of dollar from Table A-1 in Friedman and Schwartz (1963a, pp. 704-722)



We have added a simple exponential fit (in red) to the original time series (in blue). The dotted vertical lines indicate the crisis of 1920 and the beginning of the Great Depression in 1929, respectively. The bottom panels show the same plots for the pre and post Federal Reserve era.

Figure 3.1 contains a plot of Friedman and Schwartz's estimates of the money stock for a period of 94 years. The correlation of economic crises and contraction of the money stock is visible in the upper panel. From the bottom panels we can see that the post as well as pre-Federal-Reserve money stock developed roughly according to an exponential growth function, but the fluctuations of the series around the exponential trend have become stronger after 1913. This suggests that the Fed has not brought the evolution of the money stock closer to Friedman's ideal of a constant growth rate.

Friedman (2009, [1958], ch. 9) compared growth rates of the money stock with developments in economic activity and found that both follow a very similar pattern. Peaks and troughs in the growth rate of the money stock systematically preceded peaks and troughs in the level of economic activity by 16 and 12 months on average, respectively. He argued that this constitutes "strong though not conclusive evidence for the independent influence of monetary change" and that "it must take a long time for the influence of monetary changes to make themselves felt" (p. 180).²⁸ He concluded that, given the "present level of knowledge" (p. 181) there should be no discretionary fine tuning of monetary policy. Instead, a steady and predictable rule should be followed. He favored changing the money stock at a constant rate of k percent over time. And k should be chosen so that an optimal long-term development of the price level is obtained.

Yet, Friedman (2009, [1958], p. 184) found that "[a]ll in all, perhaps the only conclusion that is justified is that either rising prices or falling prices are consistent with rapid economic growth, provided that the price changes are fairly steady, moderate in size, and reasonably predictable." And hence,

If it is necessary to state objectives in terms of a price level goal, then a stable price level has the very great advantages of (a) ease of public understanding, (b) definiteness rendering successive alterations in the precise goal less likely, and (c) probably the closest approach to equitable treatment of the various members of the community. However, the difficulty of assuring the close attainment of any price level goal suggests that it might be better to express the immediate policy goal in terms of some variable other than the price level, for example as being the attainment

²⁸This claim was criticized for example by Culbertson (1960, 1961) who argued that the mere timing of peaks and troughs is misleading because Friedman was, among other things, comparing a rate of change of one series to the absolute level of another: "Friedman compares, for example, the maximum rate of increase in money supply with the upper turning point in business, i.e., with the absolute maximum in output. Now, of course, in any smooth cyclical series the maximum rate of increase occurs before the absolute maximum [...]." This is a very elegant argument from a technical point of view, but it gives itself no reason to believe that the difference would be substantial. And it also bags the question whether output is really a *smooth* series.

of a steady 4 percent per year rise in the stock of money, and then to let the price level be whatever would be consistent with this money goal. (p. 185)

In Friedman (1960) the same conclusion is drawn, but even more specifically with price stability as a long-term goal in mind, and, given that the full reserve system is not actually implemented, he also gave greater attention to the definition of the money stock for which the growth rule was formulated. As Friedman correctly stated a complete specification of the rule does not only require a growth rate k but also a definition of the quantity it relates to. He found that the money stock defined as “currency held by the public plus adjusted demand deposits plus time deposits in commercial banks but [that] excludes time deposits in mutual savings banks, shares in savings and loan associations, and the like” is the most useful concept because “it seems to be somewhat more closely related empirically to income and other economic magnitudes” (Friedman, 1960, pp. 90-91) than either a broader or narrower concept.²⁹

However, Friedman also argued that the chosen definition is probably not vital, as long as an exact choice is made. The adopted concept should incorporate at least currency as well as demand deposits, and the growth rate k has to be adjusted accordingly. The goal is to find a rate “so that on the average it could be expected to correspond with a roughly stable long-run level of final product prices.” (Friedman, 1960, p. 91) Friedman suggested that for a slightly broader concept a higher growth rate would be adopted, for a slightly narrower concept a smaller one. In both cases it would not be radically different. For the definition that Friedman proposed, the growth rate would be “slightly over 4% per year on the average of the past 90 years, something like 3% to allow for growth in output an 1% to allow for a secular decrease in velocity, which is to say for the increase in the stock of money per unit of output that the public has wished to hold as its real per capita income rose” (p. 91). Again he argued that slight deviations would not be serious. The important advantage of a fixed growth rate for the money supply would be that it eliminates the causal impact of money on larger business fluctuations. Whether it leads to stable, slightly declining or rising prices is a secondary matter.

A detailed and comprehensive account of the data underlying these claims was then published in the widely celebrated *Monetary History* (Friedman and Schwartz, 1963a). Almost a century worth of gathered and estimated time series data, from 1867 to 1960, came under scrutiny on

²⁹It should be reiterated at this point that such a purely empirical justification is perfectly in line with the methodological views outlined in section 3.1. Friedman was avoiding theoretical inquiry on the basis of simplifying and unrealistic assumptions and looked directly for statistical regularities in the available data. As we have argued above either way is compatible with *The Methodology of Positive Economics*.

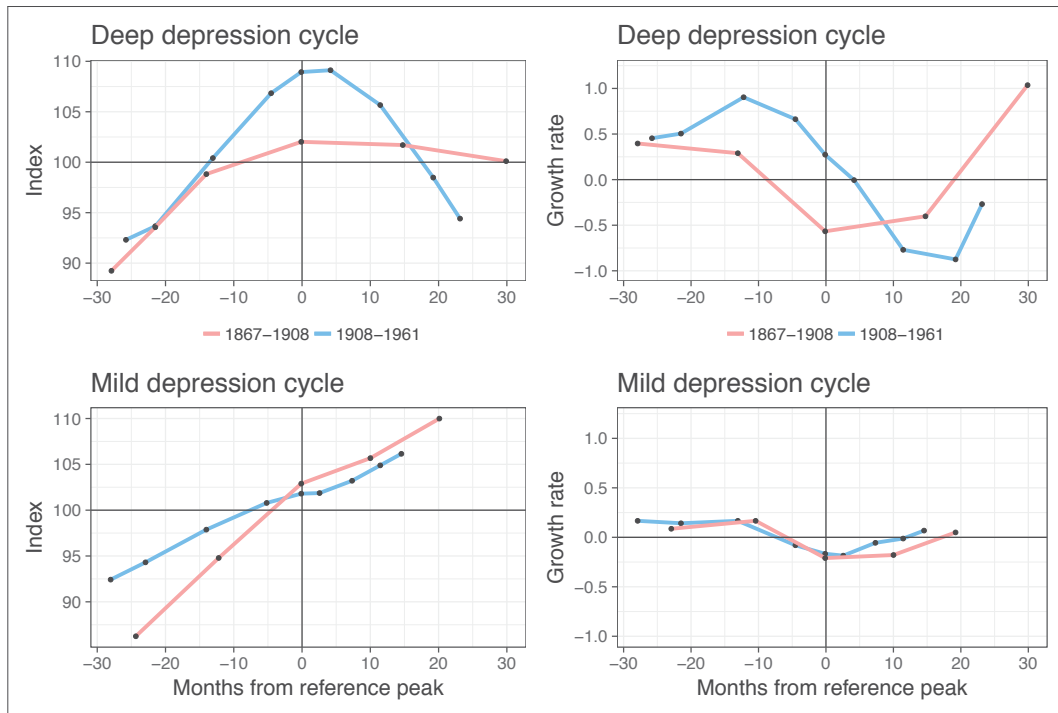
over 800 pages - and still, the book was first of all about the stock of money as stated in its very first sentence. It is more precisely an investigation of consecutive periods, each of which can be seen as a natural experiment (Bernanke, 2002) or a case study analyzed from the point of view of “Monetary Clinicians” (Rockoff, 2006, p. 42).

Friedman and Schwartz identify four periods of relative economic stability, 1882-92, 1903-13, 1923- 29, and 1948-60, each of which is characterized by a relatively stable rate of change of the money stock. They identify six periods of severe contraction, the most severe one in 1929-33, the others in 1873-79, 1893-94, 1907-08, 1920-21, and 1937-38. Each of these periods was characterized by a decline in the stock of money, the strongest again in 1929-33. Only two other periods, with the exception of some isolated months, exist in which there was an absolute decline in the stock of money, 1948-49 and 1959-60, and in both cases it was substantially smaller than during the severe contractions. The remaining less severe contractions “have left their impress in the form of a slower rate of growth of the stock of money during contractions than during expansions, rather than in an absolute decline” (Friedman and Schwartz, 1963a, p. 677) so that a contraction, whether severe or not, is always associated with a decline in the growth rate of the money stock. Over the whole cycle there was first an increase in the growth rate during the expansion phase and then prior to the beginning of the contraction phase the growth rate starts to decrease.

In another publication Friedman and Schwartz (1963b) elaborate on this analysis and calculate average reference cycles for mild as well as deep depressions over the periods of 1867-1908, for which only annual data was available, and 1908-1961, for which quarterly data was used. The reference cycles are reproduced in Figure 3.2. Friedman and Schwartz (1963b, p. 48) come to the conclusion that “[t]he stock of money displays a consistent cyclical behavior which is closely related to the cyclical behavior of the economy at large. This much the factual evidence summarized above puts beyond reasonable doubt.” Moreover, they conclude:

There seems to us, accordingly, to be an extraordinarily strong case for the propositions that (1) appreciable changes in the rate of growth of the stock of money are a necessary and sufficient condition for appreciable changes in the rate of growth of money income; and that (2) this is true both for long secular changes and also for changes over periods roughly the length of business cycles. To go beyond the evidence and discussion thus far presented: our survey of experience leads us to conjecture that the longer-period changes in money income produced by a changed

Figure 3.2: Average reference cycles for mild and deep depressions for the money stock (left panel) and the rate of change of the money stock (right panel)



This plot correspond to Chart 2 and 4 in Friedman and Schwartz (1963b, pp. 34 and 36). War cycles 1914-19 and 1938-45 are excluded. Deep depression cycles correspond to the periods 1870-79, 1891-94, 1904-08, 1919-21, 1927-33 and 1933-38.

secular rate of growth of the money stock are reflected mainly in different price behavior rather than in different rates of growth of output; whereas the shorter-period changes in the rate of growth of the money stock are capable of exerting a sizable influence on the rate of growth of output as well. (Friedman and Schwartz, 1963b, p. 53)

Friedman and Schwartz also showed how, especially for severe contractions after the foundation of the Federal Reserve System in 1913, the decreasing growth rate of the money stock can be explained by external factors and political decisions, such as rises in discount rate in January and June of 1920, bank runs that led to massive bank failures in the early 1930s and to a decreasing deposit to currency ratio,³⁰ or the doubling of legal reserve requirements in 1936-37.

³⁰Friedman and Schwartz famously argued that the Great Depression could have been over in 1930 had the Federal Reserve only provided the necessary liquidity through expansionary measures, which was entirely in their powers “without any serious threat to the gold standard.” (Friedman and Schwartz, 1963b, p. 54) They agreed with the view of Snyder (1940, p. 203):

We share the view expressed by Carl Snyder, for many years associated with the New York Bank as a statistician and economist, that if Benjamin Strong could “have had twelve months more of vigorous health, we might have ended the depression in 1930, and with this the long drawn out world crisis

This they argued provides further evidence for the chain of causality runs mostly from money to economic activity (Friedman and Schwartz, 1963b, p. 54).

Although Friedman and Schwartz have acknowledged that their evidence is not conclusive, they were criticized precisely because of suggesting a causal connection running from money to economic activity based almost entirely on empirical relationships, such as the fact that peaks and troughs in the growth rate of the money stock preceded peaks and troughs in economic activity, and historical narrative. It was argued that they were falling fool to the *post hoc ergo propter hoc* fallacy (Tobin, 1970). This is indeed a criticism that can be leveled against any correlation analysis that draws causal conclusions (Hoover, 2001a,b). However, the *Monetary History* was overall received very favorably. Even Tobin (1970, p. 301) called it a “monumental” book.

Its core conclusions were somewhat sobering in their outlook on what monetary policy can actively change for the better, and alarming in what it can change for the worse. This view is summarized in reference to John Stuart Mill (1906-1973) in the final chapter of the book:

Money is a veil. The “real” forces are the capacities of the people, their industry and ingenuity, the resources they command, their mode of economic and political organization, and the like. As John Stuart Mill wrote more than a century ago:

There cannot, in short, be intrinsically a more insignificant thing, in the economy of society, than money; except in the character of a contrivance for sparing time and labour. It is a machine for doing quickly and commodiously, what would be done, though less quickly and commodiously, without it: and like many other kinds of machinery, it only exerts a distinct and independent influence of its own when it gets out of order. [...] (Mill, 1929, [1848], p. 488)]

Perfectly true. Yet also somewhat misleading, unless we recognize that there is hardly a contrivance man possesses which can do more damage to a society when it goes amiss. (Friedman and Schwartz, 1963a, pp. 696-697)

that so profoundly affected the ensuing political developments.” (Friedman and Schwartz, 1963a, p. 692)

Benjamin Strong (1872-1928) was Governor of the Federal Reserve Bank of New York, the biggest of the member banks that had great influence on the policies of the whole Federal Reserve System. He was known to be willing to implement expansionary policies.

In this quotation we find reflected the classical idea that monetary policy is optimal when it is restricted and does not exert much of a direct influence itself, in other words, when it is “neutral.” The costs of monetary policy are then seen as *benefits forgone* due to irresponsible deviations from the optimum path that cause or reinforce economic fluctuations and business cycles. It is thus the first and foremost task of monetary policy to prevent these deviations and restrict itself to the k percent growth rule.

Like Fisher, Friedman and Schwartz came out to be in favor of a more or less stable price level, which they thought can be accomplished best when k lies in the range of 3 to 5% per year. The role of monetary policy over the long run would be above all a passive one. They were opposed to discretionary policies and advocated this rule-based approach, which is sensible in light of the acknowledged ignorance about the precise workings of the transmission mechanism. However, as progress is made on this research area an adaption of the rule would be a logical consequence. Among the Monetarists it was for example Laidler (1993) who argued that the strict k percent rule should be relaxed, taking into consideration changes in other factors, such as the demand for money.

Friedman, although commonly seen as the pessimistic alerter when it comes to politics in general, has of course let this route open, not least because he advocated a flexible fiat money standard, but also because he made concessions to the potential of improving short-run conditions through monetary policy. This can be seen best in his analysis of the Phillips curve which we cover in the next section.

3.3 The Expectations-Augmented Phillips Curve

The economic theory around the empirical relationship of the Phillips curve as discovered by Fisher (1973, [1926]) for the United States and Phillips (1958) for the United Kingdom, and popularized by Samuelson and Solow (1960) underwent a significant revision by the end of the 1960s as a consequence of the contributions by Monetarist economists, most notably again Milton Friedman.

However, in that regard we have to consider the contribution of the 2006 Nobel laureate Edmund S. Phelps (born 1933) as well, although it would be false to consider him a Monetarist. He received his Ph.D. from Yale University in 1959 where he studied under fellow Nobel laureates James Tobin (1981) and Thomas Schelling (2005). He was later employed by the

Cowles Commission and was in contact with Samuelson and Solow when visiting the MIT in 1962/1963. His correction of their interpretation of the Phillips curve as representing a permanent and stable trade-off was arguably his most important contribution to economics.

Friedman and fellow Monetarists took a similar line of argument as Phelps. It is outlined in section 3.3.1. The closing section of this chapter will then discuss how the instrumentalist methodology presented in section 3.1 above was put to work with respect to this most important theoretical contribution in monetary theory and policy.

3.3.1 The Natural-Rate Hypothesis

Friedman's 1967 presidential address at the 80th annual meeting of the American Economic Association, which was subsequently published under the title "The Role of Monetary Policy" (Friedman, 1968), contained a twofold criticism of the prevailing contemporary opinions on monetary policy and what it allegedly can accomplish. First, he pointed to the fact that monetary authorities could not keep interest rates pegged for longer than a rather limited period. Second, he argued that they cannot peg the rate of unemployment for very long either. The reason for both restrictions lies in the following argument.

Imagine central bankers would like to lower interest rates. When they increase the rate at which the money supply expands through larger open market operations, interest rates will initially fall as a larger money supply generally leads to a larger supply of credit. Sooner or later, however, price inflation will adjust to this accelerated rate of monetary growth. As price inflation becomes higher, creditors will demand compensation in the form of higher interest rates. We see that Friedman thus brought in a distinction between short-run and long-run effects of monetary policy. In the short run it may well be possible to lower or increase interest rates by increasing or decreasing the rate of monetary expansion. Yet, in the long run nominal interest rates will increase or decrease again due to higher price inflation or deflation, respectively.

A very similar mechanism is at work in the case of unemployment. It has essentially the same source, but it was less acknowledged at that time. Friedman (1968, p. 7) writes:

The second limitation I wish to discuss goes more against the grain of current thinking. Monetary growth, it is widely held, will tend to stimulate employment; monetary contraction, to retard employment. Why, then, cannot the monetary authority adopt a target for employment or unemployment - say, 3 per cent unemployment; be tight when unemployment is less than the target; be easy when unemployment is

higher than the target; and in this way peg unemployment at, say, 3 per cent? The reason it cannot is precisely the same as for interest rates - the difference between the immediate and the delayed consequences of such a policy.

The bridge between the short and the long run is built through *adaptive expectations* formation. More precisely, in Wicksellian spirit, Friedman introduced the concept of the *natural rate of unemployment* as being “consistent with equilibrium in the structure of *real wage rates*”, and as being mainly determined by institutional and political conditions.³¹ He argued that in the long run an economy will return to this rate as price inflation expectations adapt to the actual rate of price inflation, even though unemployment can be pushed below that level temporarily by accelerating the rate of monetary expansion.

In the short run, increased sales revenues due to higher nominal demand may trigger an increase in the demand for labor. Yet again, as product prices increase, there will also be an upward pressure on nominal wages, eventually pushing the real wage structure back to equilibrium. This entails a return of the unemployment rate to its *natural* level. This theory of adaptive expectations led to the notion of the expectations-augmented Phillips curve.³²

Consequently, Friedman’s analysis led to a distinction between the short-run Phillips curve, which is downward sloping, and the long-run Phillips curve, which is just a vertical line, exhibiting no relationship at all. This corresponds to the representation that we find in most university textbooks today (Mankiw, 2012, pp. 769ff.; Samuelson and Nordhaus, 2007, p. 947).

Edmund Phelps provided a very similar, but more formal exposition of the same ideas (Phelps, 1967, 1968b,a). That is why this contribution is commonly referred to as the “Friedman and Phelps natural-rate hypothesis” (Fields and Noble, 1981; Gordon, 2011).³³ Yet, as compelling

³¹Friedman (1968, p. 9) clarifies: “To avoid misunderstanding, let me emphasize that by using the term ‘natural’ rate of unemployment, I do not mean to suggest that it is immutable and unchangeable. On the contrary, many of the market characteristics that determine its level are man-made and policy-made.”

³²Several years before, von Mises ([1924] 1953, pp. 218ff.) had already pointed out that the effects of monetary policy are contingent on the expectations of entrepreneurs and employees and thus anticipated this line of reasoning. However, Friedman did not quote von Mises. We will dwell on the latter’s contributions in the second part of the thesis.

³³For a discussion of the differences between Friedman’s and Phelps’s expositions see De Vroey (2016b, pp. 95-111), who argues that the two contributions, although reaching the same conclusion, “are poles apart in style, aim and method” (p. 109). One particularly interesting claim that we find in Michel De Vroey’s analysis is that Friedman was pursuing a political agenda whereas Phelps attempted to advance technical economics. He writes:

Friedman’s overarching aim was to uphold a policy conclusion. The Presidential Address was one of the many manifestations of his recurrent aim of dismissing Keynesian activation policy. While introducing the notion of a natural rate of unemployment, the central message he wanted to convey was that monetary policy should consist of rules rather than discretion. By contrast, Phelps wrote for the sake of the advancement of analytical theory. (p. 109)

or non-compelling these theoretical considerations were, the issue was widely regarded as an empirical one, not central to a theoretical divide between Keynesians and Monetarists. Whatever empirical studies suggest the nature of the Phillips curve to be, Keynesians as well as Monetarists could easily adjust to it without ceasing to be part of either camp (Mayer, 1978, p. 36-37). In fact, De Vroey (2016b, p. 108) calls the natural-rate hypothesis a “contribution to Keynesian macroeconomics.”

3.3.2 Testing the Hypothesis

Among other things it was surely a lack of conformity of the available empirical data with the predictions made from large-scale models that triggered the success of the natural-rate hypothesis. The theory had to be adjusted to ideally become a more reliable guide to political decision making. In the United States, price-inflation rates were systematically underestimated by the models as shown in section 2.2.3 of the previous chapter. The natural-rate hypothesis helped to explain this shortcoming.

Also in other countries the crude policy trade-off interpretation was not vindicated by data, occasionally even less than in the US. Interestingly, Gordon (2011, p. 16) shares some of his more speculative suggestions:

I have always thought that the development of the natural rate hypothesis at Chicago, rather than at Harvard or MIT, reflected the deep involvement of several Chicago economists as advisers to several countries in Latin America, where the lack of correlation between inflation and unemployment was obvious.

Hence, as would be expected from the methodological framework outlined at the beginning of this chapter, the natural-rate hypothesis was a departure from the strong claim of a permanent and stable trade-off between price inflation and unemployment towards a more complicated and somewhat looser connection that was more easily reconcilable with the empirical evidence.

It provided a suitable rationale according to which the expansion of the Kennedy and Johnson administrations caused accelerating inflation from 1963 to 1969 by pushing the unemployment

And he adds in a footnote:

Friedman would of course have argued that he was pursuing the same aim. Nonetheless, I cannot but think that, like Keynes, he was more preoccupied with persuading people that his diagnosis was accurate than with polishing its supporting arguments.

rate from 5.5% below its natural rate to 3.5% (Gordon, 2011, p. 17). It was of course also a more flexible theory to explain the stagflation that followed in the 1970s. By introducing the natural rate of unemployment an additional variable to accommodate to the empirical data was gained. Episodes that did not fit well into the broader picture under the earlier theory could now be better explained among others by shifts in the natural rate itself.

However, a mathematical specification of the hypothesis that would render formal testing possible is not obvious and always leaves some room for discretion. Not surprisingly then, an extensive literature has evolved that put different specifications of the natural-rate hypothesis to the test and yielded diverging results. Numerous studies have confirmed the hypothesis for data from different countries using various proxies for the unobservable variables involved (Gordon, 1972; Turnovsky, 1972; Vanderkamp, 1972; Parkin, 1973; Mackay and Hart, 1974; McCallum, 1975, 1976; Parkin et al., 1976; Wachter, 1976; Fields and Noble, 1981). Others have rejected it, using still different data and specifications (Gordon, 1972; Turnovsky and Wachter, 1972; McGuire, 1976; Lahiri, 1977; McNees, 1979).

Fields and Noble (1981), for example, took the following standard specification of the expectations-augmented Phillips curve:

$$(U_t - U_t^*) = \frac{1}{\alpha}(p_t - {}_{t-1}p_t^e) + \epsilon_t, \quad \alpha < 0, \quad (3.3)$$

where U_t denotes the actual unemployment rate and U_t^* the natural rate at period t . The actual rate of price inflation is p_t and ${}_{t-1}p_t^e$ denotes the expected price inflation rate for period t based on information from period $t - 1$. ϵ_t is a white noise error term. Equation 3.3 can be rewritten as:

$$p_t = \alpha(U_t - U_t^*) + {}_{t-1}p_t^e + \epsilon_t^*. \quad (3.4)$$

In this form it clearly implies that the coefficient for expected price inflation is one. Fields and Noble (1981) proceeded to test the null hypothesis that it is indeed one using a t-test. The first problem was to find a proxy for the expected inflation rate. All the above studies that rejected the natural-rate hypothesis have used a series constructed by Joseph Livingston based on survey data that he collected biannually from a group of businessmen and economists since the 1940s. In none of the studies, except for McNees (1979), did the series used extend beyond 1970.

Fields and Noble (1981) used another quarterly series constructed by the University of Michigan Survey Research Center (SRC) that covers the period from 1954 to 1977. Moreover, they sought to overcome the problem of measurement error in the survey data by following

the instrumental variable approach.³⁴ In referring to Kmenta (1971, pp. 307-309), they argued that errors in measurement lead to the estimated coefficient being biased towards zero and thus against the null hypothesis. They constructed the following instrumental variable for the SRC expected price inflation series, denoted p_t^* , to solve the problem.

They assumed that price inflation expectations are formed on the basis of a linear combination of lagged values of actual price inflation rates, p_{t-1}, p_{t-2}, \dots , and money growth rates, m_{t-1}, m_{t-2}, \dots . Lagged values beyond $t-2$ for price inflation rates and $t-1$ for money growth rates were found to be statistically insignificant, so that the following linear regression was implemented:

$$p_t^* = \gamma_0 + \gamma_1 p_{t-1} + \gamma_2 p_{t-2} + \gamma_3 m_{t-1} + \epsilon_t, \quad (3.5)$$

which led to the following estimates for expected price inflation rates used as an instrumental variable in the later regression:

$$\begin{aligned} \hat{p}_t^* &= 0.399 + 0.403p_{t-1} + 0.311p_{t-2} + 0.129m_{t-1}, \\ R^2 &= 0.82, \quad n = 96. \end{aligned} \quad (3.6)$$

Fields and Noble (1981) used the change in the natural logarithm of the US Consumer Price Index for p_t and the change in the natural logarithm of $M1$ for m_t .

Next, a proxy for the natural rate of unemployment, U_t^* , another unobservable variable, had to be chosen. Fields and Noble (1981) used two alternative estimates by Gordon (1978) and Barro (1977), respectively. They then argued that the unemployment rate, U_t , and hence the unemployment gap, $U_t - U_t^*$, would also be subject to sampling error. Instead of using the unemployment gap directly, they therefore decided to use the one period lagged unemployment gap, $U_{t-1} - U_{t-1}^*$, as an instrumental variable.

Using the \hat{p}_t^* and $U_{t-1} - U_{t-1}^*$ as instrumental variables for p_t^* and $U_t - U_t^*$, respectively, and the Gordon (1978) data on the natural rate of unemployment, Fields and Noble (1981) calculated the following estimates for equation 3.4:

$$p_t = \underbrace{-0.101}_{\gamma} - \underbrace{0.342}_{\alpha}(U_t - U_t^*) + \underbrace{1.13}_{\beta} p_t^* + \epsilon_t^*, \quad (3.7)$$

$n = 96,$

³⁴It should be mentioned that McGuire (1976) and Lahiri (1977) did also recognize the problem and applied corrective statistical methods, but Fields and Noble (1981, p. 253) lamented that their series was not long enough as it did not extend beyond 1970.

where they incorporated an intercept, γ , as well as a coefficient for the expected price inflation rate, β . The 95% confidence interval for β lies between 0.95 and 1.31. Hence, the null hypothesis of $\beta = 1$ cannot be rejected on the 5% confidence level. The same held true when the Barro (1977) data on the natural rate of unemployment was used.

Fields and Noble (1981) have thus shown that the coefficient for the expected rate of inflation is not significantly different from one, which is the value implied by their specification of the natural-rate hypothesis. Hence, the estimation is consistent with the hypothesis. However, it is a curious fact that the t-statistic of the parameter for the unemployment gap, $\alpha = -0.342$, was reported to be 1.96. This means that the coefficient that actually represents the inverse relationship between price inflation and unemployment, although estimated to be smaller than zero as stipulated in equation 3.3, is actually not significantly different from zero on the 5% confidence level - a fact that the authors did not even comment on. They also refrained from reporting the R^2 statistic for regression 3.7. It is not clear how these results in total can be interpreted as supporting the natural-rate hypothesis.

When using the Barro (1977) data instead, the coefficient was estimated to be significantly different from zero, but it still goes to show that the results are sensible to more or less arbitrary choices of data manipulation. They are anything but stable. This example illustrates the difficulties involved in rigorously implementing Friedman's instrumentalist methodology. Even if one holds that it is in principle feasible, some caution is needed in practice given the lack of accuracy of available data.

3.3.3 Implications

The above complications notwithstanding, the natural-rate hypothesis has become the theoretical benchmark for the later work on the Phillips curve (Mankiw, 2001, p. 52), including the New Keynesian version discussed in chapter 5. It suggests long-run neutrality of monetary policy, but admits the possibility for short-run improvements in the real economy through expansionary monetary policy. The natural-rate hypothesis was affected by the transition from adaptive to rational expectations, and was incorporated into New Classical economics, which is the subject of the following chapter.

The main take-away from the Monetarist contributions for the analysis of the costs and benefits of active monetary policy and central banking are as follows. The expectations-augmented Phillips curve has to some extent relativized the alleged real economic benefits of expansionary

monetary policy that the large-scale macroeconomic models of the Keynesians had suggested. In particular, the extensive work of Friedman and Schwartz has put an emphasis on the potential benefits of a stable rule, according to which the money supply was to be expanded moderately at a fixed rate every year. It has echoed the older Fisherian argument for price stability. In aiming at the stabilization of the growth rate of a given monetary aggregate, Friedman would advise against any discretionary and active monetary policy, not least because of an openly acknowledged ignorance about the complex dynamics of the economy.

Overall, Friedman assigned a much more passive role to central banks and monetary policy. He did not reject the short-run stimulating effects that monetary expansion can have on the economic system, but he was much more sensitive to the possibility of unintended and negative side-effects. For the most part, however, he merely hinted at the destabilizing impact of fluctuations in the growth rate of the money stock, instead of engaging in a more systematic analysis.

Friedman would not suggest that a central bank controlled fiat standard should be abolished. Only in one of his later articles can we find arguments that correct some of his earlier held positions (Friedman and Schwartz, 1986). He became increasingly critical of the Federal Reserve System. In fact, the late Milton Friedman tended to emphasize the costs incurred by too expansionary monetary policies, that is, by deviations of actual monetary policy from his proposed moderate and constant growth rule. He pointed out, for example, that “a purely fiduciary currency [i.e. a fiat money] reduces the long-run predictability of the price level” (Friedman and Schwartz, 1986, p. 58), which renders economic planning for entrepreneurs more difficult. Additional resources would have to be applied for the provision of hedging facilities against price fluctuations in financial markets.

Interestingly, in his Nobel lecture, Friedman (1977, pp. 459ff.) speculated that the analysis of the Phillips curve would in the future enter a third stage, in which a possible positive link between price inflation and unemployment is analyzed. He did not systematically pursue this suggested line of research that might have uncovered another important cost element of expansionary central bank policies. We will propose a way into this research program in chapter 8 in the second part of the thesis.

Chapter 4

The New Classical Revolution

The history of modern monetary theory started with what is commonly referred to as the Keynesian revolution (Klein, 1950a), a self-declared revolt against classical economic theory. Monetarism as discussed in the previous chapter is often thought of as a corresponding counter-revolution (Friedman, 1970a; Johnson, 1971). There are good reasons to consider these terms exaggerated, but they certainly add an entertainment factor to what many people, even some trained economists, regard as rather dry theoretical verbiage.

The contributions of the New Classical school to monetary economics have themselves been interpreted as a counterrevolution to Keynesian economics (Seidman, 2005, 2007; Snowdon, 2007). Hence, since we might not necessarily have the feeling that two revolutions in modern economic thought already lie behind us, the title of this chapter is chosen more bluntly to indicate another climactic episode.¹ And indeed, the New Classical school exerted a very important influence on econometric modeling and the formal exposition of economic arguments that demands special scrutiny.

Nonetheless, to call it a revolution is again an exaggeration in many ways, which is shown, for example, in Hoover (1984) and Tobin (1981) who refer to New Classical economics as a second type of Monetarism or “monetarism mark II”. Hahn (1980) subsumed New Classical economics into Monetarism as opposed to Keynesianism for accepting the natural-rate hypothesis. Yet, this

¹We do so knowing that Robert Lucas, the main spokesman for this school of thought, would probably not like it. When asked whether he thought about the New Classical contributions as a revolution in modern macroeconomics, he replied:

Sargent once wrote that you can interpret any scientific development as continuous evolution or discontinuous revolution, at your pleasure. For myself, I do not have any romantic associations with the term ‘revolution’. To me, it connotes lying, theft and murder, so I would prefer not to be known as a revolutionary. (Snowdon and Vane, 2005, p. 280)

may strike the reader as an odd distinction given that the natural-rate hypothesis can just as easily be seen as a contribution to the Keynesian framework (De Vroey, 2016b, p. 108), and that it has indeed found its way into New Keynesian economics as we will see in the final chapter of the first part of this thesis. These observations suggest that the four main branches of modern macroeconomic thought are generally more closely connected than is often admitted.

However, it is important to emphasize the differences. We will see that in terms of macroeconomic modeling techniques substantial differences between Monetarism and New Classical economics exist. The Lucas critique, covered in section 4.1, had a significant impact in elaborating on a fundamental problem in econometric policy evaluation, namely the possible shift of model parameters in response to policy changes. Much of the significance and controversy of New Classical economics is due to this critique. It was instrumental in motivating a shift from traditional Keynesian and Monetarist models towards dynamic stochastic general equilibrium (DSGE) models as discussed in section 4.2. One central feature of these models is that they are inspired by Walrasian general equilibrium analysis, and that they are always presented in a strictly formal and mathematical way. Moreover, representative agents are postulated whose expectations formation is *rational* and not *adaptive*.

The influential idea and formalization of *rational expectations* had important theoretical implications for monetary policy in general, and the Phillips curve in particular. In its most radical form, combined with certain auxiliary assumptions, it implies monetary policy ineffectiveness. These issues are discussed along with some empirical qualifications in the closing section 4.3 of this chapter.

4.1 The Lucas Critique

The New Classical revolution is most notably linked to the works of Robert E. Lucas (born 1937), who was trained at the University of Chicago, where he also taught since 1975, and received the Nobel Memorial Prize in economics in 1995.² One of his most important contributions is an essay entitled “Econometric Policy Evaluation: A Critique” (Lucas, 1976) that became known as the Lucas critique.

²Therefore, the New Classical revolution is sometimes referred to as the “Lucasian revolution” (cf. De Vroey, 2016b, pp. 88, 142, 208, 261; or Blinder, 2002 as cited in Snowdon, 2007, p. 557). Moreover, De Vroey (2016b) uses the terms “New Classical economics” and “Lucasian economics” almost interchangeably (see also De Vroey, 2009).

This contribution within the instrumentalist-positivist line of macroeconomics is of particular importance for our study, not only because it was directly targeted towards conventional inflation-unemployment analyses, but also because it allows a more or less direct connection to the causal-realist approach discussed in the second part of the thesis, where we will take it up again. The central question is whether the Lucas Critique should be seen as an *empirical* hypothesis or as an *a priori* proposition. The common position among critics of the Lucas critique, and in fact the more consistent one from the instrumentalist-positivist standpoint, is to regard it as an empirical hypothesis. Yet interestingly, even though the argument in Lucas (1976) is presented without recourse to any empirical analysis, most New Classical and New Keynesian economists, including Lucas himself, who subsequently engaged in DSGE modeling as an alleged solution to the underlying problem, ultimately seem to share this view. However, in the second part of the thesis we will argue that the core problem indicated by the critique is not empirical in nature - and that its interpretation as an empirical hypothesis deprives it of its significance, namely the foretelling of a fundamental limit of econometric analysis.³

We will first dive into Lucas's statement of the problem in section 4.1.1 and then go into a brief discussion of the nature of the critique as either empirical or *a priori* in section 4.1.2.

4.1.1 Content of the Critique

Lucas's critique is one of the most celebrated contributions in modern macroeconomics, although it is based on a rather simple point. It is an elaboration of the fact that empirical relationships between macroeconomic variables are subject to change over time, especially when certain conditions such as policy arrangements change. It is indeed such a trivial insight that it seems absolutely astonishing how it could arouse so much attention.⁴ But then again, it is often the simplest points that, when repeated at the right time and brought into the right form, yield the biggest effects.⁵

Thus, part of the reason why the Lucas critique had such an impact is that it was directly

³Indeed, we will present an elaborated reformulation and an *a priori* justification of the Lucas Critique from a causal-realist point of view in section 6.2, p. 219.

⁴Potential predecessors of this idea exist of course. Oskar Morgenstern (1902-1977), who co-founded the field of game theory together with John von Neumann (1903-1957), or Ludwig von Mises could be named here (Morgenstern, 1928, 1934; von Mises, 1962).

⁵The history of economic thought is full of examples. Walras, Jevons and Menger were of course not the first economists to realize that value is subjective. Yet, their insistence on and development of this insight had an almost unparalleled effect on modern economics. Ludwig von von Mises (2012, [1920]) based his argument against socialism on the simple point that a socialist commonwealth would lack market prices and that economic calculation of costs and receipts would thus be impossible. He thereby sparked the socialist calculation debate.

addressed at the most obvious testimony of the neglect of its underlying insight that could be found in modern macroeconomics at the time, namely, the naive Keynesian interpretation of the Phillips curve and all the implications it drew concerning the alleged benefits of a heavily expansionist monetary policy. It thereby indirectly suggested that some otherwise well respected Keynesian economists had actually overlooked the possibility of parameter shifts. Lucas (1976, p. 19) wrote in the very first paragraph of his essay:

The fact that nominal prices and wages tend to rise more rapidly at the peak of the business cycle than they do in the trough has been well recognized from the time when the cycle was first perceived as a distinct phenomenon. The inference that permanent inflation will therefore induce a permanent economic high is no doubt equally ancient, yet it is only recently that this notion has undergone the mysterious transformation from obvious fallacy to cornerstone of the theory of economic policy.

This rhetorical slap, in the midst of an extended period of stagflation - high unemployment and price inflation at once - did not fall on deaf ears. Lucas correctly argued that this “cornerstone” was not built on advances in economic theory, but rather

[i]t arose, instead, from the younger tradition of the econometric forecasting models, and from the commitment on the part of a large fraction of economists to the use of these models for quantitative policy evaluation. These models have implied the existence of long-run unemployment-inflation trade-offs ever since the “wage-price sectors” were first incorporated and they promise to do so in the future although the “terms” of the trade-off continue to shift. (Lucas, 1976, p. 19)

He identified a conflict between “two rightly respected traditions - theoretical and econometric” (p. 19). On the one hand, theory suggested that money is neutral, in some form or another - on the other hand, empirical studies found relatively strong connections between monetary and real aggregates. Attempts to resolve the conflict could be found on both sides. Econometricians were engaged in ever more detailed studies as computing power and quality of data increased. Theoreticians developed *disequilibrium* analyses and tried to incorporate money illusion to rationalize a connection from nominal to real variables.⁶ Also the natural-rate

⁶These approaches are most notably linked to Don Patinkin (1922-1992), Robert W. Clower (1926-2011), and Axel Leijonhufvud (born 1933). Patinkin (1956) understood *involuntary unemployment* as a phenomenon of the adjustment process towards equilibrium, and not as an equilibrium phenomenon. Leijonhufvud (1968) and Clower

hypothesis discussed in the previous chapter is such an attempt at rationalization. Lucas thought that “it seems [...] appropriate to entertain the possibility that reconciliation along both of these lines will fail, and that one of these traditions is fundamentally in error” (p. 19).

Thus the main thesis of his essay was that the modern econometric tradition, that found its first major application in large-scale Keynesian models discussed in chapter 2,⁷ was in need of major revisions. In particular, the conclusions for monetary policy drawn from them deserved careful examination.

[T]he case for sustained inflation, based entirely on econometric simulations, is attended now with a seriousness it has not commanded for many decades. It may, therefore, be worthwhile to attempt to trace this case back to its foundation, and then to examine again the scientific basis of this foundation itself. (Lucas, 1976, p. 20)

Lucas then proceeded by outlining a schematic version of the theoretical framework that he intended to criticize. In reference to Tinbergen (1952), he called it the “Theory of Economic Policy”.⁸ As we have seen in chapter 2, the traditional models consist of a vector of exogenous variables, x_t , typically including policy variables like the base money stock or prime interest rates, a vector of state variables, y_t , typically including variables like output, unemployment and price inflation, as well as independent and identically distributed random shocks, ϵ_t , connected by some function f :

$$y_{t+1} = f(y_t, x_t, \epsilon_t) \quad (4.1)$$

The form of the function f is usually specified in advance except for a vector of parameters θ so that one can rewrite equation 4.1 as:

(1984, in particular ch. 5 [1967] and ch. 14 [1975]) saw it as a phenomenon that occurs when adjustment towards equilibrium is in some way blocked (see also De Vroey, 2016b, ch. 6, especially p. 117, table 6.1). Barro and Grossman (1971) presented a general disequilibrium model trying to combine contributions of Patinkin and Clower. Later, when the emphasis on disequilibrium became obsolete as involuntary unemployment was generally accepted to be a disequilibrium phenomenon, this line of research was labeled non-Walrasian equilibrium modeling. After all, whether one calls it disequilibrium or equilibrium analysis is not really important as both concepts are inextricably linked. Other noteworthy contributions include Grandmont and Laroque (1976), Grandmont (1977) and Malinvaud (1977) (De Vroey, 2016b, ch. 7). We do not discuss these works in any detail here, because the Walrasian general equilibrium approach that the New Classical adopted was clearly the dominant one, and in fact, by the end of the 1970s important proponents of the disequilibrium/non-Walrasian equilibrium approach have switched over to the New Classical research program. One very prominent example is Robert J. Barro (born 1944).

⁷See again appendix A, pp. 335ff., for a detailed outline of the Klein-Goldberger model as a representative example.

⁸Lucas and Sargent (1979) contains a very similar summary and critique of Keynesian macroeconomic models as the 1976 paper.

$$y_{t+1} = F(y_t, x_t, \epsilon_t, \theta). \quad (4.2)$$

The parameter vector θ has to be estimated on the basis of past data. Once F and θ are given, one can engage in forecasting as well as hypothetical policy analysis by plugging in the currently observed values of y_t and the actual politically determined or hypothetical values of x_t into equation 4.2 to calculate estimates of y_{t+1} . One can do this for several periods ahead and calculate estimates for a whole sequence of $y_{t+1}, y_{t+2}, y_{t+3}, \dots$ based on the observations of period t . In that case, random shocks accumulate and the accuracy of the estimates diminishes. Hence,

one must emphasize the intimate link between short-term forecasting and long-term simulations within this standard framework. The variance of short-term forecasts tends to zero with the variance of ϵ_t ; as the latter becomes small, so also does the variance of estimated behavior of $\{y_t\}$ conditional on hypothetical policies $\{x_t\}$. Thus forecasting accuracy in the short-run implies reliability of long-term policy evaluation. (Lucas, 1976, p. 22)

Now in practice, parameter estimates, then and now, are regularly revised in accordance with past forecasting errors to enhance the predictive performance of the models at least for the short run. This is to say that the underlying structure of the economic model is regularly revised, and hence the “unquestioned success of the forecasters [in the short run] should not be construed as evidence for the soundness or reliability of the structure proposed in that theory” (Lucas, 1976, p. 23). Lucas argued that this also explains the poorer performance of the published versions of the models as compared to their constantly revised counterparts used in practice by policy makers.

The obvious conclusion for Lucas was that the long-run “implications of current forecasting models are without content, and that the short-term forecasting ability of these models provides no evidence of the accuracy to be expected from simulations of hypothetical policy rules” (p. 24). This, as Lucas acknowledged, might only be a transitory point of criticism as the models are further developed and eventually find a stable structure on the basis of which counterfactual policy evaluation becomes possible. However, he believed there was a more fundamental problem:

There is [...] no presumption that (F, θ) will be easy to discover, but it is the central assumption of the theory of economic policy that once they are (approximately)

known, they will remain stable under arbitrary changes in the behavior of the forcing sequence $\{x_t\}$. (p. 25)

For a meaningful comparison of alternative policy rules one must in other words assume that the specified function and its parameters are not affected by the different policy rules themselves but remain constant. “For such comparisons to have any meaning, it is essential that the structure (F, θ) not vary systematically with the choice of $\{x_t\}$ ” (p. 25). Yet, this assumption is not satisfied in Lucas’s view as “[e]verything we know about dynamic economic theory indicates” (p. 25). Economic agents make their supply and demand decisions, on which the structure of the model relies, partly on the basis of expectations of the future values of certain variables. The policies implemented are inextricably linked to these expectations. They can be seen as shocks to the system:

To assume stability of (F, ϵ) under alternative policy rules is thus to assume that agents’ views about the behavior of shocks to the system are invariant under changes in the true behavior of these shocks. Without this extreme assumption, the kinds of policy simulations called for by the theory of economic policy are meaningless. (Lucas, 1976, p. 25)

There are of course also other sources of change, such as technological progress, which are simply impossible to predict, but Lucas thought they are of minor importance and well described by some kind of random walk. Yet, the important and “systematic sources of drift” (p. 26) such as politics itself need to be incorporated into the theoretical framework to avoid large errors.

In order to illuminate these general considerations further Lucas provided three examples related to the aggregate consumption function, taxation and investment demand, as well as the Phillips curve. Only the last one is of immediate interest for us. Lucas argued that because of the above considerations most of the empirical studies that claimed to have disproved the natural-rate hypothesis are irrelevant.⁹ He thus indirectly supported the position held by Friedman and Phelps. In particular, Lucas emphasized the changing role of lagged values of price inflation in providing a good empirical fit. As he argued,

one can imagine situations in which empirical Phillips curves exhibit long lags and situations in which there are no lagged effects. In either case, the “long-run”

⁹In addition to some of the early studies cited in the last section of the previous chapter, one can mention Gordon et al. (1970) as an example of the studies Lucas probably had in mind. Sargent (1971) and Lucas (1972b, 1973) followed a very similar line of argumentation against these types of empirical tests.

output-inflation relationship as calculated or simulated in the conventional way has no bearing on the actual consequences of pursuing a policy of inflation. (Lucas, 1976, p. 39)

The implemented policies will affect expectations about the absolute price level as well as price volatility and hence lead to “parameter drift” and changes in the empirical relationship between price inflation rates, unemployment and output, which will be “describable over the sample period, but unpredictable for all but the very near future.” (p. 39)

Next, Lucas made a very thought-provoking assertion, namely, that a neglect of long-run consequences and a relatively strong emphasis on the short-run effects of policies have emerged as a result:

One response to this situation, seldom defended explicitly today though in implicit form probably dominant at the most “practical” level of economic advice-giving, is simply to dismiss questions of the long-term behavior of the economy under alternative policies and focus instead on obtaining what is viewed as desirable behavior in the next few quarters. The hope is that the changes in θ induced by policy changes will occur slowly, and that conditional forecasting based on tracking models will therefore be roughly accurate for a few periods. (p. 39)

Yet, he added that “[t]his hope is both false and misleading” (p. 39)¹⁰ as shifts in the parameter vector θ can occur instantaneously and with varying degrees of magnitude in response to policy changes. The right way to cope with the problem in Lucas’s view was to endogenously incorporate the systematic shifts in θ into the model.

Hence, he proposed a model framework of the following form:¹¹

$$y_{t+1} = F(y_t, x_t, \epsilon_t, \theta(x_t, \lambda)), \quad (4.3)$$

where parameter vector θ is itself a function of the policy variables x_t as well as another parameter vector λ . The obvious question is on what basis one can argue that the very same

¹⁰Lucas thus suggested that the modeling approach he criticized was associated with short-term analysis, potentially having a blind spot for long-term costs of certain economic policies. This idea feeds perfectly well into the literature on political cycles that explains short-term thinking in politics with the specific incentive structure that politicians face in a democratic system. Another really interesting question, that certainly lies beyond the scope of this thesis, is whether the emergence of the tendencies that Lucas criticized can be explained by a politicization of economics.

¹¹Equation 4.3 does not follow the exact specification that Lucas used but it captures the central idea.

problems of “parameter drift” outlined for θ do not apply for λ as well? Lucas does not provide a satisfactory answer, but the implicit assumption is that there is indeed a way to find a more fundamental, deeper structure that remains constant. The New Classical research program is essentially characterized by the quest for such a structure. However, does this quest fundamentally pose an empirical challenge?

4.1.2 Empirical Hypothesis or *a priori* Proposition

Simply put, the Lucas critique is a warning of potential drifts in model parameters in response to policy changes. When central banks implement inflationary policies in order to exploit a hitherto observed empirical relationship, the relationship itself might shift when economic agents behave differently as a result of the policy change, or any other factor for that matter. The mere possibility of this happening can of course not be ruled out empirically for any model in advance. Nor has it been dismissed *a priori* for any model. For all we know, a fundamental change may always happen in the future.

That some models are in fact vulnerable to the Lucas critique has been shown empirically. As we have seen earlier, large-scale macroeconomic models have systematically underestimated price inflation rates in the late 1960s and early 1970s. However, the broader empirical relevance of the Lucas critique has been evaluated very differently, especially with respect to monetary policy and the Phillips curve.

The early 1980s in the US were commonly considered a time of monetary policy regime change (Feldstein, 1994) as Paul Volcker became chairman of the Federal Reserve Board in 1979, initiating a policy of disinflation, and Ronald Reagan, whose administration endorsed Monetarist policy prescriptions, became president in 1981.¹² This period is therefore regarded as suitable for empirically investigating the relevance of the critique. Two studies that claimed to have found evidence for parameter shifts in Phillips curve relationships during the early 1980s as a result of the policy changes include Cagan and Fellner (1983) and Vroman (1983). Others have found no significant shifts for the same period (Englander and Los, 1983; Perry, 1983). Taylor (1984) adds to this literature by also formally testing whether monetary policy in the form of a response rule has indeed changed. The other papers took the policy change simply for granted as Taylor pointed out. He found evidence for a changing policy rule as well as parameter shifts in the

¹²The Volcker policy of disinflation, that is, reducing and stabilizing the growth rate of the money stock, is sometimes referred to as the “monetarist experiment” which Reagan supported (Niskanen, 1988).

Phillips curve. The data suggested that “under the less accommodative policy” of Volcker “the increase in unemployment caused by inflation [deflation] ‘shocks’ is larger but less prolonged.” Yet, none of these results were “strongly significant statistically” (Taylor, 1984, p. 210).

Blanchard (1984, p. 213) found “no evidence of a major shift in the Phillips curve” and correctly added that this does not imply that the specification he used was indeed a stable one. Since then a voluminous bulk of literature has been published on the empirical role of the Lucas critique. Many studies have gathered evidence against its empirical importance for certain models and specifications,¹³ but rarely have authors directly claimed to have found a genuinely stable or so-called structural relationship.

Lucas and Sargent (1979, p. 56) held that the “question of whether a particular model is structural is an empirical, not theoretical, one.” Elsewhere, in commenting on his famous critique, Lucas noted:

I view “Econometric Policy Evaluation” as a contribution to this important question of the relation of theory to econometric work, but sometimes it is read as though it resolved the issue much more decisively than could ever be the case. The paper stressed the importance of identifying structural parameters that are invariant under the kinds of policy changes one is interested in evaluating; and in all of the paper’s examples, only the parameters describing “tastes” and “technology” were treated as having this property. This presumption seems a sound one to me, but it must be defended on empirical, not logical grounds, and the nature of such a defense presumably would vary with the particular application one has in mind. (Lucas, 1981b, pp. 11-12)

Taken as such an empirical hypothesis,¹⁴ the Lucas critique loses much of its force. After all, it seems reasonable to assume that also earlier macroeconometricians have seen the problem and were precisely searching for preferably stable relationships in order to enhance the quality of predictions as much as possible. This interpretation has even led to some revisionist accounts

¹³Ericsson and Irons (1995) review all articles in the Social Science Citation Index published between 1976 and 1990 that cite Lucas’s paper and find that there is “[v]irtually no evidence demonstrating the empirical applicability of the Lucas critique” (p. 39). Other studies that come to similar conclusions include Favero and Hendry (1992) as well as Estrella and Fuhrer (1999).

¹⁴Interestingly the claim that Lucas’s critique is an empirically testable hypothesis, has been raised already by Gordon (1976) at the very same conference it was presented, as pointed out in Cruccolini (2010, p. 72). Also Fischer (1983, p. 271) lamented that “It is indeed remarkable that the Lucas policy evaluation critique has triumphed without any detailed empirical support beyond Lucas’s accusation that macroeconomic models in the 1960s all predicted too little inflation for the 1970s.”

of the historical importance of the critique. Goutsmedt et al. (2015, p. 7) point out that “the prescriptive side of the Lucas Critique is nothing but a well-known problem in macroeconomic modeling” and argue that it had indeed been dealt with elsewhere, for example, in Frisch’s concept of *autonomous* equations (Frisch, 1934, 1938), which is akin to the idea of structural relationships and has been adopted for example by Haavelmo (1944). Frisch’s autonomous equations would be suitable for policy simulations just like the structural equations that the New Classicals and New Keynesians sought as a solution to the Lucas critique.

The Lucas critique understood as an empirical hypothesis thus represents little more than a call for caution. However, we may give it a twist and ask whether a truly structural set of equations that can be employed for policy evaluation in the sense of Lucas is likely to exist in the first place, and more importantly, whether it is likely to be found by economists. Approaching an answer to these questions is a matter of *a priori* considerations, which we will dive into in part II of the study.¹⁵ Although New Classical economists have not raised these questions directly, they have implicitly assumed an answer in the affirmative and presented a specific approach that seemed adequate to solving the underlying problems of the Lucas critique.

4.2 A New Type of Model for Monetary Economics

In a series of articles starting some years prior to the publication of the famous Lucas critique with Lucas and Rapping (1969a) and running throughout the 1970s and early 1980s a new model framework was successively developed that became known as the dynamic stochastic general equilibrium (DSGE) approach. It is the positive contribution of the New Classical school to macroeconomic modeling in general, and monetary policy analysis in particular.

We will present the key ingredients of the DSGE framework and the underlying ideas in subsection 4.2.1. A brief mathematical compendium to the New Classical version of DSGE models can be found in appendix B. We then go into the implications for monetary policy that have been drawn from the model framework and the typical New Classical auxiliary assumptions of perfect competition and fully flexible prices and wages.

¹⁵See the reformulation of the Lucas critique in section 6.2, p. 219.

4.2.1 The Key Ingredients of DSGE Modeling

The guiding idea for solving the problems indicated by the Lucas critique was to provide a specific kind of *microfoundations* for macroeconomic models. It has been argued that structural relationships that remain (relatively) stable over policy changes can only be derived from individual optimizing behavior. Hence, the macroeconomic relationships estimated in a model should not be postulated *ad-hoc* but must be grounded in microeconomic decision making of the agents involved and their objectives, preferences and expectations.

Microfoundations

The central problem that New Classical economists saw in the traditional Keynesian and Monetarist approaches was their detachment from well established microeconomic theory. In a sense they regarded the division of economics into two more or less separate branches, micro and macro, that the *General Theory* fostered if not initiated, as symptomatic of a fundamental flaw in Keynesian and Monetarist economics. They argued that conclusions drawn on the macro level, monetary theory and policy in particular, run a high risk of being misleading, if micro principles are not adequately taken into account. Indeed, Lucas thought that a unification of economics into one theoretical framework based on a common set of principles was necessary again:

The most interesting recent developments in macroeconomic theory seem to me describable as the reincorporation of aggregative problems such as inflation and the business cycle within the general framework of “microeconomic” theory. If these developments succeed, the term “macroeconomic” will simply disappear from use and the modifier “micro” will become superfluous. We will simply speak, as did Smith, Ricardo, Marshall and Walras, of *economic* theory. (Lucas (1987, pp. 107-108) as cited in Hartley (1997, pp. 120-121))

Kevin D. Hoover has put it more drastically in claiming that New Classical economists sought the “euthanasia of macroeconomics” as their ultimate goal (also cited in Hartley, 1997, p. 121; see also Hoover, 2001b, p. 59). However, the claim that there ideally need to be some microeconomic foundations to macroeconomic models was obviously not original to New Classicals. It was merely a reinforcement of an old classical creed. Moreover, microeconomic criticisms were leveled against the *General Theory* already in the year of its first publication by Leontief (1936) and Viner (1936). According to Hartley (1997, ch. 9) and Hoover (2001b, ch.

3), both economists who followed Keynes in his central ideas and policy conclusions, as well as economists who did not, saw advantages in microfoundations and worked towards them. As Hoover summarizes:

The history of the first twenty-five years of postwar macroeconomics is largely the hanging of microeconomic flesh on the skeleton of interpretation of Keynes's *General Theory* formalized in Hicks's aggregate general-equilibrium, IS/LM model. (Hoover, 2001b, p. 64)¹⁶

Janssen (1991) has a slightly different assessment and identifies the beginning of the search for microfoundations to Keynesian theory in Clower (1965), probably due to a narrower interpretation of the term *microfoundations*. So he argues that Patinkin (1956), for example, still “discussed micro and macro separately” (Janssen, 1991, p. 694) in contrast to Clower (1965) who really synthesized microeconomic and macroeconomic thought. Lucas and his followers regarded all of these contributions as unsatisfactory at best and failed at worst, although they too acknowledged them as essentially being driven by the same spirit, namely, as being attempts “to use microeconomic theory based on the classical postulate that agents act in their own interests to suggest a list of variables that belong on the right side of a given behavioral schedule” (Lucas and Sargent, 1979, p. 54).

Hence, there seems to have been a major difference in the understanding of what microfoundations are. As a matter of fact, it is rather obvious that all economic phenomena, regardless of the level of aggregation, are ultimately the result of individual decisions and actions. This general notion of methodological individualism is virtually undisputed in modern economics. However, the New Classicals had stricter views than the Keynesians about what it meant for economic modeling (Hartley, 1997, pp. 127ff.). For New Classicals it did not merely mean to be able to loosely “explicate and justify” postulated macroeconomic relationships “by reference to microeconomic behavior” (Hahn and Solow, 1995, p. 1), but rather it demanded that any macroeconomic model is derived from well defined individual optimization problems - utility maximization in the case of the consumer and profit maximization in the case of the business firm.

¹⁶In particular, Hoover (2001b, p. 64) continues “James Duesenberry, Milton Friedman, and Franco Modigliani tried to explain the microeconomics of consumption; William Baumol and James Tobin, the demand for money; Dale Jorgenson, investment; Don Patinkin, labor; and so forth.” (Duesenberry, 1949; Baumol, 1952; Modigliani and Brumberg, 1954; Patinkin, 1956; Tobin, 1956, 1958; Friedman, 1957; Jorgenson, 1963)

More precisely, New Classical microfoundations lay out exact mathematical optimization problems from which behavioral equations are derived. The empirically estimated counterparts of these equations then serve as the basis for predicting the impact of different policies on macroeconomic aggregates. In this respect, one can argue that the New Classicals pushed the mathematical reformulation of economic theory in the tradition of Ragnar Frisch and the early modern econometricians even further.¹⁷

The strictly formal and mathematical, modeling-related interpretation of microeconomic foundations of the New Classical school has had a strong impact on the broader discipline of economics¹⁸ as it was inextricably linked to the Lucas critique. It was thought to provide the formal prerequisites to solve the problem of parameter shift in response to policy changes and to find stable structural relationships that could be used for reliable counterfactual policy analysis. Hence, the idea was that underneath the unstable macro surface there would be a stable, or at least a stabler, microeconomic structure to be discovered following their new econometric modeling techniques.

¹⁷The tremendous impact that the ideal of mathematical theorizing has had by the time that Lucas's generation entered the economics profession (the 1960s) is well reflected in a passage from his "Professional Memoir" in which he commented on his studies of Paul Samuelson's celebrated *Foundations of Economic Analysis* (Samuelson, 1947), which was itself an attempt to develop a common mathematical structure underlying most of economic theory based on optimizing behavior of agents and equilibrium analysis:

I loved the *Foundations*. Like so many others in my cohort, I internalized that if I could not formulate a problem in economic theory mathematically, I did not know what I was doing. I came to the position that mathematical analysis is not one of many ways of doing economic theory. It is the only way. Economic theory is mathematical analysis. Everything else is just picture and talk. (Lucas, 2001, p. 9)

There is a difference to be noted between Lucas and his teacher Milton Friedman when it comes to formal modeling. The latter was in a way more skeptical as can be inferred from the following passage about Friedman's lectures and the use of models:

Here "model" is my term: It was not a term that Friedman liked or used. I think that for him talking about modeling would have detracted from the substantive seriousness of the inquiry we were engaged in, would divert us away from the attempt to discover "what can be done" into a merely mathematical exercise. (p. 10)

A couple of pages later Lucas made another, rather humorous comment that reveals his affinity to mathematics as a theoretical economist. When his colleague Edward Prescott one morning wrote him the following note:

Bob,
 this is the way labor markets work:

$$v(s, y, \lambda) = \max\{\lambda, R(s, y) + \min[\lambda, \beta \int v(s', y, \lambda) f(s', s) ds']\}$$

 Ed

he did not understand the meaning of the new formula immediately. Instead of asking Prescott for clarification he thought about the equation in isolation. He commented that "theoretical economists are not normal, and we do not ask for words that 'explain' what equations mean. We ask for equations that explain what words mean" (p. 25).

¹⁸Janssen (1991, p. 688) in referring to the New Classical interpretation pointed out: "As recent macroeconomics has become permeated with microeconomic techniques the term 'microfoundations' seems virtually outdated - it is almost a synonym for 'modern macroeconomics'."

Representative Agents

It goes without saying that a mathematical model including millions or even billions of individual optimization problems would be unworkable. Hence, as is the case in the *ad-hoc* formulation of functional relationships between macroeconomic variables in traditional Keynesian or Monetarist models, aggregation enters DSGE models right from the beginning in the form of a small number of representative agents, typically a *representative firm* for the production side and a *representative household* for the consumption side of the entire economy under consideration. This might be surprising, especially from a point of view of a somewhat more serious interpretation of methodological individualism and microfoundations, such as the one shared by Austrian economists for example (for an overview see Hartley, 1997, ch. 8). In this part of the thesis we shall however consider the concept of the representative agent from within the instrumentalist-positivist approach.

It is not of much interest to the New Classical model builder to know what exactly the theoretical concept of a representative agent actually represents and to what extent. There is in fact very little discussion of the justification for the representative agent as corresponding directly to some real world entity. Yet, the assumption that it would yield a more robust model structure was simply carried on implicitly. There was, for example, virtually no reflection on the older criticisms of Marshall's "representative producer" or Pigou's "equilibrium firm" (Pigou, 1928) that apply equally to the more modern conceptions of the representative agent.¹⁹

¹⁹It was Alfred Marshall who first used the concept of the representative producer or firm in his *Principles of Economics*, in order to study the determinants of the aggregate volume of production in a given sector of the economy. He introduced the concept as follows:

We shall have to analyse carefully the normal cost of producing a commodity, relatively to a given aggregate volume of production; and for this purpose we shall have to study the *expenses of a representative producer* for that aggregate volume. On the one hand we shall not want to select some new producer just struggling into business, who works under many disadvantages, and has to be content for a time with little or no profits, but who is satisfied with the fact that he is establishing a connection and taking the first steps towards building up a successful business; nor on the other hand shall we want to take a firm which by exceptionally long-sustained ability and good fortune has got together a vast business, and huge well-ordered workshops that give it a superiority over almost all its rivals. But our representative firm must be one which has had a fairly long life, and fair success, which is managed with normal ability, and which has normal access to the economies, external and internal, which belong to that aggregate volume of production; account being taken of the class of goods produced, the conditions of marketing them and the economic environment generally. (Marshall, 1890, p. 185)

The obvious question is what is *normal*? From the above quote it seems that it has to be something like an *average* firm - not the most and not the least fortunate, not the biggest, not the smallest. Interestingly, Marshall seemed to have a real world firm in mind that has to be selected as representative or average from the existing firms in the market, not merely a theoretical construction:

[A] representative firm is that particular sort of average firm, at which we need to look in order to see how far the economies, *internal and external*, of production on a large scale have extended generally

These criticisms include first of all the ascertainment that a representative firm, controlled and guided by what might be thought of as a representative managerial skill set, is not necessary to explain a given supply or price in any market, just as we do not need to assume any other representative factor of production for that purpose: “*There is no more need for us to assume a representative firm or representative producer, than there is for us to assume a representative piece of land,[...] a representative machine, or a representative worker*” (Robbins, 1928, p. 393). These concepts are unnecessary because we can explain an equilibrium state or the tendency towards it without reference to these concepts. Robbins continued:

All that is necessary for equilibrium to prevail is that each factor shall get at least as much in one line of production as it could get in any other: as much, of course, including all advantages and disadvantages of work, hiring or investment. (p. 393)

Moreover, by their very conceptual nature, representative entities always abstract from heterogeneity and thus render any type of analysis of distribution among the heterogeneous elements that are supposed to be represented impossible, be it the distribution of revenues, profits, wages, or wealth. The concealing of heterogeneity is in fact the criticism that Hartley (1997, p. 18) regards as the “most devastating” also for modern representative agent models.²⁰ Yet, while these criticisms are important from a certain point of view, one should evaluate theoretical concepts with regard to the question of whether they serve their purpose, as long as the purpose itself is not the primary target of criticism.

Only a few pages earlier Hartley (1997, pp. 15-16) quotes from Schumpeter’s chapter on Marshall in *Ten Great Economists*. Writing on Marshall’s theoretical concepts, Schumpeter stated: “Like old friends, [...], they occasionally prove treacherous. Some of them, such as the representative firm [...], cover rather than mend the logical difficulties we are bound to encounter [...]” (Schumpeter, 2003, pp. 99-100). The word “occasionally”, that Hartley omits in his

in the industry and country in question. We cannot see this by looking at one or two firms taken at random: but we can see it fairly well by selecting, after a broad survey, a firm, whether in private or joint-stock management (or better still, more than one), that represents, to the best of our judgment, this particular average. (p. 185)

But hence, it is obvious that the very purpose of Marshall’s representative firm was to abstract from competitive dynamics on the market and it was thus an attempt at aggregating production in a sector or even a whole economy. And it was indeed criticized, most notably by Robbins (1928), for precisely that reason - for a lack of *microfoundations* one might say. That the representative agent has become the core ingredient for microfoundations in New Classical and New Keynesian models is another indicator of how flexible the notion of microfoundations is.

²⁰See the entire second chapter of his book for a more detailed discussion of the historical origins of Marshall’s concept and its criticism (Hartley, 1997, ch. 2).

quotation, is crucial. In fact, Schumpeter seems to have had a rather nuanced view overall and acknowledged the usefulness of the concept if employed for the right purpose.²¹

So we might speculate, that the older criticisms were not taken into account by the New Classicals who revived the concept, because the purpose of the modern representative agent is not to provide a logical reconstruction or explanation of any specific market phenomenon, but to provide a basis from which to model and predict certain macroeconomic variables. And the predictive accuracy of representative agent models should thus provide the benchmark for evaluation.²²

The very first use of the representative agent in modern macroeconomic modeling is contained in Lucas and Rapping (1969a). The two authors postulate a representative household defined by a utility function dependent on four variables, present and future consumption as well as present and future supply of labor, in order to derive an aggregate labor supply function as the solution to the households utility maximization problem.²³ The household maximizes utility subject to a two-period budget constraint which contains the set of explanatory variables for the labor supply, namely, present and expected future goods prices and money wage rates, as well as nominal interest rates and asset holdings.

The derived aggregate labor supply in man hours of work in some period t is a function of present real wages, expected future real wages, real interest rates, and real asset holdings. However, in their empirical test of the presented model the authors deleted the latter two variables, which facilitated the analysis and led to a better fit. Using adaptive expectations formation on one period lagged values allowed Lucas and Rapping (1969a, p. 732) to rewrite labor supply as a function of current and past wages and prices. They added an aggregate labor demand function derived from a production function with constant elasticity of substitution and the marginal productivity condition for labor. The third and final equation of the model consists of a Phillips curve relationship that was derived from their definition of a *normal labor supply*, the hypothetical labor supply in period t that would actualize when price and wage expectations

²¹In the same paragraph Schumpeter wrote that Marshall's theoretical concepts, including the representative firm, "are such old friends of ours and have become such familiar denizens of our arsenal of analysis that we hardly realize any more what we owe to them" (Schumpeter, 2003, p. 99). In his *History of Economic Analysis* we find the statement that the concept "received neither the criticism nor the development it deserves" (Schumpeter, 2006, p. 963, fn. 14), indicating that it might indeed be useful (see also Schumpeter, 2006, p. 1015).

²²Note that we might still come to the same conclusion as Kirman (1992, p. 119), namely, that the representative agent "deserves a decent burial", if we find that it has not actually served its purpose. We will discuss the empirical success of important elements in state of the art DSGE models in the next chapter.

²³In appendix B that contains a canonical version of the modern New Classical DSGE model an optimization problem over infinitely many periods is postulated instead of only two, present and future.

formed in the previous period $t - 1$ turned out to be correct, as well as the assumption that measured unemployment rates stand in a direct linear relationship to the deviation between normal and actual labor supply. The Phillips curve so derived reads as follows:

$$U_t = \beta_0 - \beta_1 \ln \left(\frac{w_t}{w_{t-1}} \right) - \beta_2 \ln \left(\frac{P_t}{P_{t-1}} \right) + \beta_3 U_{t-1} + u_t, \quad (4.4)$$

where U_t , w_t , and P_t are the unemployment rate, the real wage rate, and the price index in t , respectively, and u_t is a random error term.

Based on assumptions about the underlying utility function of the household, namely, that it is linear and increasing in consumption and decreasing in hours worked, Lucas and Rapping hypothesize that $\beta_1 > 0$ and $\beta_2 > 0$. They successfully tested these hypotheses, alongside a host of other hypotheses on the consistency of their model, on the $\alpha = 5\%$ confidence level and obtained an R^2 of 0.92 for the estimated equation 4.4 using annual US time series data from 1930 to 1965.

These results have been criticized by Hartley (1997, ch. 3) as exaggerating the actual empirical fit of the model, because, as pointed out before, for their conducted empirical tests, real asset holdings and interest rates have simply been deleted from the equations derived in the theoretical part.²⁴

The actual empirical model is thus a mixed bag and not consistently derived from underlying optimization problems. Hence, it is regarded as only a first step towards DSGE modeling. Hartley (1997, p. 23) correctly argued that the procedure followed in Lucas and Rapping (1969a) was “little different than that which Keynesian macroeconomic model builders had been using for decades.” They used some microeconomic considerations to select explanatory variables for macro equations.

But even more importantly, the estimated Phillips curve equation as it stands suggests a permanent trade-off between price inflation and unemployment just like Keynesian models did. However, Lucas and Rapping rejected such a naive reading in their discussion, which was very much in line with the Friedman and Phelps natural-rate hypothesis. They argued that their crude adaptive expectations model would only hold for reasonably stable rates of price increases. They

²⁴He concludes:

So, far from having astounding empirical support for the model they developed, the empirical support is rather mixed. By dropping two of the variables from the regression, Lucas and Rapping were able to make it look as if the empirical support for their model was much better than it actually was. The first new classical representative agent model was thus portrayed as having stronger empirical support than it in fact had. (Hartley, 1997, p. 22)

were not yet able to formally model long-run neutrality. They could thus not implement all the features that they deemed necessary and important into a formal model of price inflation and unemployment trade-offs. Lucas would succeed in that regard only three years later with a model that incorporated the assumption of *rational expectations*.²⁵

Towards Rational Expectations

Before, however, in a follow-up article to the Lucas-Rapping model²⁶ the two authors came already closer to estimating an empirical model with some sort of long-run neutrality by generalizing their expectations formation process into a more complex, yet still adaptive, version. They put the problem of their original model in a nutshell:

[T]he reason [the model] offers a long run trade-off lies in the assumption of an unreasonable stubbornness on the part of households: if a sustained inflation policy is pursued by the government, households following [the simple adaptive expectation scheme] will continue *forever* to underpredict future prices. (Lucas and Rapping, 1969b, p. 344)

By generalizing the expectations formation process to what Jorgenson (1966) called “Rational Distributed Lag Functions”, hence, by simply incorporating more than only the one period lagged values of prices and wages, Lucas and Rapping (1969b) could derive a more general Phillips curve. The authors decided to use the values of up to the third lag “which, after some experimenting, appeared to yield the ‘best’ results” (Lucas and Rapping, 1969a, p. 345). They then estimated the following Phillips curve:

$$U_t = \beta_0 + \beta_1 \ln \left(\frac{P_t}{P_{t-1}} \right) + \beta_2 \ln \left(\frac{P_{t-1}}{P_{t-2}} \right) + \beta_3 \ln \left(\frac{P_{t-2}}{P_{t-3}} \right) + \beta_4 \ln \left(\frac{w_t}{w_{t-1}} \right) + \beta_5 \ln \left(\frac{w_{t-1}}{w_{t-2}} \right) + \beta_6 U_{t-1} + \beta_7 U_{t-2} + u_t. \quad (4.5)$$

²⁵Lucas later reflected on the model that he developed with Leonard Rapping and underlined the role of rational expectations for modeling long-run neutrality satisfactorily:

Friedman’s argument was theoretical, but his premises all seemed to Leonard and me to hold for our model. Yet our model did imply a long-run trade-off. Later, we came to see that this difference was due to our use of adaptive, rather than rational, expectations, but at the time we simply accepted it as an unresolved puzzle.

²⁶It is interesting that the two articles refer to each other. Lucas and Rapping (1969a) refer to Lucas and Rapping (1969b) with the comment “in press” and Lucas and Rapping (1969b) refer to Lucas and Rapping (1969a) as “forthcoming.” It seems that the incentive structure back in the 1960s has already been such that one should never put more content into one paper than is absolutely necessary to get it published in a certain journal. One should rather make it two.

They also added pre-Great-Depression data so that their time series covered a longer period, 1904-1965. They showed that, if separated into three sub-periods, 1904-1929, 1930-1945, and 1946-1965, only for the period covering the Great Depression and World War II could the null hypothesis of no long-run trade-off between price inflation and unemployment, which translates into $H_0 : \beta_1 + \beta_2 + \beta_3 = 0$, be rejected.²⁷

For the other two periods, prior to the Great Depression and after World War II, it could not be rejected. Over the entire sample the hypothesis had to be rejected as well. However, Lucas and Rapping's presentation is suggestive of this empirical result being due to the particular circumstances of the period 1930-1945, which might skew the overall relationship between price inflation and unemployment in favor of a seemingly stable long-run trade-off. This is a vivid illustration of how the particular circumstances of a specific period might lead to unwarranted generalizations, a danger to which empirical research in the instrumentalist-positivist tradition is always exposed, and that lies at the heart of the critique that Lucas would write only a few years later.²⁸

Another point merits emphasis here. The Lucas and Rapping (1969b) follow-up paper shows that long-run neutrality of price inflation can be estimated on the basis of an adaptive expectations model. From an empirical point of view, long-run neutrality is simply an artifact of the available data and the theoretical concept of rational expectations is not necessary to obtain it in an econometric application. In fact, any expectation or prediction must rely on information of the past and is in that very general sense *adaptive* to the past. As Lucas and Sargent (1981, p. xvi) themselves point out in their introduction to the first volume of *Rational Expectations and Econometric Practice*, “[t]he future must be forecast on the basis of the past,” and “[t]he difficulty lies not in postulating forecasts which are [...] functions of history but rather in introducing the coefficients in these [...] functions.” Hence, the rational expectations assumption for the applied economist is simply a device for selecting variables. It helps in a more or less convenient way to decide which lagged values for which time series to incorporate in the relevant equations.

The theoretical breakthrough appeared in Lucas (1972a), which used the assumption of *rational expectations* as introduced by Muth (1961) in a dynamic macroeconomic model for the first time, with the result that the theoretical model implied long-run monetary neutrality.²⁹ In

²⁷Indeed, if the coefficients for price change variables sum up to zero, a sustained and constant price inflation rate would have no impact on the unemployment rate. The stronger null hypothesis of $H_0 : \beta_1 + \beta_2 + \beta_3 = 0$ and $\beta_4 + \beta_5 = 0$, meaning that neither a constant and sustained price nor wage inflation rate have an impact on unemployment, obviously had to be rejected as well for the 16-year period 1930-1945.

²⁸Early drafts of the Lucas critique started to circulate as early as 1973 (De Vroey, 2016b, p. 169).

²⁹Rational Expectations provide another popular element for the dramaturgy of the history of modern macroeco-

that sense Lucas (1972a) can be read as a further theoretical lining of the Friedman and Phelps natural-rate hypothesis. In Lucas's own and personal assessment this paper on "Expectations and the Neutrality of Money" was one of the most important for his academic career: "No one else was doing macroeconomics this way in 1970. The paper made my reputation" (Lucas, 2001, p. 21).

The assumption of rational expectations went against the commonly held suspicion in the 1960s that economists tend to assume too much rationality. Muth (1961) held the contrarian view, at least when it came to expectations formation. His assumption of rational expectations implies that economic agents form accurate expectations on average over the long run and are not systematically fooled. In Muth's words: "expectations, since they are informed predictions of future events, are essentially the same as the predictions of the relevant economic theory" (Muth, 1961, p. 316). Hence, expectations formation should not be formulated *ad-hoc*, as a function that seems at any given point to be reasonable, but should be derived from the relevant economic model itself.

The logic behind this statement is rather intuitive. He argued that if economic theory would lead to systematically better predictions about future events, there would be incentives to profit from them - "by inventory speculation if possible, by operating a firm, or by selling a price forecasting service to the firms" (p. 318). In one form or another the expertise would find its way into the market.³⁰ In other words, the rational expectations hypothesis "states that expectations reflected in market behavior will be optimal forecasts using all available information" (Mishkin, 1983, p. 1).

Provided that the respective model developed is itself taking due account of the available information, the rational expectations assumption, in technical terms, states that the *subjective* probability distribution of a random variable that the representative agent uses to form expectations about the future is no different from the *objective* distribution function that the structure of the model presupposes. One can use weaker formulations that at least allow for the equality between the household's expected values and the model's expected values, as mentioned in

conomic thought. They are often referred to as a revolution. Mishkin (1995, p. 1), for example, states that in "the early 1970s Robert Lucas launched the rational expectations revolution" and that "[e]ver since then macroeconomics has never been the same." This passage is used as the opening quote to the chapter on the emergence of DSGE modeling in De Vroey (2016b, ch. 9), who attributes to Lucas's contributions to macroeconomics "all the trappings of a Kuhnian scientific revolution: a shift in the type of issues addressed, a new conceptual toolbox, new mathematical methods, the rise to power of a new generation of scholars." (p. 151)

³⁰One might feel inclined to ask about the possibility that the predictions of professional economists are systematically worse than those of the entrepreneurs in their respective fields of specialization. Would there be an incentive for the successful business man to enter academia?

appendix B when we derive the equilibrium paths for real economic variables in the canonical baseline version of the New Classical DSGE model. In any case, rational expectations ensure that the representative agent does not form expectations naively and independently of the broader model, but in accordance with it.³¹

One can easily recognize how this would imply a conclusion of a certain kind of monetary neutrality, depending on the strength of the rationality assumption. Do all agents always correctly predict everything, or only on average and only over the long run? Yet, it took some time to incorporate the idea satisfactorily in a mathematical model. Lucas (1972a) was the first successful and most influential attempt. Moreover, the model was built within a Walrasian type of general equilibrium framework, which is why it is today widely regarded as the first DSGE model.

Dynamic Stochastic General Equilibrium

New Classical economists sought to provide a specific type of microeconomic foundations for macroeconomic models by postulating representative agents and their respective intertemporal, that is, dynamic, mathematical optimization problems under rational expectations. Given the optimality conditions of the agents the models are solved by invoking general equilibrium conditions or market clearing. So the output produced by the representative firm corresponds to the consumption of the representative household. The labor supplied by the household is equal to the demand of labor input in the production function of the firm. The coefficients of the equations so derived are then estimated on the basis of historical time series data. Unexplained residuals are interpreted as random stochastic shocks.

DSGE modeling was in many respects divergent from traditional approaches, but it is interesting that Lucas regarded himself as a Keynesian early in his career (Lucas, 2004), whereby Keynesian refers to the applied branch of econometric modeling (De Vroey, 2016b, p. 154) spearheaded by Lawrence Klein.³² And there is indeed, the Lucas critique notwithstanding,

³¹The interested reader should again consider the more detailed discussion and criticisms contained in Gertchev (2007).

³²Even after his celebrated critique of this branch of econometric modeling, he acknowledged and even praised the efforts of the Keynesian model builders and indicated his approval of the goals pursued by this line of research:

The Keynesian macroeconomic models were the first to attain this level of explicitness and empirical accuracy; by doing so, they altered the meaning of the term ‘theory’ to such an extent that the older business cycle theories could not really be viewed as ‘theories’ at all. (Lucas, 1977, p. 11)

Lucas and his collaborators intended to advance this empirical model building tradition. In a later interview, he stated:

common ground between them.

We have argued in the second chapter of this thesis that large-scale Keynesian macroeconomics was “Walrasian” in its attempt to build models encompassing the entire economy. The same holds true to an even stronger extent for New Classical models. They are Walrasian general equilibrium models of the whole economy as opposed to Marshallian partial models favored by Friedman and the Monetarists. For both large-scale Keynesian as well as New Classical models, the objects of analysis are entire economies. Yet, it should be noted here that neither approach can be regarded as Walrasian in a broader sense. Léon Walras was an idealist with very little interest in directly applying his model to the real world. He focused on its mathematical structure and analytical convenience, and in fact attempted to provide a model of his ideal society: “his *idéal social* as a *synthèse de l'utilitarisme et du moralisme*.” (Jaffé, 1980, p. 531).³³

One can undoubtedly find elements of complete detachment from the real world in the New Classical modeling literature that might remind readers of a kind of Walrasian utopia. These however can just as well be interpreted as artifacts of Friedman’s advocated instrumentalism,³⁴

Keynesian models in the 1960s, and this is what excited people like Sargent and me, were operational in the sense that you could quantify the effects of various policy changes by simulating these models. You could find out what would happen if you balanced the budget every year, or if you increased the money supply, or changed fiscal policy. That was what was exciting. They were operational, quantitative models that addressed important policy questions. (Snowdon and Vane, 1998, p. 130)

³³See also the lengthy but extremely revealing quotation from an undated manuscript, probably from Walras’s youth, reproduced by Jaffé (1980) in the appendix. Jaffé argues that Walras never lost his skepticism “of the value of accumulations of descriptive statistics so much in vogue in his day.” (Jaffé, 1980, p. 532) From the quoted passage in his manuscript one can even get the feeling that he wanted to flee the real world by theorizing in isolation. Walras wrote:

Je suis un idéaliste. Je crois que les idées transforment le monde à leurs images et que l'idéal entrevu par un homme pour son siècle s'impose à l'humanité. Je crois que le monde a mis dix-huit siècles à tâcher de réaliser - sans y réussir - l'idéal de Jésus et des premiers apôtres. Je crois que le monde mettra dix-huit ou vingt autres siècles peut-être à essayer sans y mieux réussir de réaliser l'idéal entrevu par les hommes de [17]89 - aperçu plus clairement par nous - éclairé par nos successeurs. Heureux de penser que moi-même j'aurai peut-être répandu la moindre lumière sur ce tableau. - En cela je suis directement au rebours de mon siècle. La vogue est aux faits, à l'observation des faits, à la constatation des faits, à l'érection des faits en lois. Par un jour de tempête la direction politique est tombée aux mains des masses ignorantes. L'art, la science, la philosophie ont été submergées. Nous avons été écrasés par le nombre. Les faits sont les maîtres, l'empirisme couronné règne et gouverne. Nos hommes de l'analyse examinent [l'explosion], attendant que le chaos se répande de proche en proche, pour le décrire avec amour et le glorifier avec quiétude. Quant à moi, je m'y refuse. Que mon idéal soit borné, c'est possible. Il l'est moins toutefois que ne pourrait le faire croire la traduction imparfaite que ma bouche en donne. Quel qu'il soit, je m'y réfugie - c'est mon asyle contre l'envahissement des faits brutaux et si mon siècle m'écrase comme l'univers le roseau de Pascal, il ne m'aura du moins pas fait vivre de sa vie. J'aurai vécu dans le passé et dans l'avenir.

³⁴The New Classical contributions have in part been interpreted as an outgrowth of what is often labeled the “formalist revolution” (Hands, 2009) - another one among the many - that, for example, Kenneth Arrow (1921-2017) and Gérard Debreu (1921-2004) played a significant role in (Arrow and Debreu, 1954; Debreu, 1959). Hands (2009, p. 148) points out that “there are many formalist successors, but new classical macroeconomics and

that is to say, they can be seen as modeling tools that, while being unrealistic as such, are supposed to help building a model that “behaves through time so as to imitate closely the time series behavior of actual economies” (Lucas, 1977, p. 11). Hence, they fit perfectly well into Friedman’s “*as-if* methodology” (Vromen, 2009, p. 261). The emphasis is not on whether the assumptions are realistic but rather on whether they work for replication and prediction of variables and dynamics observed in the real economy.³⁵

It has been argued that the New Classicals did for macroeconomics what Walras had done for microeconomics and there is certainly some truth to it. But all the formal model building that occasionally seems to lack real world relevance was indeed supposed to be applicable to real and practical problems of monetary and fiscal policy.³⁶ Moreover, it was essentially motivated by overcoming a fundamental problem of the older Keynesian models that was seen as an empirical one in nature. Hence, it fits into the general empirical research program initiated by Keynesian macroeconometricians. After all, one, if not the most, influential element in the New Classical literature is the empirical reading of the Lucas critique and the research agenda it set out. New Classical modeling thus ultimately shared the purpose of economic policy evaluation for real world applications.

It is precisely by the postulated interdependency of the representative agents, and the formulation of a closed mathematical system, that one hoped to capture truly structural and stable rational expectations theory are perhaps the clearest examples.” It has been subject to debate whether Friedman’s methodological essay “licensed” the formalist revolution and by extension the breakthrough of New Classical economics or not. Blaug (1994, 2003) and Hutchison (2000) argue that it has. Yet, Mayer (1993), who otherwise shares most of Blaug’s and Hutchison’s views on either Friedman’s message or methodology in general, argues that it has not. Hands (2009) supports the latter view. However, as we have pointed out earlier, formal mathematical abstraction and deduction which is characteristic of the New Classical literature is perfectly compatible, and as such legitimate, under the premise that the realism of assumptions is irrelevant, although it might not be the route that Friedman himself chose to take. Whether or not Lucas was directly influenced by Friedman on questions of methodology, we can find many statements in Lucas’s published writings as well as the Lucas Archives at Duke University that reflect an instrumentalist point of view. On this see De Vroey (2016b, ch. 10) and the numerous quotations assembled therein.

³⁵De Vroey (2016b, p. 178) vividly calls Lucas’s aim of building an artificial model economy that reproduces and predicts important characteristics of the real economy “Lucas’s FORTRAN injunction” and quotes the following passage:

Our task as I see it is to write a FORTRAN program that will accept specific economic policy rules as ‘inputs’ and will generate as ‘output’ statistics describing the operating characteristics of times series we care about, which are predicted to result from these policies. (Lucas, 1981b, p. 288)

³⁶Neo-Walrasians did not necessarily see Lucas as one of their own, because he emphasized the importance of empirical application and testing. See for example David Cass’s interview in Spear and Wright (1998, p. 546) as cited in De Vroey (2016b, p. 193):

Bob [Lucas] was in the Chicago tradition and was very concerned about empirical testing - whatever the hell that means - something that I have little sympathy for and very little interest in, to be perfectly honest.

relationships that incorporate all relevant feedback effects and render the models resistant to the Lucas critique. Whether or not any specific model is successful in that regard remains an empirical question to the New Classics and needs to be tested.

It is true that Lucas (1972a), for example, was an exclusively theoretical modeling exercise. Yet, only shortly after its publication the author delivered an operationalized version of the model and tested some of its hypotheses for time series data from 18 countries over the period 1952-1967 (Lucas, 1973). He found no evidence for a stable long-run Phillips curve trade-off as predicted by the model. Moreover, the short-run trade-off “tends to fade away the more frequently it is used, or abused” (Lucas, 1973, p. 334).

Obviously the economic developments of the day with upcoming stagflation were on his side and delivered evidence in favor of his positions on monetary policy. He later remarked:

In a way the timing couldn't have been better. We were arguing that there was no stable Phillips curve relating unemployment and inflation. You could go either way on that question given the available post-war data up to the early 70s, but by the end of the 70s it was all over. (Snowdon and Vane, 1998, p. 122)

4.2.2 The Big Building Blocks and the Role of Auxiliary Assumptions for Monetary Policy Conclusions

Lucas (1972a, 1973) constitute important contributions on the way to modern state of the art DSGE modeling. They incorporated a variant of the Friedman and Phelps natural-rate hypothesis and strongly rejected any long-run impact of expansionary central bank policies, that is, expansionary stimuli over and above a stable and moderate Friedmanian growth rate of the money supply that allegedly allows the economy to function and evolve smoothly over time. Moreover, the empirical evidence studied in Lucas (1973) suggests that the impact of monetary expansion varied across different countries depending on their history of price movements:

In a stable price country like the United States, then, policies which increase nominal income tend to have a large initial effect on real output, together with a small, positive initial effect on the rate of inflation. Thus the apparent short-term trade-off is favorable, as long as it remains unused. In contrast, in a volatile price country like Argentina, nominal income changes are associated with equal, contemporaneous price movements with no discernible effect on real output. These results are, of

course, inconsistent with the existence of even moderately stable Phillips curves. On the other hand, they follow directly from the view that inflation stimulates real output if, and only if, it succeeds in “fooling” suppliers of labor and goods into thinking relative prices are moving in their favor. (Lucas, 1973, pp. 332-333)

These empirical findings supported the idea underlying the so-called *signal extraction* problem that producers are facing when their selling prices change. How much of it is due to absolute changes in the price level and how much of it is a relative price change, and thus a reflection of changes in consumer preferences? In general, when prices are very volatile signal extraction is supposed to be more difficult and the real effects of price changes are estimated to be lower. Agents in an environment of generally stable prices are more easily “fooled.”

Whether or not representative agents in a theoretical model are fooled by any nominal variation depends on a series of auxiliary assumptions, such as, for example, the way rational expectations formation is specified and which information are supposed to be accessible and known by the agents.³⁷

In its simplest, and indeed strongest form, the rational expectations assumption collapses to a kind of complete and perfect information assumption, which, as in the canonical model framework presented in appendix B, implies that the equilibrium paths of real economic variables, such as output, employment, real wages and real interest rates are completely independent of money and monetary policy. In this baseline setting, that is closely related to Real Business Cycle models, there is no such thing as an optimal monetary policy. Monetary policy has simply no effect on real macroeconomic fluctuations. Money is said to be *superneutral* in the sense of Michaelis (1993). There are however unknowns of a different kind, such as technology shocks, that can generate business cycle fluctuations (Kydland and Prescott, 1982; Prescott, 1986). How a specific model plays out thus depends on these assumptions. The dynamics of a model are not entirely determined by the big building blocks presented in the previous section.

One of the first general equilibrium models of the business cycle that suggested a weaker form of monetary neutrality, among other things by rejecting perfect information and foresight, is contained in Lucas (1975, 1977), which were extensions of Lucas (1972a, 1973). In this business cycle model *unexpected* monetary shocks produce cyclical fluctuations of real output around a trend line. Associated with these fluctuations are procyclical movements in nominal prices,

³⁷We will see in the next chapter that New Keynesian DSGE modeling takes the same model framework and pairs it with a different set of auxiliary assumptions, which allows for drawing Keynesian policy conclusions.

investments as a part of output, and nominal interest rates.

Without directly taking unemployment into consideration Lucas thus derived a type of short-run Phillips curve relationship between price inflation and economic activity measured in hours worked.³⁸ This relationship is not systematically exploitable by policy makers and monetary policies are neutral with respect to the long-run trend of real variables.

The model was not only significant for articulating this more moderate conception of monetary neutrality mathematically. De Vroey (2016b, p. 162) points out that “[t]he mere fact that Lucas was able to construct an equilibrium model of business fluctuations was a feat.” It applied the Walrasian general equilibrium notion to a dynamic problem.³⁹ Of course, if one were to define business cycles as situations in which consumer preferences do not align with the plans of producers as, for example, Austrians do, one could not regard these fluctuations as a meaningful representation of business cycles. The fluctuations produced by the model correspond to a dynamic equilibrium. The representative agents in the model at any point in time behave optimally, given their preferences and available information, and markets clear. Yet, neither the general equilibrium notion as such, nor any of the other key ingredients of DSGE modeling as presented above, do imply any strong policy conclusions. Their importance derives from the methodological considerations discussed at the beginning of the chapter.

We can draw a direct connection between the perceived relevance of the general equilibrium approach and the Lucas critique. Janssen (1991, p. 706) argues that one of the reasons for adopting the general equilibrium approach of the New Classics was its solution to the problem indicated by the Lucas critique: “This problem is solved by employing the GE program, which allows alternative policy regimes to be incorporated in the optimal decision rules of economic agents.” Interestingly, in his more detailed analysis in *Microfoundations: A Critical Inquiry* that incorporates much of the earlier paper, he omits this passage (Janssen, 1993, p. 101). He might have thought it to be too strong a claim after all. Nonetheless Mayer (1995, p. 30) still refers to

³⁸He did not look at how hours are distributed across agents, which would allow for an analysis of unemployment. Indeed, as we have pointed out above a representative agent model as such does abstract from matters of distribution. Again auxiliary assumptions are needed to perform the analysis.

³⁹And it is in that sense that we can say that the New Classical approach is more Walrasian than large-scale Keynesian modeling. Lucas truly integrated a dynamic general equilibrium notion into his models. As pointed out by Michel De Vroey (2016b, p. 162, fn. 13) “[e]ven economists who cannot be suspected of an inclination towards Lucas’s views, such as Leijonhufvud, acknowledged” the importance of Lucas’s model in that respect:

By the early thirties, business cycle theorists had come to realize that use of the equilibrium toolbox could be strictly justified only for stationary and perfect foresight processes. This pretty much excluded business cycles - and there was no other toolbox. Keynes’s new method successfully evaded this dilemma. Lucas’s new method attempts to salve it. (Leijonhufvud, 1983, p. 184)

Janssen (1993) when he argues that microfoundations within a general equilibrium framework are desirable, because “they would also enhance the predictive success of economics in general, since it seems plausible that the underlying relationships are stabler than the observed macro regularities” and hence “help with respect to the Lucas critique.”⁴⁰ Obviously some economists would like to believe that DSGE modeling is indeed a solution to the Lucas critique. As we will see in the next chapter, New Keynesian economists regularly make this claim.⁴¹

However, neither representative agents, rational expectations, nor dynamic general equilibrium as such, but the added auxiliary assumptions, implicit or explicit, that render the concepts mathematically tractable and empirically operational carry most of the weight of the political implications of the model and the respective evaluation of costs and benefits of alternative monetary policies. Together they define a selection of explanatory variables for quantifiable macroeconomic phenomena of interest, but they are in principle as vulnerable to the Lucas critique as any other *ad-hoc* equation in traditional models. Which set of assumptions delivers stable or stabler, and politically relevant relationships remains, within the instrumentalist-positivist research program, subject to continuously ongoing empirical investigation. The set of assumptions employed is thus evolving and dependent on recent historical developments.

The empirical data analyzed is the same for everyone as far as there is no suspicion of systematic biases in data collection and estimation. From the outset it is clear that any econometrician, whether he postulates rational expectations of representative agents or not, could stumble upon the same relationships in the available historical data as a DSGE modeler. It therefore comes as no surprise that underlying assumptions that led to rejecting long-run Phillips curve trade-offs gained popularity throughout the 1970s where unemployment and price inflation rates were positively correlated over an extended period of time.⁴²

⁴⁰In fact, Mayer refers here to page 59 of Janssen’s book, where he does not even write about the Lucas critique.

⁴¹It seems reasonable to assume that the success of DSGE modeling is directly related to the acceptance of this claim. That DSGE modeling enjoyed triumphal procession over the IS-LM alternative is unquestionable:

It has taken time, but we have reached the point at which there are graduate students for whom John Hicks’s IS/LM model is just a dim memory from an undergraduate textbook and whose first lecture in their graduate macroeconomics courses began with a Hamiltonian describing the dynamic optimization problem of what appears to be an individual agent. (Hoover, 2001b, p. 60)

⁴²For a descriptive data analysis for France, Germany, the United Kingdom and the United States see chapter 8 in part II of the thesis.

4.3 Early Debates of Monetary Policy Analysis under DSGE Modeling

The history of monetary policy analysis since the 1970s is essentially a history of the evolution of auxiliary assumptions plugged into the DSGE model framework to accommodate observed empirical time series data and to draw various kinds of theoretical conclusions on the abstract level of mathematical modeling. *Superneutrality* of money under the strongest set of assumptions was an interesting starting point, and served more as an idealized situation to which various elements have to be added. Nobody ever seriously advocated the view that it does not matter at all how monetary policy is conducted. Lucas himself for example regarded Real Business Cycle models as developed by Kydland and Prescott (1982) that completely abstract from any monetary distortions “not as a positive theory suited to all historical time periods but as a normative benchmark providing a good approximation to events when monetary policy is conducted well and a bad approximation when it is not” (Lucas, 2013, p. 371).

In the remainder of the chapter we discuss the main policy conclusions drawn from early, that is, New Classical DSGE models. We will discuss the *monetary policy ineffectiveness proposition* in section 4.3.1 followed by an outline of the *time inconsistency* problem, or the question of whether rules are to be preferred over discretionary monetary policy, in section 4.3.2. We will move on with a discussion of the real costs of disinflation, that is, the costs in terms of output loss and unemployment due to a transition from relatively high inflation rates to lower inflation rates. Next, the idea of central bank independence and its connection to price stability is briefly reviewed in section 4.3.4. Some concluding remarks follow at the end.

4.3.1 Monetary Policy Ineffectiveness

Two early combatants for the New Classical approach were Thomas J. Sargent (born 1943, Nobel laureate of 2011) and Neil Wallace (born 1939), who took up Lucas’s neutrality results and introduced the *policy ineffectiveness proposition*, arguing that systematic monetary policy, following a clearly communicated and well defined rule, has no effect on the evolution of real macroeconomic variables (Sargent and Wallace, 1975, 1976).

Sargent and Wallace (1976) argued against Phelps (1972) and Hall (1976) who suggested that the natural-rate hypothesis, even if accepted, still allows for a systematic exploitation of the short-run Phillips curve through adequate monetary policy rules, particularly rules that implement

countercyclical policy measures. Phelps and Hall thus argued against Friedman's simple k -percent rule for money supply growth, whose optimality in a simple rational expectations model has been demonstrated by Lucas (1972a). Sargent and Wallace (1976) showed that Phelps's and Hall's arguments in favor of more sophisticated monetary policy rules with some feedback effect to other macroeconomic indicators are misguided if rational expectations instead of adaptive expectations are adopted in an otherwise conventional theoretical model.⁴³ In these models the real effects of monetary policy rely entirely on differences between the expected and actual rates of price inflation. However, agents with rational expectations would pay attention to the communicated policy rule and the implications of an announced monetary expansion. Their price inflation expectations would not simply adapt at the point where actual inflation rates have already risen, but immediately at the point in time when the policy rule implies monetary expansion.

In this system, there is no sense in which the authority has the option to conduct countercyclical policy. To exploit the Phillips Curve, it must somehow trick the public. But by virtue of the assumption that expectations are rational, there is no feedback rule that the authority can employ and expect to be able systematically to fool the public. This means that the authority cannot expect to exploit the Phillips Curve even for one period. Thus, combining the natural rate hypothesis with the assumption that expectations are rational transforms the former from a curiosity with perhaps remote policy implications into an hypothesis with immediate and drastic implications about the feasibility of pursuing countercyclical policy. (Sargent and Wallace, 1976, pp. 177-178)

The authors emphasize that countercyclical policy here always means policy in the form of a well defined rule. Since the rules are understood by the public and integrated into expectations formation, only deviations from the established rule, that is unexpected policy measures, have a real impact. Yet, discretionary policies are rejected right from the beginning as being destabilizing. Their conclusion is then that feedback rules à la Phelps (1972) or Hall (1976), or an activist

⁴³Note that the policy conclusion is not due to rational expectations as such, but to rational expectations in an otherwise conventional model. Changing the auxiliary assumptions of the model would change the policy conclusions even under rational expectations. It is thus not merely "by virtue of the assumption that expectations are rational" that the conclusions follow as Sargent and Wallace state in the following quotation. Examples can be found in Fischer (1977) and Phelps and Taylor (1977) as pointed out below.

Keynesian policy in general, are no improvements over a stable growth rule. There is no systematically exploitable Phillips curve, not even in the short-run.

Nonetheless, the authors acknowledge the possibility of there being an optimal rate of expected price inflation and hence an optimal rate of money supply growth. One growth rate might thus be better than another. This, however, is not a matter of countercyclical and stabilization policy. Sargent and Wallace do not pursue this line of reasoning any further.

Their paper draws a sharp line between anticipated and unanticipated monetary policy with only the latter having any real effects. Moreover, these effects exist only over the short run. This policy ineffectiveness proposition has sparked much discussion on the theoretical as well as empirical level. Fischer (1977) and Phelps and Taylor (1977), for example, developed models in response to Sargent and Wallace, which included some form of wage rigidity that implied real effects of systematic monetary policy. This line of research leads us directly to the New Keynesian variant of DSGE models covered in the next chapter.

As far as empirical studies are concerned, one has to mention Barro (1977, 1978) who found empirical evidence in favor of the monetary policy ineffectiveness proposition. Previously, Barro (1976) developed himself a rational expectations model that came to the same theoretical conclusion. Barro (1977) investigated US data on money growth and unemployment for the period 1939 to 1975. He chose the annual growth rate of M_1 according to the *Federal Reserve Bulletin* to quantitatively specify monetary policy. In order to separate the money growth rate into an expected and an unexpected part, Barro regressed the annual money growth rate on two annual lag values of itself, one annual lag value of the unemployment rate, as well as a measure of current federal expenditure. The fitted values of the regression were used as a proxy for expected changes in the money growth rate. The residuals served as a proxy for the unexpected part of the actual money growth rate. He then showed that the current as well as the two annual lagged values of his proxy for unanticipated money growth had “considerable explanatory value for unemployment” (Barro, 1977, p. 114) and that the actual money growth rate had little to no impact, given the unanticipated part.

However, these results are obviously subject to many more or less arbitrary choices on how to construct a proxy for the expected money growth rate. Other empirical studies such as Mishkin (1982) and Gordon (1982a), using different data sets and following different approaches, came to the conclusion that both expected as well as unexpected monetary policy matter for real economic outcomes. Snowdon and Vane (2005, p. 247) conclude in their brief review that “while

the empirical evidence is mixed, it does not appear to support the view that systematic monetary policy has no real effects.” Hence, empirical research kept the doors open for a discussion of optimal rules of monetary policy - a line of research that Sargent and Wallace did not reject either as indicated above by their acknowledgement of the possible existence of an optimal expected price inflation rate.

Given the existence of such a rate, a stable growth rule could only attain it, if there is a fixed relationship between money growth and price inflation, which is arguably not the case. Pursuing an optimal rate of price inflation would thus naturally lead to a more complex policy rule. Yet, Sargent and Wallace, just like Lucas, supported Friedman’s case for the simple growth rule. We recall that this case rested primarily on the confession that we simply do not have enough information, neither to accurately evaluate the current state of the economy, nor to predict the outcome of any discretionary political interventions. This does obviously not boil down to an argument against modern central banks and a flexible fiat money supply *per se*. Quite to the contrary, it is precisely fiat money under central bank control that facilitates the constant expansion of the money supply. The costs of modern central banking from that perspective are the destabilizing effects from unwarranted deviations from a stable growth rule, that is, from discretionary measures in order to exploit a transitory Phillips curve trade-off.

4.3.2 Rules rather than Discretion

A more general case for rules over discretion in monetary policy and beyond has been made by Kydland and Prescott (1977). The authors argue that even under the assumption that there is a “fixed social objective function and policymakers know the timing and magnitude of the effects of their actions,” (Kydland and Prescott, 1977, p. 473) discretionary policies would not lead to the objective function being maximized under a New Classical model framework. By discretionary policies the authors mean the optimal policy choice in each period according to dynamic control theory. This means that in each period, given past decisions and the current state of the economic system, a policy is chosen which maximizes the sum of the value of the current outcome and the discounted value of the end-of-period outcome. Such an approach would be consistent but not optimal overall. “The reason for this apparent paradox is that economic planning is not a game against nature but, rather, a game against rational economic agents” (Kydland and Prescott, 1977, p. 473).

Let us reconstruct their argument step by step for the Phillips curve example they use in the

paper. It is implied in most of what we have discussed already. Suppose a simple Phillips curve relationship of the following form:

$$U_t = \lambda(\pi_t^e - \pi_t) + U^*,$$

where λ is a positive constant describing the short-run trade-off between price inflation and unemployment, π_t and U_t are the price inflation and unemployment rates in period t , respectively, U^* is the natural rate of unemployment, and π_t^e is the expected rate of price inflation for period t . We further assume an objective function that weighs the relative social costs of price inflation and unemployment:

$$S(\pi_t, U_t)$$

This objective function guides discretionary monetary policy, that is, following dynamic control theory, price inflation rates in each period t are set such that:⁴⁴

$$\max_{\pi_t} S(\pi_t, U_t)$$

subject to the Phillips curve postulated above, which includes taking π_t^e as given. The series of optimization problems over all periods leads to what is defined as a *consistent* solution. Now under the assumption that expectations are formed mechanically, or in an *ad-hoc* way upon past prices, control theory would also lead to an *optimal* solution over the entire time horizon under consideration $t = 1, \dots, T$. It would thus also be a solution to:

$$\max_{\pi_1, \dots, \pi_T} S(\pi_1, \dots, \pi_T, U_1, \dots, U_T)$$

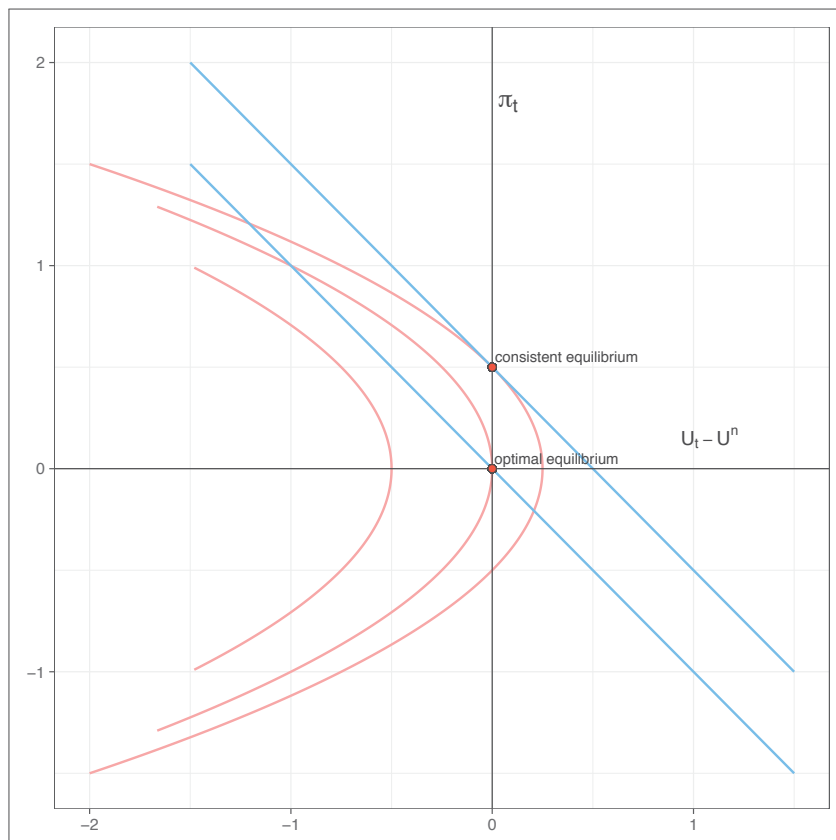
In that case the consistent solution would be optimal. However, under the assumption of rational expectations this is no longer the case. Under rational expectations, agents have “as much information about the economic structure as does the policymaker and some information concerning the implicit objective function which rationalizes policy selections” (Kydland and Prescott, 1977, p. 478),⁴⁵ that is, the objective function S weighing the social costs of price inflation and unemployment. And who would know about it if not the public itself? In that scenario, the public can anticipate future policies, and this influences current policy choices.

⁴⁴The monetary policy authorities know the exact consequences of monetary expansion on price inflation, so that π reduces to a variable that can be chosen at will.

⁴⁵Kydland and Prescott also point out that it is not important that they have perfect knowledge about the objective function. “Just partial predictability of policy is sufficient to invalidate the use of optimal control theory” (p. 478).

The problem is best thought of as a game theoretic situation that can be solved by backwards induction. The public anticipates the attempted “ride of the Phillips curve” by the monetary authorities for the last period T . Expected price inflation rates will be such that the additional social costs of any higher actual rate of inflation would outweigh the gains in employment, and likewise for any lower actual rate the social costs of additional unemployment would be higher than the gains from lower inflation. The authorities thus pick the rate that coincides with the expectations. The solution for period T would be high price inflation and no deviation of the unemployment rate from its natural level, since actual price inflation will coincide with the expected rate. The same reasoning holds for the remaining periods $t = T - 1, \dots, 1$, given the solutions for $t + 1, \dots, T$. In every period there will be relatively high price inflation and the unemployment rate remains at the natural level. Under fairly regular assumptions about the social utility function this yields a sub-optimal solution as illustrated in Figure 4.1.

Figure 4.1: Illustration of the inflation bias



This figure is reproduced from Kydland and Prescott (1977, p. 479). We have indifference curves derived from the objective function $S(\pi_t, U_t)$ under standard assumptions and the short-run Phillips curves as straight lines with slope $-\frac{1}{\lambda}$ shifted by the expected price inflation rate.

The consistent solution according to dynamic control theory has a high price inflation rate and no employment gains, $U_t = U_t^*$. It lies on a lower indifference curve than the optimal solution, which is supposed to have zero inflation, i.e. price stability. There is thus an *inflation bias* and no real effect under the discretionary policy solution. The choice of zero price inflation being optimal is arbitrary and Kydland and Prescott (1977, p. 480) notice that another rate, positive or negative, might be optimal. The reasoning remains the same. Consistent policy planning yields a tendency towards excessive price inflation in the New Classical world.

The optimal policy plan on the other hand is inconsistent. There is, if the optimal plan is credibly implemented, such that the public believes that it will be followed, in any selected period a socially beneficial Phillips curve trade-off that could be exploited in the short-run by deviation from the plan in the direction of higher price inflation.⁴⁶ But this short-run benefit would come either at the costs of adjustment to lower price inflation or a permanently sub-optimal state of too high price inflation in the future. Kydland and Prescott (1977) conclude that central bankers should follow rules rather than discretion, which requires them to abstain from surprise monetary expansion. With the latter one can merely reap short-term benefits in the form of increased economic activity and employment at the expense of long-run costs in the form of permanently higher price inflation or temporary adjustments with increased unemployment and lower economic activity.

The question is of course which rule to follow? What exactly is the optimal plan? In reference to Lucas (1976) the authors hold that there has not yet been a reliably tested and validated theory that shows any sophisticated rule to be superior to a constant growth rule:

The implication of this analysis is that, until we have such a theory, active stabilization may very well be dangerous and it is best that it not be attempted. Reliance

⁴⁶It is interesting to note that this situation boils down to a kind of prisoner's dilemma, which is rather remarkable given that the monetary policy authority tries to maximize public welfare with respect to π_t and U_t . This is presumably in the interest of the public. However, it leads us to the paradoxical situation where the representative household, representing the public as a whole, should really prefer to systematically underestimate price inflation rates. This could be explained by what is commonly called a *rationality trap*. For this argument to be coherent, one must assume, that while correct expectations are obviously beneficial for any agent individually, it is socially sub-optimal if the public as a whole forms correct predictions. In fact, the public must prefer to get fooled. This paradox is blatantly obvious in the following statement by Barro (1986, pp. 24-25) describing the general setup of the Kydland and Prescott model: "Finally, inflation is itself a bad - people value it only as a device to create unexpected inflation and thereby higher levels of economic activity." However, it is not clear how systematically wrong expectations can be beneficial overall. The obvious alternative to this paradox is to assume that the public and the authority are actually playing against each other and have conflicting goals. It thus really is a game *against* the public from the authority's point of view. Who would go that far? Public Choice theorists might. We dive into that line of reasoning in the second part of the thesis, where idealized assumptions about the intentions of policy makers go overboard.

on policies such as a constant growth in the money supply and constant tax rates constitute a safer course of action. (Kydland and Prescott, 1977, p. 487)

Moreover, if such a theory is developed, for “a democratic society, it is probably preferable that selected rules be simple and easily understood, so it is obvious when a policymaker deviates from the policy” (p. 487). In any case, the theory should be used to formulate a credible rule for policy makers that adequately balances short and long-term consequences. They should not have discretionary powers, because it might lead to shortsightedness. It “implies selecting the decision which is best, given the current situation. Such behavior either results in consistent but suboptimal planning or in economic instability” (p. 487).

Kydland and Prescott also emphasized that their argument against discretion is not that policy makers “are stupid or evil” (p. 187). But of course the curious reader might ask: and what if? The answer is simple. A binding constraint on the powers of policy makers in that case, would most likely be better too. One strength of a binding rule, that is not highlighted anymore in most of the rules versus discretion debate that emerged in reaction to Kydland and Prescott (1977), is that it renders the assumption of a benevolent and omniscient social planner less important than under discretion. As Barro (1986, p. 23) points out “one had no reason to deny” discretion, i.e. flexibility, “to a smart benevolent policy maker.” However, the point of Kydland and Prescott’s analysis, as extended for example by Barro and Gordon (1983a,b), is that rules are to be preferred either way, even under the most optimistic of all assumptions, or as Barro (1986, p. 28) writes “actually especially if [...] the policy maker is well-meaning.” In other words,

[a]lthough the low-inflation, rules equilibrium is superior to the high-inflation, discretionary equilibrium, the rules equilibrium is still not ‘first best.’ The benefits from inflation surprises - for example, from lower unemployment or from the generation of distortion-free government revenue - reflect some external effects that have not been eliminated. It is the desire to approach the first-best solution via inflation surprises that threatens the viability of the low-inflation equilibrium. The pursuit of the first-best tends to push the economy away from the second best of a rule with low inflation, and toward the third best of discretionary policy with high inflation. Again, this perspective highlights the importance of the enforcement power that makes a rule sustainable. (Barro, 1986, p. 28)

The above statement also indicates how various considerations may soften up the case for

a stable growth rule and pave the way for a more complicated feedback or contingent rule. There might be beneficial “external effects” such as “the generation of extortion-free government revenue.”⁴⁷ So a rule for monetary policy might be contingent on emergencies to render quick and easy finance of government expenditures possible, for example, during wars. A corresponding model has been developed by Lucas and Stokey (1983) and Persson and Svensson (1984). Barro (1986, p. 28) mentions two historic examples to illustrate the point, Britain during the Napoleonic Wars and the US during the Civil War. Both countries temporarily abandoned the gold standard. Of course one might add to the list many countries that abandoned the gold standard in the course of the two world wars and numerous other examples. Barro (1986, p. 28) argues that this procedure “enables a government to pursue the type of contingent policy for inflation” that he has in mind, and “[i]n this sense a movement off gold during wars is not necessarily a violation of the ‘rules.’” The important thing was the subsequent return to gold after the emergencies were over.

From that perspective the boundaries between discretion and rule get indeed blurred. One might turn the rationale on its head and ask whether it was not rather the discretionary power to go off gold if needed that incentivized aggressive foreign policies in the first place and thus caused the emergencies that later serve as justification for actually abandoning convertibility into gold. Be that as it may, it is important that the events qualifying as emergencies are limited and clearly defined. This way “greater ease of enforcement makes it less likely that the situation will degenerate into a high-inflation, discretionary equilibrium” (Barro, 1986, p. 29).

Accommodating an otherwise simple growth rule to emergencies, not to call it exceptions, can be seen as a first step in the direction of a more complex contingent rule. In fact, Barro (1986) advocates a Fisherian rule targeting a constant price level, or possibly nonzero but stable price inflation if that can be shown to be beneficial. If prices are above target for a certain period of time the monetary base should be expanded more slowly and *vice versa*. Barro (1986, p. 34) notices a major disadvantage of such a rule:

One possible drawback of this scheme is that it severely limits the government’s revenue from printing money. However, it would be possible to permit deviations from the target price level and thereby more revenue from money creation - during

⁴⁷For a critical discussion of various kinds of distortions as a result of inflationary finance and monetary expansion in general see sections 7.2 and 7.3 beginning on page 245. Barro as most other economists in the New Classical or New Keynesian tradition have a rather narrow concept of distortion in mind when they write that inflation can be seen as a distortion-free lump-sum tax.

major wars. This kind of provision parallels the tendency under previous monetary regimes for governments to depart from gold in wartime.

We have here in a nutshell the main benefits of modern central banking under fiat money from a New Classical perspective. The growth of the money base can be adjusted so as to stabilize the price level or a specified rate of inflation that allows for efficient market operations as there would be a “small forecast variance of future price levels.”⁴⁸ “In such a regime the prices of individual commodities would be accurate guides for the allocation of resources” (Barro, 1986, p. 34).

This conclusion constitutes only a slight deviation from Friedman’s constant growth rule, especially given that he thought that it also would generate a roughly stable price level if the rate of expansion was adequately chosen. However, it is a more substantial deviation from the Keynesian position, which held that real economic variables such as employment should be targeted as well, or even exclusively. Although the existence of a short-run surprise Phillips curve trade-off was accepted by the New Classics, they thought a systematic exploitation of it was impossible or at least for the time being not feasible. Central bank authorities should thus focus exclusively on nominal targets like the price level. In that sense the monetary neutrality position that Lucas brought back on the table has asserted itself among the New Classics.⁴⁹

The other main advantage is that fiat money creation allows for an easy and quick generation of government revenue if necessary. In what kind of situations it is deemed necessary needs to be specified and communicated to the public in advance.

The costs of central banking are implicit in the specified rule. Discretionary deviations from it lead to an inflation bias with no sustainable real economic benefits in employment or output and may foster economic instability. They are costs in the form of potential benefits foregone due to more or less violent deviations from the rule that is considered optimal among the practically

⁴⁸Notice, in relation to footnote 46, how more or less accurate predictions of price levels are ultimately, and not surprisingly, seen as beneficial overall.

⁴⁹This again can be seen as merely an elaboration of Friedman’s position, who had already pushed the focus from real economic policy targets towards monetary targets. Lucas himself emphasized several times his affinity to Friedman’s policy conclusions, as for example here:

Now, Friedman and Phelps had no way of foreseeing the inflation of the 1970s, any more than did the rest of us, but the central forecast to which their reasoning led was a conditional one, to the effect that a high-inflation decade should not have less unemployment on average than a low-inflation decade. We got the high-inflation decade, and with it as clear-cut an experimental discrimination as macroeconomics is ever likely to see, and Friedman and Phelps were right. It really is as simple as that. (Lucas, 1981a, p. 560)

feasible. The costs considered are incurred by a misguided, often discretionary, monetary policy not by central banks as such.

4.3.3 Costs of Reducing Inflation and Optimal Money Growth

The 1970s were a period of very high price inflation. In light of the above discussion one can interpret this decade as the consistent but sub-optimal equilibrium outcome of discretionary monetary policy with inflation bias. Two questions for macroeconomics follow immediately. Firstly, how is a transition back to a lower inflation regime optimally managed, or put differently, how is the *sacrifice ratio*, i.e. the real economic costs associated with such a transition, minimized? And secondly, to what rate should monetary expansion be reduced in order to ensure a rate of price inflation that is deemed optimal?

The New Classical position holds that the real costs of a monetary authority's intended reduction of price inflation by lowering the rate of monetary expansion is dependent on the credibility of the intent. If the public thinks an announced reduction of monetary expansion is credible, the sacrifice ratio would be rather low. In the baseline New Classical model it is indeed zero. There would be no problems associated with the adjustment towards lower price inflation if it was expected. The monetary adjustment would be neutral.⁵⁰ However, empirical evidence suggests that the costs of adjustment can be quite substantial. Both the United Kingdom under Thatcher and the United States under Reagan went through economic recessions in the early 1980s when price inflation fell in both countries by more than 10 percentage points and unemployment rates increased by more than 5 per cent in the US and almost 4 per cent in the UK.

One possible explanation from the New Classical perspective is that the public did not believe that the announced policies of the new administrations were actually to be put into place and the recessions were necessary to gain long-term credibility and demonstrate commitment to the new policy plan (Minford et al., 1980; Sargent and Wallace, 1981; Poole, 1988; Sargent, 2013). A

⁵⁰ This is generally accepted for the long run across the different schools discussed so far. The conceptual connection between long-run analysis and rational expectations has been noted among others by Hoover. For the opposite case of monetary expansion he rightly argued that the typical New Classical model really abstracts from any short-run adjustment processes:

Clearly, Lucas's analysis would not allow such gradual convergence to the natural rate. As soon as workers discover their mistake they reduce their labour supply to its equilibrium level. The distinction between the long run and the short run is not very useful in this analysis. The real distinction is between the expected and the unexpected. In effect, rational expectations collapses the short run into the long run. (Hoover, 1988, p. 30)

similar line of argument was taken by Friedman (1984). Indeed, according to Monetarism the sacrifice ratio is dependent on how quickly inflation expectations adapt to the new policy plan. Another explanation are non-monetary policy interventions. The reduction of unemployment and more specifically the reduction of the natural rate of unemployment ultimately remains a matter of microeconomic supply side policy not monetary policy in the New Classical and Monetarist view. In contrast, Keynesians would explain the real costs of the adjustment in terms of higher unemployment with price and wage rigidities of various kinds. The downward adjustment would be costly even if expectations are correct from the outset (Snowdon and Vane, 2005, pp. 247-248).

In the same way that the stagflation of the 1970s lent a powerful empirical illustration of some of the Monetarist and New Classical policy conclusions, the adjustment recessions of the early 1980s went in favor of the Keynesian position that had warned about the real costs of disinflation from the beginning. Some economists referred to the historical experience of the early 1980s as an unsuccessful “Monetarist experiment” (Arestis, 1984; Keegan, 1984; Smith, 1987). It is interesting to note that in the case of the US, M_1 did grow by 10.8% per year on average over five years since 1982 and price inflation still remained substantially lower than in the 1970s. At the same time the real economy recovered. This led Benjamin M. Friedman to argue that the “quantitative relationships connecting income and price movements to the growth of familiar monetary aggregates, including especially the M_1 measure of the money stock that had been the chief focus of monetary policy during 1979–82, utterly fell apart during this period” (Friedman, 1988, p. 51). He calls the monetary policy of the Federal Reserve System from 1982 to 1987 “a distinct success” precisely because it stopped following Monetarist or New Classical prescriptions.

The data thus suggested that stable moderate rates of price inflation or even price stability were compatible or would even require monetary expansion of substantially greater magnitude than the advocated 3% to 5% of the Monetarists. These findings paved the way for further discussion and research on optimal policy rules that culminated in the New Keynesian consensus approach discussed in the next chapter.

4.3.4 Central Bank Independence

Before we proceed to the New Keynesian approach, another debate that evolved around New Classical contributions demands brief consideration. It is closely related to Kydland and Prescott’s

ideas on the inconsistency of optimal plans, the acceptance of the natural-rate hypothesis, as well as the the real costs of disinflation.

Whatever the precise optimal plan may be, the question of the best institutional arrangement for its implementation emerged simultaneously. Under what conditions would central banks be more reliably committed to an optimal rule and resist short-run benefits at long-run costs? This question led to an extensive and still ongoing debate about *central bank independence*.

It has been argued that it would be more likely for independent monetary authorities to reliably commit themselves to the optimal but time-inconsistent rule with lower price inflation, and that politically independent central banks would in general enjoy more credibility among the public. It would therefore also be easier for an independent central bank to minimize the sacrifice ratio during the transition towards lower inflation (Goodhart, 1994a,b). Hence, institutional reforms towards more central bank independence prior to disinflation policies might be beneficial in that regard.

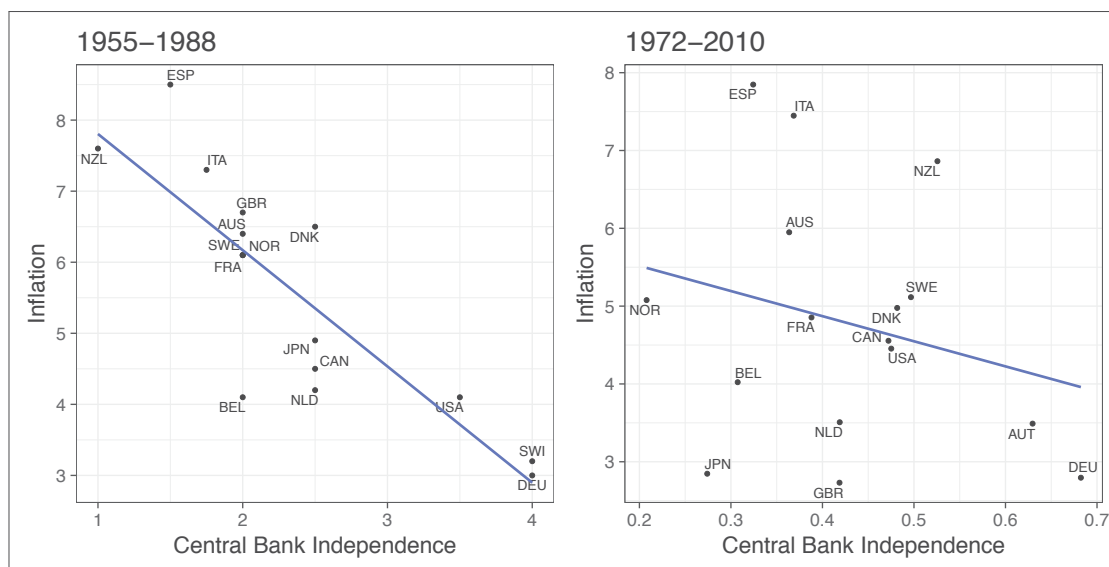
The literature on the political business cycle provides another strong argument for central bank independence. According to this line of research, independent monetary authorities are more likely to prevent the exploitation of discretionary powers for short-term benefits to gain political popularity in advance of elections at the expense of the optimal path for monetary policy over the long-run (Alesina and Roubini, 1992; Alesina et al., 1997).

The prigg in the post war period has long been the German *Bundesbank*, which was widely regarded as one of the most independent central banks with a record of comparatively low inflation (Spinelli and Masciandaro, 1994; Berger, 1997; Tavelli et al., 1998). In general, empirical research has shown a positive correlation between measures of central bank independence and relative price stability (Alesina and Summers, 1993).

The empirical results of Bodea and Hicks (2015) support both a discipline effect, that is, independent central banks have historically engaged in less expansionary monetary policies, as well as a credibility effect, that is, price inflation expectations have been lower among the public in countries with independent central banks. Figure 4.2 provides a very rough overview for a selection of sixteen countries over two periods of more than three decades.

The index computed by Alesina and Summers (1993) is strongly negatively correlated with average price inflation data for the selected countries. The Bravais-Pearson correlation coefficient is -0.84 . The more recent data of Bodea and Hicks (2015) reveals a much lower but still weakly negative correlation of -0.25 .

Figure 4.2: Correlation of indexes for central bank independence and average price inflation for selected countries



The left panel contains the constructed index and price inflation data from Alesina and Summers (1993). The authors use a composite index of the ones constructed by Grilli et al. (1991) and Parkin and Bade (1982). The data was retrieved from Snowdon and Vane (2005, p. 260). The right panel contains a more recent data set compiled by Bodea and Hicks (2015) following Cukierman et al. (1992) in their construction of an index for central bank independence. The data was retrieved from Raymond Hick's website on March 29, 2017: <http://www.princeton.edu/~rhicks/>. In the right panel Switzerland (SWI) is replaced by Austria (AUT) since there is no data on Switzerland in Bodea and Hick's data set. The central banks of both countries are seen as rather independent and both countries have a history of relatively low inflation.

The obvious problem is that central bank independence is a rather vague notion and not a very well-defined concept. What exactly do we mean when we say that a central bank is politically independent? Does it have *goals* independence, that is, can monetary authorities freely choose their policy targets without direct interference from the elected national governments, or do we rather mean *instrument* independence, that is, monetary authorities may freely choose the policy instruments they deem adequate for achieving the goals specified by the government, or maybe both? The latter combination would transform the central bank into a democratically uncontrolled institution that still enjoys the politically granted privilege of money creation. A complete lack of independence on the other hand makes the national central bank a powerful tool of the currently elected government likely to be abused. However, all other things equal, the same powerful tool would be given to politically independent monetary authorities, which can be corrupted just as well.⁵¹

Alesina and Summers (1993) use indexes for the freedom central bank officials enjoy in

⁵¹We will very briefly take up the discussion of central bank independence again in the second part of the thesis, arguing that the very term is ultimately an oxymoron. See section 7.3.3 starting on page 262.

selecting policy objectives without government interference, and the central bank's requirements to finance government deficits. Hence, in that respect their indicator is goal independence oriented. Yet, they do also include proxies to cover other aspects, such as the selection procedure of the central bank's governor as well as restrictions on the choice of policy instruments, reflecting the notion of instrument independence. Bodea and Hicks (2015) construct another, somewhat more detailed, annual index based on the work of Cukierman et al. (1992). It incorporates indicators such as the frequency of change of governors, questionnaires answered by specialists in various countries and an aggregated legal index.

The concept of central bank independence is obviously sufficiently nebulous to leave a wide range of arbitrary choices open to the statistician in constructing a quantitative proxy that tends, depending on the choices made, to yield statistical results in either or no direction. Cukierman et al. (1992, p. 353) come to a somewhat uninspired conclusion when noting that an "inflation-based index of overall central bank independence contributes significantly to explaining cross-country variations in the rate of inflation." It is not surprising that a measure based on inflation is statistically correlated with inflation.

It is then also not surprising that other empirical research has been conducted that suggests a weaker relationship between central bank independence and price stability. Hayo (1998) argues that independence alone does not explain price stability. Instead it might be related to historical experience that created a cultural aversion to inflation in some countries. Germany would be a particularly important example. Likewise, Daunfeldt and de Luna (2008) show that price stability in many countries was achieved prior to central bank reform towards more independence. A survey study by Arnone et al. (2007) shows that the overall evidence is mixed. The relationship between inflation and central bank independence is especially weak in developing countries of Africa, suggesting that the truly important factors on preventing abuse by central bankers are effective checks and transparency of policy measures.

Another series of works has altogether rejected the idea of central bank independence and simple rules-based monetary policy as desirable and beneficial. It has been argued that monetary and fiscal policies need to be coordinated into a coherent mix. If central banks operate independently, conflicting policy combinations can emerge that hamper economic growth (Nordhaus et al., 1994). The early years of the Reagan administration have been interpreted as an example of a mismatch between monetary and fiscal policies. Looser monetary policy should have accompanied the larger government deficits (Blanchard et al., 1987; Modigliani, 1988).

Indeed, the role of other policy measures has been noted by New Classical economists as well. They have emphasized the importance of microeconomic and supply side policies in order to reduce unemployment and achieve sustainable long-run growth - a byproduct of their skepticism towards monetary stabilization policy (Lucas, 2003).

The uncertainty of the future has been invoked as another argument against central bank independence and strict rules for monetary policy. In light of the possibility of large unforeseen shocks it might be very harmful to bind central banks to a limited set of political measures (Solow and Taylor, 1998; Blinder, 1998; Stiglitz, 1998; Tobin, 1998). These are again mainly contributions from the Keynesian side of the debate over monetary policy.

4.3.5 What Remains of the New Classical Contribution

As we have argued above Robert Lucas regarded himself as Keynesian during his youth and was inspired by the large-scale modeling attempts undertaken by Lawrence Klein and others. His own research, however, led him to thoroughly Monetarist Policy conclusions. Later in life, and still during the 1990s he regarded himself as a Monetarist (Snowdon and Vane, 2005, p. 222). This separation from Keynesian policy conclusions and most likely Lucas's own very determined criticism of Keynesian economics have induced some backfire.

Many rather orthodox Keynesians have always been very skeptical about the formal New Keynesian modeling approach as an alternative to large-scale macroeconometrics. They have criticized New Classical models for being ivory-tower creations inadequate and useless for practical purposes in economic policy, which is ironic, given that the emergence and breakthrough of these new models was embedded in the Lucas critique which claimed that old Keynesian models were inadequate for policy evaluation. Now it is true that we can find many purely abstract and formal mathematical modeling exercises without immediate practical or empirical applications in the New Classical literature, but we have tried to show in this chapter that Lucas himself would regard these elements as irrelevant for economic policy conclusions. He has emphasized the role of empirical testing and engaged himself in a whole series of empirical works to form and underpin his policy conclusions. He ultimately regarded his own celebrated critique as an empirical hypothesis, even though he did not believe it to contain merely "transitory debating points" (Lucas, 1976, p. 24) easily settled by a closer look at the data. He was convinced that a solution to the problem indicated, if at all possible, must be grounded in rigorous empirical investigations and he regarded the microfoundational modeling approach as a more promising

basis in this endeavor.

Despite the shared goals, some Keynesians, possibly for some of its more idle abstract elements, have seen the New Classical alternative as a “defective product” that had no effect on actual economic policy. It was as neutral as it made money to be, so to speak. One particularly striking account is the following:

Now a question naturally arises to any economist: If new classical macroeconomics was really a defective product, how could it prevail in the market place? The answer is that it did *not* prevail. Customers in government and the business community never “bought” it. The Federal Reserve Board under Volcker and Greenspan has remained thoroughly Keynesian, continuously practicing countercyclical monetary policy to combat both inflation and recession. The business community takes it for granted that the Fed will practice such countercyclical policy, and there would be widespread alarm if a new classical economist became chairman of the Fed. The undergraduate textbooks that dominate the market continue to use a Keynesian framework based on the assumption of sticky wages and prices and give a sympathetic treatment of countercyclical monetary policy and automatic fiscal stabilizers. When recession hit the U.S. in 2001, the public expected the Fed to cut interest rates, and even expected Congress and the president to provide fiscal stimulus (like the \$600 rebate). The fact is that new classical economics failed in the marketplace. (Seidman, 2007, p. 138)

Seidman holds that the only place where New Classical economics has succeeded is in academia, because academia “welcomes flights of the imagination” and “[u]ndergraduates (and their tuition-paying parents) tolerated new classical economics professors just as they tolerate literature and mathematics professors.”

The sweeping freshness of this remarkable statement notwithstanding, it is half wrong. The general model framework as developed by New Classical economists has passed the test. It has merged with Keynesian elements, such as sticky wages and prices as well as monopolistic competition, and formed a new consensus approach that is widely used in and outside of academia. A substantial and indeed dominant group of economists in the Keynesian tradition, in a large sense, adopted the DSGE modeling approach and showed how its big building blocks, microfoundations, rational expectations, and general equilibrium, are compatible with Keynesian policy conclusions. This is the topic of the following chapter.

4.3.6 Implications

On the previous pages we have seen that there are very few, if any, substantive differences between New Classical and Monetarist policy conclusions. The implications for the cost-benefit analysis of central banking are thus also very similar. New Classical economists have made a somewhat stronger case for monetary policy rules, specifically for a constant growth rule, with their discussion of the time inconsistency of optimal plans. The benefit of a central bank controlled fiat money from their perspective is that it facilitates the implementation of such a constant growth rule, and *a fortiori* of any more complex rule, if a persuasive argument for its superiority can be made. At the same time they have recognized the danger of deviations from established rules and in particular warned about the inflation bias of central banks.

The costs of central banking and active monetary policy are again tied to deviations from the optimal policy plan. New Classical economists argue that there is a temptation for monetary policy authorities to deviate from the rule that is tentatively deemed optimal, precisely because it is possible to reap short-term real benefits in terms of increased output and employment from unexpected expansionary policies. The idea of the short-run Phillips curve is thus not rejected. Its sustainable exploitability for public policy, however, is further questioned. Whatever the optimal rate of price inflation, a surprise deviation in favor of higher rates either comes at future adjustment costs back towards lower rates, or permanent sub-optimally high rates of inflation.

Given these considerations, it is indeed surprising, that we cannot find within the New Classical literature a serious recommendation for a return to a strict commodity standard and the abolition of fiat money and modern central banks as we know them. Such an institutional change would constitute one of the strongest commitments to lower price inflation imaginable. However, it seems as if the mere possibility of a more efficient policy rule that would be feasible only under a fiat standard was considered worth accepting the danger of deviations from it.

Chapter 5

New Keynesian DSGE Modeling

In the previous chapters we have covered the traditional large-scale Keynesian approach to monetary policy analysis, as well as the Monetarist and New Classical alternatives. While there certainly are between these schools varying degrees of rivalry and antagonism, we have stressed the common ground and their interrelatedness with respect to modes of expression, that is, a clear trend towards formal mathematical exposition, and to methodological justification of their respective models, that is, some form of quantitative empirical testing of hypotheses drawn from the models. This emphasis will also permeate this last chapter under the umbrella of instrumentalist-positivist approaches to monetary policy analysis. This is not to deny that there has been a shift from *econometric testing* towards a technique that is commonly referred to as *calibration* in the course of the breakthrough of DSGE modeling. In terms of policy analysis, New Keynesian DSGE modeling in a way leads us back to where we started, i.e. Keynesian policy conclusions, albeit in a new garment.¹

There are nonetheless important internal points of disagreement in the common tradition we sketch here. The contributions of the New Classicals in contrast to those of the Monetarists are commonly regarded as containing a much stronger and more important critique of the early Keynesians. Especially Lucas's methodological critique that was specifically directed at large-scale models of the 1960s has attracted much attention. Monetarists, while very close to the New Classicals and opposed to the Keynesians in terms of policy conclusions, had no such fundamental points of criticism (Laidler, 1986). A significant number of up-and-coming economists in the

¹We wish to emphasize once more that the attribute "Keynesian" has been used in very different ways, sometimes incompatible, and some economists have in fact argued that "New Keynesian" is not "Keynesian" at all, neither when it comes to theoretical building blocks nor practical policy conclusions (Davidson, 1992). However, the terminology has already been set by the dominant literature and New Keynesian models are Keynesian at least in the admittedly loose sense that they represent and promote a return to a more activist central bank policy.

Keynesian tradition has thus taken the New Classical critique very seriously² and adopted the framework of DSGE models developed as an alternative to large-scale macroeconometrics for their own research. They have shown that many of the New Classical policy conclusions are not inherent in DSGE models as such. Endowed with a different set of auxiliary assumptions and specifications they allow for Keynesian policy conclusions, can rehabilitate an exploitable version of the Phillips curve, and reproduce outcomes that more closely resemble empirical observations in certain respects.

New Keynesian DSGE modeling was the result. It has developed into a near consensus approach among modern macroeconomists in recent years. The merger of the New Classical model framework with thoroughly Keynesian elements has been labeled *New Neoclassical Synthesis* (Goodfriend and King, 1997). Moreover, Monetarist influences have been absorbed into this approach as well, most notably in the form of the natural-rate hypothesis (Mayer, 1997). We will review this synthesis in the first section of this chapter.

In section 5.2 we will then present the features of a standard New Keynesian model as well as the typical policy rules derived from it, both on completely idealized theoretical grounds, as well as for practical policy applications. A mathematical compendium can be found in appendix C. In the last section of the chapter we discuss the New Keynesian version of the Phillips curve and its particularities. The implications are again summarized at the end.

5.1 The New Neoclassical Synthesis

In the 1980s, one of the leading Keynesian economists of the 20th century, James Tobin (1918-2002), Nobel laureate of 1981, declared after a decade of decline for Keynesian ideas, that Keynesian economics was not dead and that it “has a future because it is essential to the explanation and understanding of a host of observations and experiences past and present, that alternative macroeconomic approaches do not illuminate” (Tobin, 1986, p. 354).

Some ten years later Goodfriend and King (1997) baptize the *New Neoclassical Synthesis* in which Keynesian elements play indeed a central role:

The New Neoclassical Synthesis inherits the spirit of the old, in that it combines Keynesian and classical elements. Methodologically, the new synthesis involves the

²For example the articles gathered in Mankiw and Romer (1991) mostly contain reactions to the Lucas critique from a Keynesian perspective.

systematic application of intertemporal optimization and rational expectations as stressed by Robert Lucas. In the synthesis, these ideas are applied to the pricing and output decisions at the heart of Keynesian models, new and old, as well as to the consumption, investment, and factor supply decisions that are at the heart of classical and RBC models. Moreover, the new synthesis also embodies the insights of monetarists, such as Milton Friedman and Karl Brunner, regarding the theory and practice of monetary policy. (Goodfriend and King, 1997)

It is thus a synthesis of elements from the three previously discussed approaches. The most important Monetarist element, that was also adopted in New Classical models, is the natural-rate hypothesis. Among others Alan Blinder argues that Friedman and Phelps's hypothesis has found its way into the works of Keynesian economists from the late 1970s onward, and that on the basis of the natural-rate hypothesis in combination with real economic shocks such as oil price shocks a thoroughly Keynesian reinterpretation of the developments of the 1970s was possible (Blinder, 1986, 1988).

More narrowly as outlined in the article of Goodfriend and King (1997, p. 255) the synthesis consists of the equipment of Real Business Cycle models as part of New Classical economics with New Keynesian assumptions of monopolistic competition as well as wage and price rigidity. We will in turn go over these two elements.

5.1.1 Real Business Cycle Models as Blueprint

It may seem surprising that Real Business Cycle (RBC) models that explicitly abstract from monetary distortions became the blueprint for the contemporary consensus models of monetary policy analysis, but a moment's reflection shows that it is not a far cry. For one thing, RBC models provided the most rigorous mathematical formulation of a closed macroeconomic system, an artificial economy in the Lucasian sense, based on intertemporal utility maximization by representative agents and profit maximization by representative firms. Their DSGE framework was regarded as the most promising in dealing with the issues raised by Lucas in his celebrated critique, as it is supposed to incorporate all relevant feedback effects of any policy change on the optimal behavior of agents and firms. Plosser (1989, p. 67) for example pointed out that "[t]hese models provide an artificial laboratory for answering questions regarding policy changes that is not subject to the criticism of Lucas (1976)." Lucas (1987) himself applauded the models for

their methodology but regarded the focus on non-monetary factors as a mistake and advocated a hybrid approach, later effectively adopted by New Keynesians.

RBC theory is an attempt to quantitatively model macro dynamics in an economy where monetary institutions and policy do not add any relevant shocks of their own. This can be regarded as a direct implication of accepting a rather strong monetary neutrality hypothesis as put forth in early New Classical works. Yet, it may also be seen as merely a first step towards analyzing the actual problems involved in monetary policy, namely, in answering the more basic question of how an economy works without it, or more precisely, how a simplified economy without any monetary frictions works. The potential of monetary policy to optimize the workings of the real economy both with respect to long-term economic growth and short-term fluctuations can then be tackled as a next step.

In fact, one important element of RBC modeling is the integration of neoclassical growth theory and the theory of fluctuations around a trend, whereby the dichotomy of short run and long run was effectively broken down. Real factors, mainly technological developments, determine the long-term trend or growth path as well as short-lived fluctuations, because there are no frictions or nominal rigidities and the economy is modeled as being constantly in a state of a perfect competition equilibrium. More precisely, technology shocks as operationalized for empirical work by Solow residuals³ or by some random process for simulation exercises account for the business cycle, which is in turn within the scope of the model interpreted as an efficient adjustment to these underlying shocks.

Here, another indirect link to monetary theory and policy is given by the mere fact that an alternative model and interpretation of business cycle fluctuations was provided. When productivity shocks⁴ are capable of producing cyclical movements that closely resemble empirically observed business cycles like in Prescott (1986) and Plosser (1989), monetary factors must no longer be seen as their most important or even exclusive cause. Moreover, if economic fluctuations and business cycles are largely seen as natural responses of the perfect competition and frictionless economy to real changes, and are thus efficient phenomena, no stabilization policies would be required to counterbalance them.⁵

³Solow residuals are the unexplained parts of the standard neoclassical growth model, that is, the part of output growth that cannot be accounted for by mere increases in invested capital and labor. They are interpreted as technology or innovation shocks and go back to Solow (1957).

⁴These shocks are sometimes referred to as “supply shocks” and RBC models are usually built on supply side factors. However, the link between supply side analysis and RBC models is not necessary. Plosser (1989, p. 57) makes the interesting remark that RBC models could just as well be built on changes in preferences and tastes and hence on “demand shocks.”

⁵Compare again the highly stylized classical baseline DSGE model in appendix B, where no optimal monetary

A last important feature of DSGE modeling concerns the evaluation of the models. There has been a shift from conventional *econometric testing* to *calibration* or *computational experiments*, again first applied in Kydland and Prescott (1982). The idea is that the parameters within the developed mathematical model are not estimated in regression analyses and kept or discarded based on statistical significance, but they are specified based on preexisting microeconomic evidence and stylized macroeconomic facts. In cases where no evidence is available, parameter values are chosen according to theoretical considerations or simply in a way that yield model outcomes that closely resemble the observed macroeconomic phenomena of interest. A model so calibrated can be used to run computer simulations in order to evaluate the effects of different policies within the model economy. Hoover (1995, p. 29) describes the difference between econometric testing, which he calls the competitive strategy, and calibration, which he calls the adaptive strategy, as follows:

To throw the difference into high relief, one can think of estimators pursuing a competitive strategy and calibrators pursuing an adaptive strategy. Under the competitive strategy, theory proposes, estimation and testing disposes. In fine, alternative theories compete with one another for the support of the data. The adaptive strategy begins with an unrealistic model, in the sense of one that is an idealized and simplified product of the core theory. It sees how much mileage it can get out of that model. Only then does it add any complicating and more realistic feature. Unlike the competitive strategy, the aim is never to test and possibly reject the core theory, but to construct models that reproduce the economy more and more closely within the strict limits of the basic theory.

Hoover (1995, p. 29) holds that this constitutes a “genuine difference” and that the “dominance of theory in the choice of models lies at the heart of the difference between estimators and calibrators.” He argues that “the real-business-cycle modeller typically does not regard the core theory at risk in principle.” While, this may be true or not, hardly any economic theory postulates exact values for essentially any of the parameters involved in DSGE models *a priori*. They are determined almost entirely on the basis of observed data and they determine, to a large extent, the dynamics of the model, the simulation results, and hence the policy prescriptions drawn from them. Moreover, while any individual RBC modeler might not actually try to empirically test his policy exists. This radical conclusion was of course vehemently rejected in the New Keynesian adaptation of the model framework.

underlying New Classical theory, it is on a broader scale, the empirical performance of the resulting model, at least with respect to certain dimensions of interest, that determines its acceptance or rejection for policy applications. Hence, the shift from econometric testing to calibration does not qualify as a fundamental methodological change from our point of view. DSGE modeling and the practice of calibration fall under the category of instrumentalist-positivist approaches to monetary policy analysis as understood here.

In fact, it were primarily empirical criticisms that led to a systematic adaptation of the RBC framework (Hansen and Heckman, 1996; Sims, 1996; Danthine and Donaldson, 2001; Christiano et al., 2011) and to the New Neoclassical Synthesis itself. The perspective of monetary policy ineffectiveness or monetary neutrality is reversed when DSGE models allow for imperfect competition and frictions of various kinds. These are arguably more realistic assumptions, but the important question is whether or not they render the predictions and simulations of the models more reliable for policy advice.

5.1.2 The New Keynesian Toppings

Greenwald and Stiglitz (1987) argue that New Classical economics is the attempt to adapt macroeconomic theory to neoclassical micro theory. New Keynesian economics on the other hand is the attempt to cure the schizophrenia by adapting the micro theory to macroeconomics (Snowdon and Vane, 2005, p. 21). This is a very keen observation. While New Classics started from the idealized conditions of neoclassical micro theory and generalized the respective consequences that follow from them to the macro level, New Keynesians systematically altered the assumptions on the micro level in order to derive the results that Keynesian macroeconomics postulates.

Already before, and then for some time simultaneously to, the development of RBC models, New Keynesian models were built to cope with the Lucas critique and rehabilitate Keynesian policy conclusions based on microfoundations. A common element in all of them is monetary non-neutrality due to market imperfections. Within the confines of the models, these lead to real and systematic effects of monetary policy. In that sense, the Monetarist perspective on monetary policy, which holds that there are some real short-run effects, has been internalized and pushed to its ultimate conclusion by New Keynesian economics. The divide between Monetarist and Keynesian views at that point was perceived as relatively narrow. Indeed, Mankiw (1992, p. 22) argues that

[t]here are [only] two schools of thought: new classical and new Keynesian economics. Monetarists now are members of the new Keynesian family, which shows how much the debate has changed. The distance between the new classical and new Keynesian schools is so large that it makes the monetarist-Keynesian debate of the 1960s look like sibling rivalry. (as quoted in De Vroey (2016b, p. 227))

New Keynesian models have nonetheless at a certain point also internalized the general equilibrium character of New Classical RBC models. However, first generation New Keynesian models, as they are often referred to (Goodfriend and King, 1997, p. 246; De Vroey, 2016b, ch. 13) were usually disequilibrium models, since their solutions often do not require market clearing conditions, and they were typically partial and not general, in focusing on specific parts of the economy, most notably the labor market, to demonstrate the existence of *involuntary unemployment*. They can be separated into four different groups.

The first group are implicit contract models as introduced by Baily (1974), Gordon (1974) and Azariadis (1975). They paved the way for the broader research area of modern contract theory. These models demonstrate the possibility of involuntary unemployment as mutually beneficial quantity adjustments, instead of wage adjustments, during economic recessions. The possibility of layoff for the workers can be interpreted as an insurance premium in exchange for an otherwise stable wage rate. Workers are risk averse and insure against fluctuating wage rates in the spot market for labor with implicit long-term contracts, or put differently, with an “invisible handshake” (Okun, 1980). The randomly selected fraction of workers that actually has to be laid-off during a downturn can be regarded as involuntarily unemployed (Azariadis, 1987).

Second, as a direct answer to the policy ineffectiveness hypothesis, as pointed out in the previous chapter, Fischer (1977), Phelps and Taylor (1977), and Taylor (1979) developed staggered wage-setting models. In these models wages are, for whatever reason, set for more than one period and are not adjusted continuously. Under this assumption, even if the rational expectations hypothesis is accepted, the policy ineffectiveness hypothesis can be invalidated as expansionary monetary policy can diminish relative labor costs over the short term.

Efficiency wage models form the third group (Salop, 1979; Weiss, 1980; Stiglitz and Shapiro, 1984). They try to demonstrate the existence of involuntary unemployment due to above market clearing wage rates as an efficient solution to information asymmetry in labor markets. For instance, when the employers’ ability to supervise the employees’ work effort is limited, Stiglitz and Shapiro (1984) argue that a wage rate above the market clearing level may prevent employees

from shirking and increases their productivity due to higher costs of potential job loss. Other common explanations for paying above market clearing wages include lower turnover costs, improvement in the quality of job applicants, and improved morale (Yellen, 1984).

De Vroey (2016b, p. 232) makes an interesting observation in pointing out that it is quite surprising that these types of models are commonly regarded as contributions to Keynesian macroeconomics, because unemployment in these models is actually a solution to a problem (like shirking and information asymmetry) instead of the problem that needs a solution, which would be the standard Keynesian perspective. The same can of course be said about implicit contract models, as well as staggered wage-setting models if one considers that the rigidity of wages may itself be a function of the preferences of employers and employees as explained for example by implicit contracts. Be that as it may, efficiency wage models provide another rationale for downward wage rigidity and hence monetary policy interventions to alleviate unemployment.

Lastly, menu cost models as presented in Akerlof and Yellen (1985a,b), Mankiw (1985), and later Blanchard and Kiyotaki (1987) as well as Ball and Romer (1990) provide another, in a sense, more general counter-argument against monetary neutrality. They postulate price rigidity on the basis of potential costs incurred by changing prices, for example, a disproportional loss in consumer satisfaction. Under these assumptions an expansionary shock can diminish the level of involuntary unemployment. Some firms, instead of adjusting their prices upwards, will demand more labor at the same wage rate in response to increased demand for their goods.

Note that in the last group of models the rigidity assumption has shifted from wages to other prices. This, according to Goodfriend and King (1997, p. 249), marks the transition from first generation to second generation New Keynesian models. Especially in combination with monopolistic competition, under which firms are price setters as opposed to price takers and hence price setting can be explicitly modeled, price rigidity has found its place in the New Neoclassical Synthesis and modern DSGE models. The most common specification of price rigidity used is the one introduced by Calvo (1983).⁶ As Goodfriend and King (1997, p. 255) put it:

The New Neoclassical Synthesis is defined by two central elements. Building on new classical macroeconomics and RBC analysis, it incorporates intertemporal optimization and rational expectations into dynamic macroeconomic models. Building

⁶In a model with Calvo price rigidity, in any given period, only a fraction θ of randomly selected producers is able to adjust their selling prices. See appendix C.1.2 on page 350 for a more detailed exposition.

on New Keynesian economics, it incorporates imperfect competition and costly price adjustment. Like the RBC program, it seeks to develop quantitative models of economic fluctuations.

Second generation New Keynesian models in the spirit of the New Neoclassical Synthesis allow for both real effects of monetary policy and cycle inducing disturbances from the real economy, such as developments in productivity and technology. Both elements are elegantly combined via wage and price rigidity to derive rules for optimal monetary policy. As a leading economist in the field explains:

Because of delays in wage and price adjustment, the consequences of real disturbances are usually inefficient, and their degree of inefficiency depends upon the response of monetary policy. As a result there is a role for active monetary policy, to mitigate the distortions that would otherwise result from the failure of wages and prices to adjust sufficiently in response to these real disturbances. (Woodford, 1999, p. 29)

The task of monetary policy in this framework is thus to reduce the costs of real disturbances in the face of market imperfections and not to add any additional disturbances itself. One could argue that the benefits of central bank monetary policy from that perspective are seen as a reduction in the costs of necessary real adjustments, and its costs are seen as either benefits foregone, every time central bank monetary policy does not quite optimally diminish the costs of real adjustments, or as additional real adjustments made necessary by completely misguided policy decisions.

5.2 A Modern Standard New Keynesian Model

In appendix C we present a formal mathematical exposition of the standard New Keynesian DSGE model following Galí (2008, ch. 3). Other representative expositions of the New Keynesian model framework can be found in Woodford (2003) and Walsh (2010). In the following we restrain ourselves to discussing the dynamics obtained from the central model equations and to verbal exposition wherever possible.

Section 5.2.1 focuses on the central building blocks of the model and the effects of technology and monetary shocks that a standard calibration of the model yields. Next, we discuss

theoretically optimal policy rules within the model framework in section 5.2.2, and then optimal practicable rules for policy purposes in section 5.2.3. Finally, we will briefly discuss the DSGE framework as an alleged solution to the Lucas critique.

5.2.1 The Basic Setup and the Dynamics of the Model

The central equations derived from specified mathematical optimization problems of representative households and firms, as shown in appendix C, are the *New Keynesian Phillips curve* (NKPC) and the *dynamic IS curve* (DIS):

$$\pi_t = \beta E_t\{\pi_{t+1}\} + \kappa \tilde{y}_t, \quad (5.1)$$

$$\tilde{y}_t = E_t\{\tilde{y}_{t+1}\} - \frac{1}{\sigma}(i_t - E_t\{\pi_{t+1}\} - r_t^n). \quad (5.2)$$

The DIS in equation 5.2 can be solved forward under the assumption that nominal rigidities become negligible in the long-run, that is, $\lim_{T \rightarrow \infty} E_t\{\tilde{y}_{t+T}\} = 0$. The output gap can then be written as the weighted sum of the gap between the real rate of interest and the natural rate:

$$\tilde{y}_t = -\frac{1}{\sigma} \sum_{k=0}^{\infty} (r_{t+k} - r_{t+k}^n).$$

This latter equation suggests that when the expected real return, i.e. the real interest rate, r_t , does not systematically deviate from the natural rate, r_t^n defined as the equilibrium rate under perfect price and wage flexibility (König and Chervyakov, 2017a, p. 2),⁷ output will be at its natural level as well. Whenever the real rate of interest is systematically above or below the natural rate, the output gap will be negative or positive, that is to say, real output will be below or above its natural level, respectively.

Given that the real rate of interest is defined by the nominal rate minus inflation expectations, $r_t \equiv i_t - E_t\{\pi_{t+1}\}$, we can also gain a first intuitive understanding of what the proper role of monetary policy is within the model framework. The nominal rate of interest, as a central bank policy tool, must be set so that the real interest rate is at its natural level, which will in turn stabilize real output around its natural level.

⁷The natural rate of interest is a theoretical concept originally introduced by Swedish economist Knut Wicksell (1962, [1898]). Because of the central importance of the natural rate of interest in the New Keynesian model, it is sometimes referred to as Neo-Wicksellian (Woodford, 2003). The concept of the natural rate of interest is discussed in more detail in chapter 7, section 7.2.1, stating on page 247.

Before we go into optimal policy rules, however, we first look at the dynamics generated by a calibrated model economy. The structure of the model is straightforward. The NKPC determines price inflation rates given the output gap, and the DIS determines the output gap given the actual real rate of interest and a process describing the path of the natural rate. To close the system one has to describe how monetary policy operates. It is clear from the outset that, given monopolistic competition and Calvo price rigidity underlying the model, as described in appendix C, monetary policy is not neutral.

We consider a simple interest rate rule:

$$i_t = \rho + \phi_\pi \pi_t + \phi_y \tilde{y}_t + v_t. \quad (5.3)$$

Keep in mind that ρ corresponds to the household's rate of time preference, or its discount rate.⁸ The intercept ρ allows for a zero inflation steady state as pointed out in Galí (2008, p. 50). The coefficients ϕ_π and ϕ_y are chosen by the central bank and have non-negative value. This means that nominal interest rates are increased both when price inflation or actual output relative to natural output are high. The term v_t is the exogenous part of the nominal interest rate equation.

Equations 5.1, 5.2 and 5.3 can be rewritten as the following system of difference equations:

$$\begin{bmatrix} \tilde{y}_t \\ \pi_t \end{bmatrix} = \mathbf{A}_T \begin{bmatrix} E_t(\tilde{y}_{t+1}) \\ E_t(\pi_{t+1}) \end{bmatrix} + \mathbf{B}_T(\hat{r}_t^n - v_t), \quad (5.4)$$

where $\hat{r}_t^n \equiv r_t^n - \rho$.⁹ In order to illustrate the equilibrium response of the model economy to a monetary policy shock, we assume in the following that v_t , the exogenous part of the interest rate rule, follows an AR(1) process, $v_t = \rho_v v_{t-1} + \varepsilon_t^n$, where $\rho_v \in [0, 1)$ and ε_t^n is a white noise term, that is, an uncorrelated random shock with zero mean.

An exogenous monetary policy shock is reflected in the white noise term. A positive value corresponds to a contraction, that is, an increase in the nominal interest rate if the indirect effect via price inflation and output gap does not offset the direct effect of the white noise term

⁸We use this term in exactly the same way as in equation B.6 in appendix B for the optimality conditions of the household in the New Classical model. See Galí (2008, p. 18) for further explanation.

⁹Moreover,

$$\mathbf{A}_T \equiv \Omega \begin{bmatrix} \sigma & 1 - \beta\phi_\pi \\ \sigma\kappa & \kappa + \beta(\sigma\phi_y) \end{bmatrix}; \quad \mathbf{B}_T \equiv \Omega \begin{bmatrix} 1 \\ \kappa \end{bmatrix}; \quad \text{and} \quad \Omega \equiv \frac{1}{\sigma + \phi_y + \kappa\phi_\pi}$$

See appendix C for the meaning of the coefficients. The system of difference equations has a locally unique solution if and only if the two eigenvalues of matrix \mathbf{A}_T lie within the unit circle (Blanchard and Kahn, 1980), which is the case, given that $\phi_y > 0$ and $\phi_\pi > 0$, if and only if, $\kappa(\phi_\pi - 1) + (1 - \beta)\phi_y > 0$ (Galí, 2008, p. 50). This is assumed to be the case.

in the interest rate rule defined in equation 5.3. A negative value corresponds accordingly to an expansionary shock. We consider an exogenous contractionary shock of 25 basis points ($\varepsilon_t^n = 0.25$), thus reproducing the results presented in Galí (2008, pp. 52-54) using the same calibration of the model.¹⁰

Figure 5.1 illustrates the impact of the exogenous contraction on some relevant variables based on the calibrated model as summarized in footnote 10. The corresponding period length is a quarter. Hence, the exogenous shock of 25 basis points would directly translate into an increase by one percentage point of the annualized nominal rate of interest. However, the exogenous shock also leads to a decrease in price inflation expectations as well as the output gap which has an offsetting effect. Hence, nominal interest rates increase by less than a percentage point, roughly 0.4%. We can see that the increase in the real rate of interest is higher. This is because of the downward correction of price inflation expectations. Both variables gradually move back to their natural levels.

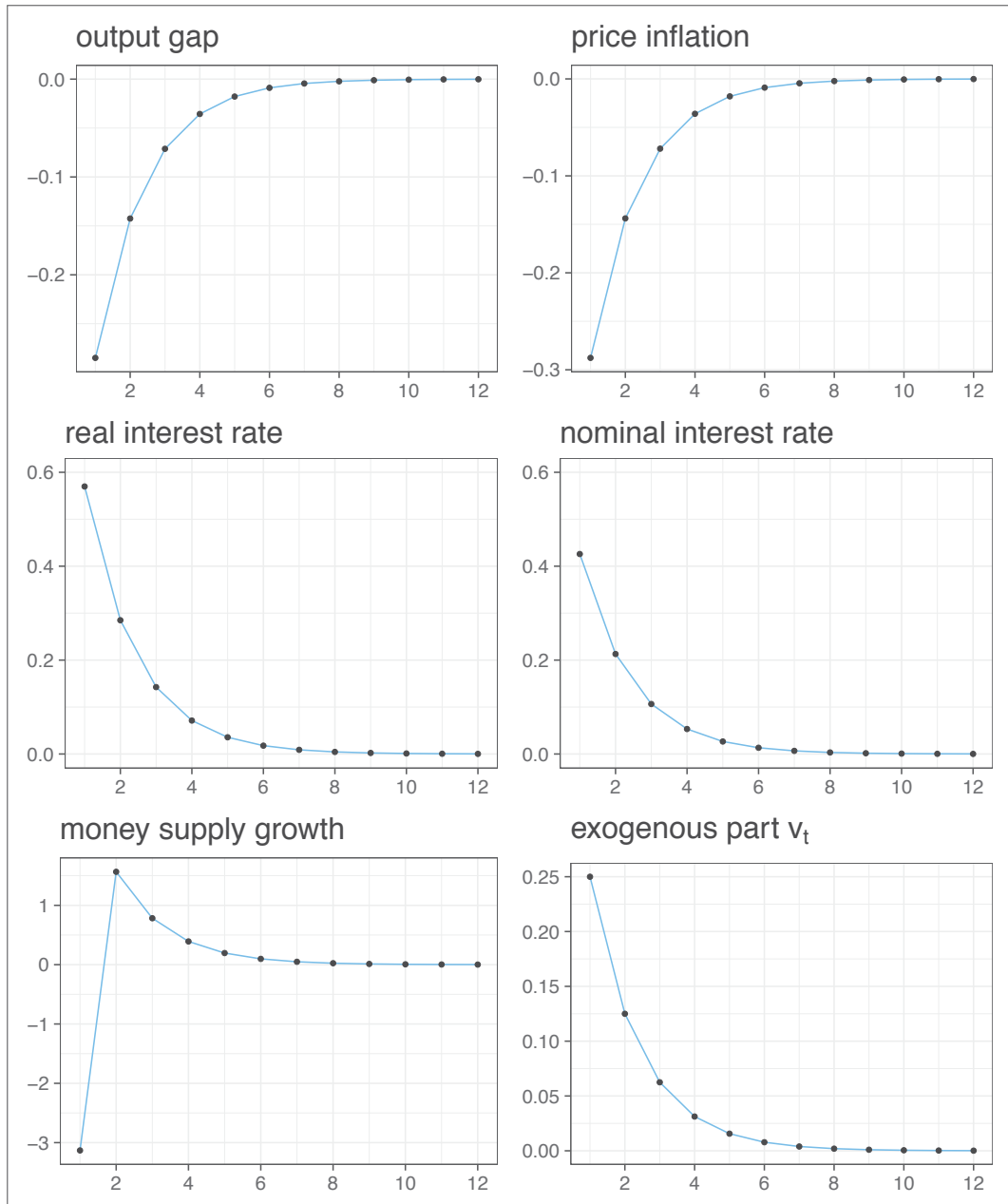
The output gap diminishes sharply and then moves logarithmically back to its steady state value of zero. The actual level of output behaves in exactly the same way, since the monetary shock has no impact on the natural rate of output. In order to accommodate the increase in interest rates, the central bank has to engineer a decline in the money supply.¹¹ According to the interest rate policy rule, the growth rate has to increase above zero in the next quarter and then gradually approaches zero thereafter. The shock is moderately persistent and its impact vanishes from quarter to quarter as depicted in the bottom right panel and reflected in all response functions shown in the other panels.

Next, we consider the impact of a positive technology shock by reproducing the results in Galí (2008, pp. 54-56). The central difference here is that real changes have an impact on the natural levels of output and interest rate. Analogously to the previous case, we assume that the common technology parameter in the production function of the firms follows an AR(1) process, $a_t = \rho_a a_{t-1} + \varepsilon_t^a$. Potential shocks from the monetary side are neglected, $v_t = 0$. A positive

¹⁰Galí (2008) uses the following parameter values: $\beta = 0.99$, $\sigma = \varphi = 1$, $\alpha = \frac{1}{3}$, and $\varepsilon = 6$, which the author claims to be found commonly in the business cycle literature. The interest semi-elasticity of the demand for money, η , is set equal to 4 “based on the estimates of an OLS regression of (log) M2 inverse velocity on the 3 month Treasury Bill rate (quarterly rate, per unit), using quarterly data over the period 1960:1 – 1988:1.” (Galí, 2008, p. 52, fn. 4) The price stickiness parameter is $\theta = \frac{2}{3}$ corresponding to an average price duration of 3 quarters (the period length under consideration is a quarter) which is in line with empirical evidence provided in Galí et al. (2001) and Sbordone (2002). Moreover, $\phi_y = 0.125$ and $\phi_\pi = 1.5$ consistent with the empirical evidence from the Greenspan era provided in Taylor (1999). The exogenous shock is chosen to be moderately persistent with $\rho_v = 0.5$. The chosen parameters imply that $\kappa = 0.1275$.

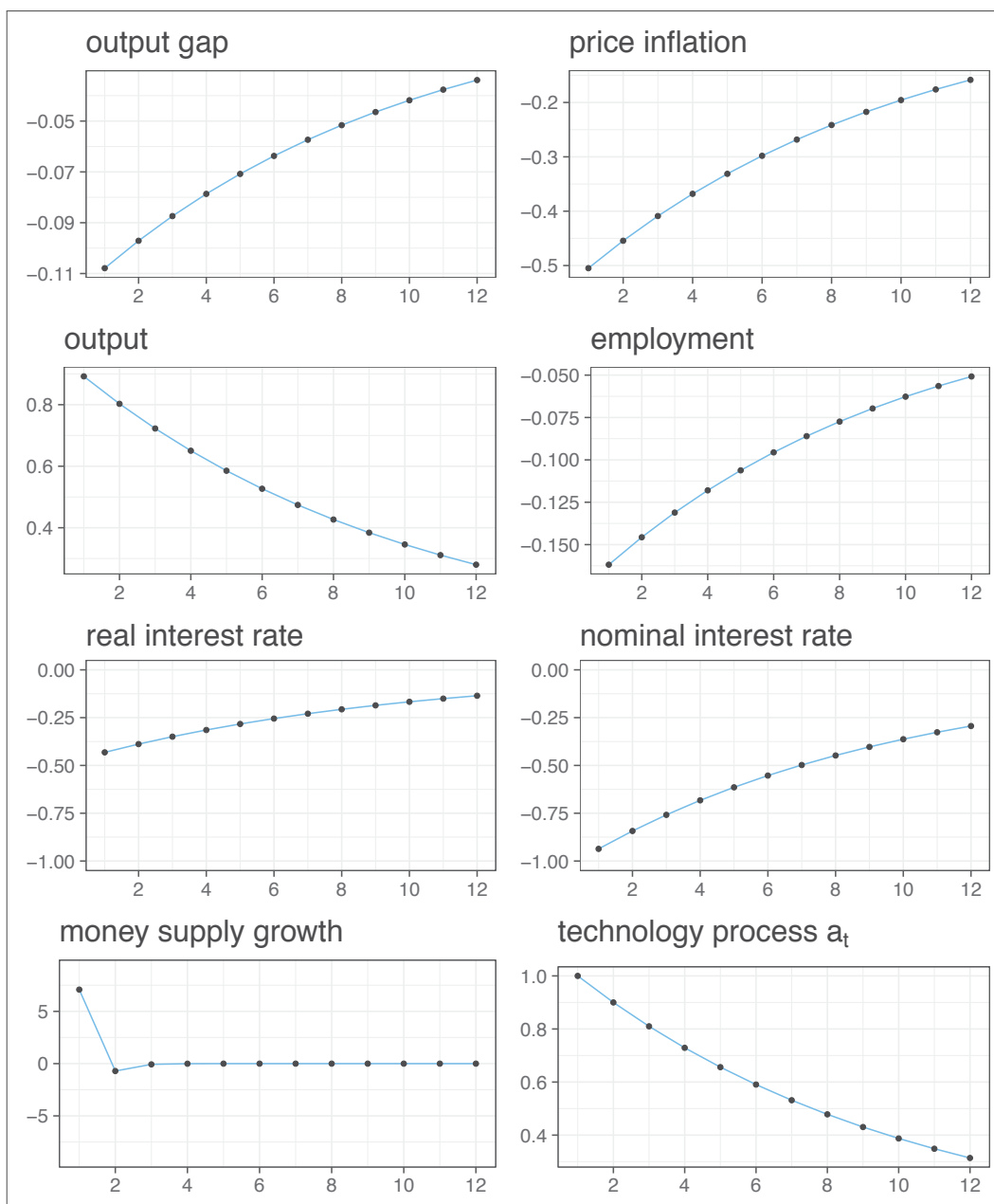
¹¹This can be derived from equation C.9 in appendix C.

Figure 5.1: Response functions of a moderately persistent exogenous contraction in monetary policy



This graph is a reproduction of figure 3.1 in Galí (2008, p. 53). The corresponding period length is a quarter. Nominal and real interest rates as well as price inflation have been annualized.

Figure 5.2: Response functions to a persistent exogenous technology shock



This graph is a reproduction of figure 3.2 in Galí (2008, p. 55). The corresponding period length is a quarter. Nominal and real interest rates as well as price inflation have been annualized.

technology shock, that is, an improvement in technology, occurs when the white noise term equals a positive value, say $\varepsilon_t^a = 1$. We assume the shock to be relatively persistent by setting $\rho_a = 0.9$.

Figure 5.2 contains the response functions for several variables, this time including the actual output as it differs from the output gap, as well as a measure of employment. The improvement

in technology increases both the natural and the actual levels of output. Yet, the latter increases to a lesser extent, which results in a decline in the output gap. It gradually approaches zero as the actual output catches up. Prices fall in response to the shock and the negative price inflation turns gradually back to the zero inflation steady state. Real and nominal interest rates fall, the difference between them being explained by price deflation. Employment diminishes initially but approaches its natural level in the following quarters. According to the interest rate rule of monetary policy, the technology shock is accommodated by monetary expansion. It can, however, not close the output gap and hence does not offset the negative impact on employment and price inflation completely as described by the NKPC. In all panels of Figure 5.2, we can see that the effect of the technology shock is more persistent as the variables approach their steady state levels slower as in the previous example.

The results of both examples resemble at least qualitatively the available empirical evidence. Galí and Rabanal (2004), for example, review the empirical literature on the relationship of technology and employment showing that technological progress can have persistent negative employment effects. Empirical studies on monetary policy shocks based on *Vector Autoregression* (VAR) are reviewed, for example, in Christiano et al. (1999). Rotemberg and Woodford (1999), Christiano et al. (2005) and Galí (2008) emphasize that for quantitative accuracy various features have to be added to the basic New Keynesian DSGE model. The real challenge that economists in this area face is thus fundamentally an empirical one, although additional auxiliary assumptions and parameters might also complicate the mathematical solution of the models. In the following section we stick to the basic model and ask what monetary policy rule would be theoretically optimal.

5.2.2 Theoretically Optimal Rules

On a theoretical and practical level there has been wide agreement on the desirability of well defined monetary policy rules ever since the rules-versus-discretion debate presented in the previous chapter. Woodford (2003, p. 2) emphasizes the development towards rule-based monetary policy on the first pages of his treatise: “What appears to be developing, then, at the turn of another century, is a new consensus in favor of a monetary policy that is disciplined by clear rules intended to ensure a stable standard of value, rather than one that is determined on a purely discretionary basis to serve whatever ends may seem most pressing at any given time.” The central question is of course how do we find good, better and even optimal monetary policy

rules? His answer is welfare analysis, an approach not only followed by Woodford (2003, ch. 6), but also Galí (2008, ch. 4) and Walsh (2010, ch. 8). All of these sources contain standard expositions of modern monetary policy analysis and evaluation.

According to this approach one has to determine first the goal of monetary policy, that is, the optimal or efficient allocation of resources with respect to the representative household's utility function, subject to the firms' production function. The solution of this optimization problem is straightforward in the baseline model presented in appendix C and above. Given the symmetry of the model, resources are optimally allocated when the total labor input is equally distributed across all firms, each of which produces an equal amount of output, and the marginal rate of substitution between consumption and work hours coincides with the marginal product of labor. This corresponds to the equilibrium market outcome in the basic New Classical model. Yet, it is precisely due to market power of firms, that is, monopolistic competition, and Calvo price rigidity that this outcome is not automatically achieved in the New Keynesian model.

First, monopolistic competition implies that firms face an imperfectly elastic demand curve and hence sell their products in lower quantities at a mark-up price. They demand less labor than the socially optimal scenario requires. The solution to this problem as explained by Galí (2008, p. 73) is a matter of fiscal policy. It can be solved by an employment subsidy financed through lump-sum taxes. Monetary Policy is important to mitigate the second source of distortion.

Since only a fraction of $1 - \theta$ firms adjusts its selling price in any given period, there is no reason to expect prices and hence mark-ups to be identical across firms as they should be. So, first the average mark-up must be stabilized at its frictionless level, that is, the common mark-up under monopolistic competition and perfect price flexibility. Moreover, monetary policy needs to mitigate the necessity for price adjustments, so that ultimately not only the average mark-up but every mark-up, every selling price, and every firm's sold quantity are identical. The fraction of firms that can adjust its price in any given period must keep its old selling price as monetary policy has adjusted in a way that makes price adjustments redundant.

Hence, the optimal monetary policy requires that there is zero price inflation, which is the case, if and only if, the output gap is completely closed in every given period as described in the NKPC in equation 5.1. This can be accomplished when the nominal rate of interest set by the central bank equals the natural rate of interest:

$$i_t = r_t^n. \tag{5.5}$$

Notice that rationally formed price inflation expectations of zero in the DIS of equation 5.2 are consistent with the policy rule 5.5, just like an expected output gap of zero for the next period, which would in turn imply an effective current output gap of zero. If the nominal rate of interest coincides with the natural rate in every period, the actual output level coincides with the natural output and employment is at its socially optimal level.

We could imagine a starting situation with sub-optimal price dispersion. In that case the above monetary policy rule would still be consistent with the social optimum in the long run. However, the socially optimal solution would not be the only possible solution to the system of difference equations that we obtain by implementing this policy rule.¹² Starting from any random situation with price distortion, several other equilibria are consistent with rule 5.5. There is no reason to believe that one would effectively end up with the social optimum of $\pi_t = \tilde{y}_t = 0$. This problem can be solved by a somewhat more complex rule.

Galí (2008, pp. 77-80) discusses two possible alternatives. The first interest rate rule differs from equation 5.3, which we used for the simulation of response functions above, only in that it ignores the exogenous part and that the rate of time preference, ρ , assumed to be constant in this context, is replaced by the varying natural rate of interest:

$$i_t = r_t^n + \phi_\pi \pi_t + \phi_y \tilde{y}_t. \quad (5.6)$$

Rule 5.6 is again consistent with the social optimum. Notice that when price inflation and output gap are zero, the nominal interest rate corresponds to the natural rate as before. Whether or not this is the unique equilibrium solution depends on the choice of parameters ϕ_π and ϕ_y .¹³

Both parameters have to be sufficiently large, that is, the reaction of the central bank to deviations from the socially optimal state of zero price inflation and a closed output gap have to be relatively strong. Given the above calibration of the model, the parameters $\phi_\pi = 1.5$ and

¹²Plugging policy rule 5.5 into the DIS in equation 5.2 we obtain the following system of difference equations:

$$\begin{bmatrix} \tilde{y}_t \\ \pi_t \end{bmatrix} = \mathbf{A}_1 \begin{bmatrix} E_t(\tilde{y}_{t+1}) \\ E_t(\pi_{t+1}) \end{bmatrix}, \quad \text{where } \mathbf{A}_1 = \begin{bmatrix} 1 & \frac{1}{\sigma} \\ \kappa & \beta + \frac{\kappa}{\sigma} \end{bmatrix}$$

The matrix \mathbf{A}_1 has only one eigenvalue within the unit circle. The other one lies outside. Hence, there are multiple equilibria that solve the system.

¹³The system of difference equations we obtain by implementing this rule is as follows:

$$\begin{bmatrix} \tilde{y}_t \\ \pi_t \end{bmatrix} = \mathbf{A}_2 \begin{bmatrix} E_t(\tilde{y}_{t+1}) \\ E_t(\pi_{t+1}) \end{bmatrix}, \quad \text{where } \mathbf{A}_2 = \frac{1}{\sigma + \phi_y + \kappa\phi_\pi} \begin{bmatrix} \sigma & 1 - \beta\phi_\pi \\ \kappa\sigma & \kappa + \beta(\sigma + \phi_y) \end{bmatrix}.$$

Both eigenvalues of matrix \mathbf{A}_2 are inside the unit circle, if and only if, $\kappa(\phi_\pi - 1) + (1 - \beta)\phi_y > 0$, which is the case, given a certain calibration of the model, if ϕ_y and ϕ_π are sufficiently large.

$\phi_y = 0.125$ that we used for our simulations are sufficient to make the social optimum the unique equilibrium solution. In fact, as long as the price inflation parameter $\phi_\pi > 1$, the parameter for the output gap could even be 0. This means that any deviation from price stability, for example, a price inflation rate of 1%, leads to a more than one-to-one adjustment of the nominal interest rate, that is, an increase by more than 1%. This result is known as the *Taylor Principle*, which will be discussed below in subsection 5.2.3.

The second alternative is a forward looking interest rate rule, which takes the expected price inflation and output gap for the next period into account:

$$i_t = r_t^n + \phi_\pi E_t\{\pi_{t+1}\} + \phi_y E_t\{\tilde{y}_{t+1}\}. \quad (5.7)$$

Again, the reaction parameters have to satisfy certain conditions to ensure that the social optimum is the unique equilibrium solution to the system of difference equations that we obtain from applying rule 5.7.¹⁴ The above parameter choice would be one possible combination. However, even though policy rules 5.6 and 5.7 provide theoretical solutions to the problem of multiple possible equilibria that emerges under rule 5.5, there are drawbacks with both of these rules when it comes to practical policy application.

5.2.3 Optimal Rules in Practice: The Taylor Rule

The obvious problem with both interest rate rules resulting in the unique and optimal equilibrium solution is that the natural rate of interest, as a theoretical concept, is not observable, and neither is the natural level of output, nor the output gap. Interest rate rule 5.7 even includes expected values of price inflation and output gap. Hence, the challenge is to find a suitable approximation applicable in practice.

In particular, modified versions of rule 5.6 have been extensively discussed in the literature and are known as *Taylor rules*. As we pointed out above, according to the Taylor Principle, as long as $\phi_\pi > 1$, one can set the the parameter for the output gap close or even equal to zero without running into the problem of multiple sub-optimal equilibria, and thereby reduce the

¹⁴The system of difference equations we obtain by implementing this rule is as follows:

$$\begin{bmatrix} \tilde{y}_t \\ \pi_t \end{bmatrix} = \mathbf{A}_3 \begin{bmatrix} E_t(\tilde{y}_{t+1}) \\ E_t(\pi_{t+1}) \end{bmatrix}, \text{ where } \mathbf{A}_3 = \begin{bmatrix} 1 - \frac{\phi_y}{\sigma} & -\frac{\phi_\pi - 1}{\sigma} \\ \kappa \left(1 - \frac{\phi_y}{\sigma}\right) & \beta - \frac{\kappa}{\sigma}(\phi_\pi - 1) \end{bmatrix}.$$

Both eigenvalues of \mathbf{A}_3 are below unity, if and only if, $\kappa(\phi_\pi - 1) + (1 - \beta)\phi_y > 0$, the same condition as for \mathbf{A}_2 , and $\kappa(\phi_\pi - 1) + (1 + \beta)\phi_y < 2\sigma(1 + \beta)$.

impact of inaccurate estimations of the output gap arbitrarily. Taylor (1993) himself originally proposed to set $\phi_\pi = 1.5$, the same value used in the above simulation, and $\phi_y = 0.5$, which is slightly higher than the above value. Moreover, he assumed a target rate of price inflation of two percent and a constant natural rate of interest:

$$i_t = 0.04 + 1.5(\pi_t - 0.02) + 0.5\tilde{y}_t \quad (5.8)$$

He assumed an equilibrium real interest rate of 2%, hence a nominal rate of 4% when price inflation is on target and the output gap closed. The natural rate of output was simply interpreted to lie on the linear trend line of observed annual real GDP on the logarithmic scale. He estimated a constant growth rate for the natural level of output of 2.2% per annum. Taylor presented his rule as a good empirical approximation of how US monetary policy has been conducted in the period from 1987-1992, a period during which monetary policy is considered to have performed very successfully (Woodford, 2001). Because of that he suggested to make the rule a principle of monetary policy (Taylor, 1999).

Subsequently, various contributions have been made on the theoretical and empirical level. Most notably Clarida et al. (1998, 2000) have extended Taylor's analysis and shown that certain specifications of the Taylor rule are good empirical descriptions of the interest rate setting of various central banks in the 1980s and 90s, including the Federal Reserve, the German Bundesbank as well as the Bank of Japan. Moreover, Clarida et al. (2000) argue that the Federal Reserve has violated the Taylor principle in the 1960s and 70s by changing the nominal interest rate by less than one-to-one in response to changes in price inflation.

Similarly, Lubik and Schorfheide (2004) argue that the stagflation experience of the 1970s was precisely an instance of indeterminacy, that is to say, the reaction parameter to deviations in price inflation rates was not large enough to ensure the uniqueness of the stable price, i.e. stable price inflation, equilibrium. Hence, their analysis fleshes out a prime example of the costs of central banking under fiat money within the New Keynesian framework, namely, as the welfare benefits foregone due to a deviation from the optimal rule. Monetary policy according to their analysis was too expansive in the 1970s. Nominal interest rates should have been increased more rapidly early on in the inflationary process. Only after 1982 was interest rate setting of the Federal Reserve in line with the Taylor principle.

Interestingly, Perez (2001) comes to a different conclusion using real-time data, that is, only data that were actually available at the time the policy decisions were made. He reestimated the

reaction function of the Federal Reserve and found that the price inflation parameter was above one even in the earlier period prior to 1982, thus satisfying the Taylor principle. The high price inflation rates of the 1970s occurred in spite of that. These findings are very important because they suggest that empirical analyses can be misleading when ex post revised data is used.¹⁵ The Taylor principle may not be a sufficient condition for macroeconomic stability, especially when the relevant knowledge about the current state of the economy is lacking.

Indeed, this is not merely a practical problem. Multiple formal extensions to the basic New Keynesian model have been made, showing under which conditions the Taylor principle ceases to be a sufficient condition for determinacy. Not surprisingly the specification of the model is crucial. As Walsh (2010, p. 343) points out, there have been two important modifications.

When the model incorporates a *cost channel*, that is, a direct effect of interest rates on the real marginal costs of production as, for example, in Christiano et al. (2005), Ravenna and Walsh (2006), Llosa and Tuesta (2009) or Abo-Zaid (2015), or when models include *search and matching frictions* on the labor market as in Ravenna and Walsh (2008) and Kurozumi and Van Zandwedge (2008), the Taylor principle need not ensure a unique equilibrium solution. In particular, Llosa and Tuesta (2009) direct their results against the findings for the standard New Keynesian model as discussed above and in more detail in Bullard and Mitra (2002) and Evans and Honkapohja (2003).

Moreover, when steady state price inflation is assumed to be above zero, as in Taylor's original rule, the Taylor principle as formulated above may again be insufficient as argued in Ascari and Ropele (2007). Coibion and Gorodnichenko (2008) use a calibrated model under which the responsiveness of central banks to price inflation would have to be ten times stronger to ensure determinacy when steady state price inflation lies around 6 percent. Kiley (2007) claims that volatility of price inflation in response to cost-push shocks, *ceteris paribus*, is stronger under moderate to high trend inflation.

This leads us to the next important and somewhat controversial element in the New Keynesian system, namely, the role of exogenous shocks. As seen above some economists have argued that monetary policy has been too expansive in the 1970s. Others, however, argue that it has in fact been too tight and defend this position by reference to exogenous factors such as oil price

¹⁵Orphanides (2001) also argues that the empirical results vary when real time data are used instead of revised data that were not available at the time the political decisions were made. Real time data are also used in Rudebusch (2006) who comes to the conclusion that monetary policy inertia is an empirical artifact due to revised ex post data. Given the data available in real time it disappears. Papell et al. (2008) show that there are differences for estimated Taylor rules when real-time or ex post data are used for both the United States and Germany, but to a larger extent for the latter.

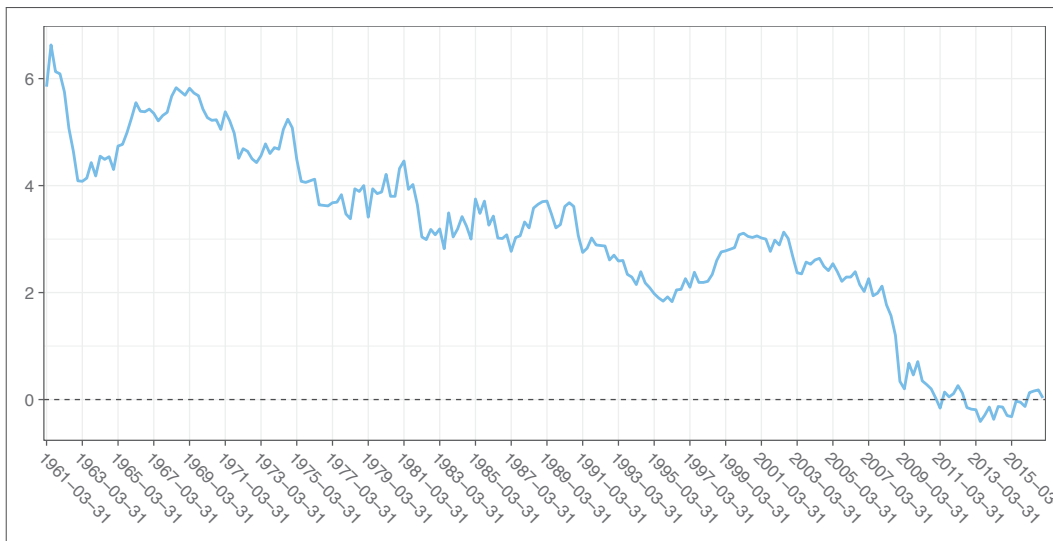
shocks. Bernanke et al. (1997) provide a standard example for this line of argument. They find that “an important part of the effect of oil price shocks on the economy results not from the change in oil prices, per se, but from the resulting tightening of monetary policy” (Bernanke et al., 1997, p. 136). A more recent overview is given in Blinder and Rudd (2013). In this view, external shocks account for a shift of the entire Phillips curve schedule towards higher rates of price inflation. Trying to keep price inflation at some target level of say 2% or even 0% in that situation is incompatible with real economic activity at its natural level. The important question, however, is to what extent increases in oil prices are really exogenous, that is, independent of monetary policy itself. The view that it was in fact the shift towards more expansionary monetary policy regimes that caused the oil price shocks in the first place is defended in Barsky and Kilian (2002). A systematic confrontation of both views is given in Kilian (2009).

This debate is just one example of a more general problem. Not only are magnitudes like the natural rates of output and interest unobservable and have to be replaced by some proxy, such as trend estimates of the observable counterparts, but any unexpected deviation of macroeconomic variables that is subsequently observed can in principle be explained in four different ways, namely, by reference to either the unsuitability of the chosen proxy variables, the erroneous calibration of the model, external shocks, or the falsehood of the entire theoretical framework. Of course, a combination or a subset of all four sources of error is possible too.

It gets even more complicated when we keep in mind that the empirical identification of any of these four sources of error, must presuppose that the other three are negligible, that is, that they are no source of error. For instance, if we want to identify exogenous shocks we have to presuppose the validity of the theoretical framework, the calibration of the model, as well as the suitability of the chosen proxies. Residuals that are not accounted for by the model can then be interpreted as external shocks to the system, like, for example, in the case of Solow residuals interpreted as technology shocks. However, in principle, what is then identified as a shock, could as well be a manifestation of the unsuitability of the proxy, the model as such, or its specific calibration.

One of the most important elements in the DSGE framework is the natural rate of interest. The theoretically optimal rule requires knowledge of this theoretical magnitude that is itself not observable. It is simply taken to be a constant in the original formulation of the Taylor rule. But is it reasonable to assume that the natural rate of interest, understood as the equilibrium rate under flexible prices, is constant over time? Many economists have in fact argued in recent years

Figure 5.3: Estimates of the natural rate of interest from Holston et al. (2016)



that the natural rate of interest has substantially decreased, even below the zero bound in the euro area. It has been suggested that the failure of central banks to adjust, or in fact, the impossibility of adjusting nominal interest rates accordingly, is one of the causes of the Great Recession that started in 2007 (Galesi et al., 2017). Holston et al. (2016) argue that falling natural rates of interest are an international trend.¹⁶

Various estimation methods for the natural rate of interest have been suggested and applied with diverging results (König and Chervyakov, 2017b). The simplest approach follows some form of univariate filtering and does not require a specific model (Hodrick and Prescott, 1997; Christiano and Fitzgerald, 1999). The underlying idea is that real interest rates fluctuate around the natural rate and that the latter can be estimated by some average of the observed real rates. The estimate ends up being a smoothed version of the observed data. Other estimation methods require a macroeconomic model that allows controlling for other factors that impact the natural rate. König and Chervyakov (2017b) further distinguish between the multivariate approaches based on simple models as in Laubach and Williams (2003) and fully-fledged DSGE models as in Smets and Wouters (2003) or Giammarioli and Valla (2004). The estimates of Holston et al. (2016) are plotted in Figure 5.3.

According to König and Chervyakov (2017b, p. 3) “a DSGE model allows to identify the

¹⁶These ideas feed into the debate on *secular stagnation* (Summers, 2014, 2015; Baldi and Harms, 2015; Baldi, 2017). According to the secular stagnation hypothesis “weak aggregate demand and a lack of productive investment opportunities have shifted the economy into a state of persistent stagnation at very low - if not negative - real interest rates” (Baldi, 2017, p. 1).

driving forces behind the NRI [natural rate of interest] and to forecast its future values.” But at the same time they acknowledge that estimates of the natural rate “are heavily dependent on the assumed structure of the economy and are subject to model uncertainty” (p. 4). These drawbacks have led Weber et al. (2008) to argue against the usefulness of the natural rate concept in political decision making. However, this is only indicative of problems that spread beyond this specific concept and touch the entire DSGE modeling program.

Indeed, whether an estimate of the natural rate can be considered reliable or not depends on whether the calibrated model accurately reflects the dynamics of the actual economy. But using a model that itself relies on the unobservable natural interest rate in order to estimate the natural interest rate must necessarily be self-fulfilling in the sense that the estimated magnitude improves the overall fit of the model to the observed data. Put differently, the model partly creates its own data with which it then unsurprisingly matches. If the theoretical explanation of an economic downturn is closely linked to actual interest rates having been above the natural rate, then the ex post estimation of the natural rate based on that theory will suggest that the natural rate has been even lower than the actual rates. In general, the more unobservable factors are allowed into the system the more leeway there is to develop an ex post explanation of unexpected events such as the Great Recession that fits into the overall framework. Hence, a good portion of skepticism towards the “gremlin’s price mark-up shocks”, as well as the “troll who makes random changes to the wages paid to all workers” (Romer, 2016, pp. 6-7), and generally any postulated exogenous shock or unobservable theoretical quantity seems advisable.

In practice it seems, even the most precise feedback rule in the New Keynesian system leaves room for discretion in any direction, although one would not discard the usefulness of scientific inquiry altogether, as a leading expert vividly explained: “Having looked at monetary policy from both sides now, I can testify that central banking in practice is as much art as science. Nonetheless, while practicing this dark art, I have always found the science quite useful” (Blinder, 1997, p. 17).

5.2.4 Solving the Lucas Critique

A pertinent question is then whether or not the DSGE approach can in fact be considered to have solved the fundamental problem indicated by the Lucas critique. The intuitive answer after the foregoing considerations is probably in the negative. However, in many modern macroeconomic texts one can find either explicit claims or implicit suggestions of having overcome the critique.

Gregory Mankiw has argued that the “theoretical challenges of Lucas and his followers have been met” and that New Keynesian macroeconomics rests on firm microfoundations (Mankiw, 1992, pp. 559-560). And Lucas himself seems to suggest that part of his own and his followers criticism may no longer be valid for the more modern branch of macro models:

Sargent and I were talking about a particular set of [1960s Keynesian] models which we were completely clear about... If a completely different class of models comes up which people like to call Keynesian, of course our criticisms can't apply. You can't write a paper in 1978 criticising work done in 1988. (Snowdon and Vane (1998) as cited in Snowdon (2007, p. 542))

Woodford (2003, p. 4) pays very close attention to the critique and the contributions of the New Classicals and explains the ambitious goal of his *magnum opus* as follows:

“The present study seeks to show that it is possible to use the tools of modern macroeconomic theory - intertemporal equilibrium modeling, taking full account of the endogeneity of private-sector expectations - to analyze optimal interest-rate setting in a way that takes the concerns of central bankers seriously, while simultaneously taking account of the “New Classical” [i.e. the Lucas] critique of traditional policy-evaluation exercises.”

A few pages later he argues that the microfoundations of the aggregate demand and supply relationships in his models allow for selecting “specifications (from among those that might appear similarly consistent with econometric evidence) with clear behavioral interpretations that thereby allow one to take account of the ‘Lucas critique’”(Woodford, 2003, p. 56).

Galí and Gertler (2007, p. 26) are very optimistic when discussing the turning away from traditional large-scale models in response to the Lucas critique and the focus on New-Neoclassical-synthesis models in noting that “[o]verall, the progress has been remarkable.” They argue that “[b]ecause these models have explicit theoretical foundations, they can also be used for counterfactual policy experiments”, which is exactly what the old models were not suited for according to Lucas.

Galí (2008, p. 6) in contrast is rather cautious when pointing out that the models discussed in his book “are arguably suited for the analysis and comparison of alternative monetary regimes without being subject to the Lucas critique.” He adds in a footnote that the critique is overcome

“[a]t least to the extent that the economy is sufficiently stable”, which would ensure among other things that the assumed degree of nominal rigidities and price stickiness can be viewed as constant.¹⁷ The problem is indeed that any choice of the price rigidity parameter θ is as *ad-hoc* as an assumption can be. Empirical studies on which to base the choice of the parameter are rather problematic for the same reasons pointed out earlier.

Whether or not to consider a certain price to be rigid upon observation requires knowledge of what the price actually should have been under price flexibility. This knowledge must come from the model itself, or else, price rigidity merely boils down to an empirical description of how long prices remain at a certain level without being changed, that is, an empirical phenomenon very unlikely to remain constant especially under monetary policy interventions or changes in policy regimes.¹⁸

Chari and Kehoe (2007, p. 4) claim that “[t]he practical effect of the Lucas critique is that both academic and policy-oriented macroeconomists now take policy analyses seriously only if they are based on quantitative general equilibrium models in which the parameters of preferences and technologies are reasonably argued to be invariant to policy.” This however is questionable on two grounds. First, DSGE models are used alongside more traditional models at all major central banks.¹⁹ Second, already before Lucas there was awareness of the necessity of finding ideally stable relationships in order to derive reliable policy advice.²⁰

If as we argued at the beginning of the previous chapter, in section 4.1.2, the Lucas critique is considered to be an empirical hypothesis, then the benchmark should be the empirical performance of DSGE models. Yet, there has been vehement criticism against these models on precisely this ground. Hurtado (2014), for example, argues that DSGE models would have performed equally poorly had they been used instead back in the 1970s. The traditional large-

¹⁷The transition from the overly optimistic point of view to the more cautious claim one year later in 2008 may have been a result of the emerging recession and the incapability of DSGE models to predict it. This, however, is mere speculation.

¹⁸There have been attempts to derive price rigidity endogenously as in Bakhshi et al. (2003). This however pushes the problem only one step back. We have to rely on ad-hoc assumptions on what factors determine “price stickiness.”

¹⁹The Fed, for example, employs among others two DSGE models, which are revised and extended versions of the models presented in Christiano et al. (2005) and Smets and Wouters (2007), respectively. The SIGMA model is an open economy model with multiple countries Erceg et al. (2006), whereas the ODE model is a closed economy model for the United States Edge et al. (2008). Yet, there main model remains a more traditional hybrid-type model, the FRB/US model Brayton et al. (2014). Two of the most important models used by the ECB include the New area-wide Model, which has no explicit microfoundations (Fagan et al., 2001), and the CMR Model based on Christiano et al. (2010). For a summary of the use of DSGE models in the ECB see also Smets et al. (2010). Again, a whole range of different models is used. The same holds true for the Bank of Japan that employs a “suite of models” (Hara et al., 2009).

²⁰This claim is made persuasively by Cruccolini (2010).

scale models did not predict the stagflation of the 1970s and neither would the new models have done the job. Moreover, the new DSGE models did not predict the recent recession of 2007. Undoubtedly also because of these observed deficiencies, much criticism has been launched against modern macro in the very recent past (Colander et al., 2008; Caballero, 2010; Romer, 2016).

Several years before, Lucas stated that the importance of his critique seems to be fading. “It used to be that you could hold that up, like a cross to a vampire, and defeat people simply by saying ‘Lucas critique’. People have gotten tired of that and I think that is fair enough” (Snowdon and Vane, 1998, p. 128). But the important question is not whether people have become tired of it, but whether considering the critique relevant only for some quantitative models has ever been justified. The recent experience suggests that this might indeed have been an instance of the pretense-of-knowledge problem: “The Lucas critique is clearly valid, but for many (most?) policy questions we haven’t yet found the solution - we only have the pretense of a solution” (Caballero, 2010, p. 99). The critique is relevant for all empirical investigations into the effects, costs and benefits of different political measures, also within a DSGE framework.

Colander et al. (2008) contains some alternatives to DSGE modeling that are in the positivist tradition. Like Kocherlakota (2016) they call for a return to an empirically based macroeconomics, using “a lot less theory”. However, it is not really a question of more or less theory, but of what kind of theory. A theoretical framework built on unrealistic assumptions about human behavior can be of any practical use only under an instrumentalist creed and if the validity of its conclusions for real world applications is carefully brought to the empirical test. It is then always subject to the Lucas critique, just like any other less theory-laden approach, as we will argue in more detail in chapter 6 in the second part of the thesis. Alternatively, a theory must be *realist*.

5.3 The New Keynesian Phillips Curve

In our exposition of the New Keynesian DSGE framework we have identified the New Keynesian version of the Phillips curve, specified in equation 5.1 on page 180 and formally derived in appendix C, as a central building block. There are several points to be discussed with respect to this standard specification and a potential alternative which has been called *triangle model* (Gordon, 1988, ch. 22.4).

In the following, we will briefly describe some of the particularities of the NKPC in com-

parison to the triangle model in section 5.3.1 and then comment on their relative empirical performances in section 5.3.2. This discussion will allow us to exemplify a fundamental drawback of the general approach, alluded to already in the previous section. Finally, we close our discussion with the most relevant implications for our topic.

5.3.1 The Particularities in Comparison

In his account of the history of the Phillips curve, Gordon (2011) identifies a bifurcation in the post-1975 literature. The first strand, which he refers to as the “right fork in the road” was the one initiated by the New Classical economists Kydland, Prescott and Sargent, and followed in recent years, for example, by Galí. It is thus precisely the line of research that culminated in the standard NKPC specification within the framework of DSGE models as described above.

Gordon calls the other strand the “left fork in the road” or the triangle model. Interestingly, it is the latter that he very explicitly regards as a thorough resurrection of Keynesian ideas. At the same time Gordon (2011) demonstrates that the standard New Keynesian version of the Phillips curve is nested in the triangle version, and hence, that there is no unbridgeable divide between them. Both are in a sense New Keynesian. In fact, he advocates a synthesis, or more precisely, a generalization of the standard NKPC into the triangle model. Let us first, however, describe the particularities of the two versions.

The NKPC as presented in this chapter incorporates the output gap and price inflation expectations for the next period as explanatory variables for the current rate of price inflation. It is thus forward-looking. The output gap could be replaced by the unemployment gap in order to directly formulate a relationship between unemployment and price inflation.²¹ As usual, the equation incorporates an error term whenever empirical analyses are performed.

The triangle version of the Phillips curve, in contrast, explicitly incorporates a supply shock term alongside the conventional error term. The underlying idea is that an adverse supply shock, for example, can have a depressing effect on both employment and economic output, if the price elasticity of the respective commodity is less than one. In that case, an increase of the commodity’s relative price would lead to an increase in the expenditure share for that commodity. Hence, the expenditure share for all other goods, including labor, would have to decrease. An adverse supply shock can thus shift the Phillips curve schedule towards higher

²¹Other specifications use marginal costs instead of output or unemployment gap. See again appendix C, where we derived the relationship between price inflation and marginal costs as an intermediate step for the standard NKPC specification.

rates of unemployment and price inflation. It were Phelps (1978) and Gordon (1975) himself who initiated this line of formal modeling.

Gordon (1984) argued that nominal GDP would thus have to grow at a faster rate than money wages in order to keep the level of employment relatively high. This is because of the additional nominal spending required for the commodity for which the adverse supply shock occurred. Let us suppose it is oil, since this is the most discussed example in the literature.²² Gordon (2011, p. 21) mentions three possible scenarios, including the one that in his opinion explains the stagflation of the 1970s:

If nominal wages are flexible, one option is for the growth rate of wages to become negative, allowing the growth rate of nominal GDP to remain fixed. At the alternative extreme with rigid wages, to avoid a decline in non-energy output, an accommodating monetary policy must boost nominal GDP growth by the amount needed to ‘pay for’ the extra spending on oil, but this will lead to an inflationary spiral if expectations respond to the observed increase in the inflation rate. A third alternative, and the one that actually occurred in the 1970s, was a combination of wage rigidity with a partial response of nominal GDP growth, pushing down both real non-energy spending and employment.

Incorporating supply shocks thus allows for the reconciliation of the Phillips curve with episodes of stagflation. Moreover, not only the supply-side element in form of a supply shock term, but also demand-side elements are incorporated into this “left fork” version of the Phillips curve as Gordon explains (p. 23). They take the form of the level and the rate of change of either the unemployment gap or the output gap. The third element is backward-looking inflation inertia that is caused, for example, by implicit and explicit contracts. Given these three groups of explanatory variables for the price inflation process, Gordon coined the term “triangle model” that was adopted in Rudd and Whelan (2005a) or Fitzenberger et al. (2008). One might also call it a dynamic demand and supply model of the Phillips curve.

Now, the substantive difference between the standard NKPC and the triangle model is twofold. First, the latter incorporates more explanatory variables than the former. Second, the triangle model is backward looking, that is, price inflation today is partly explained by the price

²²Other examples of supply shocks could include changes in the relative prices of imports and exports, the imposition or abolition of price controls, changes in productivity growth, or political events such as general strikes as occurred in France in 1968.

inflation rates of the past. Hence, it incorporates inertia, which should not be seen as a denial of rational expectations, but simply as a means to incorporate longer lags in the impact of monetary policy. It is also because of these longer lags that supply shocks gain importance in explaining fluctuations. In an idealized model scenario, monetary policy authorities could control at least one variable, either price inflation or unemployment, and would have to leave the other wherever it may happen to be. Given inertia, they cannot even control one of them immediately. This allowed Blinder and Rudd (2013) to account for the stagflation without blaming it on misguided monetary policy. As (Gordon, 2011, p. 24) explains:²³

In fact, given ample empirical evidence of long lags in the response of output and unemployment to monetary policy actions, the Blinder–Rudd results make perfect sense - an adverse supply shock causes an initial spike of unemployment, and the monetary policy response then determines by how much unemployment declines in the subsequent years after the shock.

According to Gordon, the decisive argument in favor of the more general triangle model is an empirical one. The explicit specification of supply shocks allows to account for both episodes of negative correlation between unemployment and price inflation as well as episodes during which the observed correlation is positive, that is, stagflation. The triangle model overall provides a better fit to historical experience, at least in the US.

5.3.2 Empirical Performance

The triangle specification of the Phillips curve avoids the forward-looking expectations of the NKPC, $E_t\{\pi_{t+1}\}$, and replaces them by a backward-looking term, $E_{t-1}\{\pi_t\}$, that is, the expected rate of inflation for the current period formed in the past. Gordon’s specification takes the following form:

$$\pi_t = \beta E_{t-1}\{\pi_t\} + \phi \tilde{U}_t + z_t + \varepsilon_t, \quad (5.9)$$

where z_t is a summary of supply shock variables, ε_t is a conventional white noise error term, and $\tilde{U}_t = (U_t - U_t^n)$ is the unemployment gap. The standard NKPC equivalent would be:²⁴

²³In the quoted passage, Gordon refers to the NBER working paper version of the article that was published already in 2008.

²⁴In contrast to equation 5.1, we have added the error term to signify its use for empirical analysis outside of the pure model framework. The output gap was replaced by the unemployment gap.

$$\pi_t = \beta E_t\{\pi_{t+1}\} + \phi \tilde{U}_t + \varepsilon_t. \quad (5.10)$$

For empirical work, the price inflation expectations as well as the natural rate of unemployment in both equations have to be specified more precisely. Apart from that, the inflation process in the NKPC is entirely driven by movements of the actual unemployment rate. The hypothesis is that the coefficient for the unemployment gap is negative. Gordon (2011, p. 28) argues that, at least for the United States, it is easy to show that this hypothesis is false in reality, because the correlation between price inflation and unemployment rates is positive over some periods and negative over others.²⁵

This empirical fact about the changing correlation is correct. However, the conclusion relies on an implicit assumption, namely, that the natural rate of unemployment remains relatively stable. As soon as this assumption is relaxed, an additional variable would be gained to potentially rescue the negative short-run price-inflation-unemployment relationship.

In the triangle version, however, there is even more leeway, since supply shocks are explicitly incorporated. For an empirical analysis they have to be specified and estimated as well. The NKPC specification subsumes supply shocks into the white noise term and thereby assumes that they have no systematic impact on the relationship. However, Gordon holds that adverse supply shocks can account for periods of negative correlation between price inflation and unemployment. And the reasoning goes in the other direction as well. Gordon (1998), for example, argued that it were beneficial supply shocks, as opposed to adverse supply shocks, that explained the unexpectedly low rates of price inflation during the 1990s in the US.²⁶

Gordon (2011) demonstrates the alleged superiority of the triangle version over the NKPC by using US quarterly data from 1962 to 2007. He specifies, in the first stage, the forward-looking expectations in the NKPC as a regression of the current price inflation rate, measured as the overall PCE deflator,²⁷ on four of its own lagged values as well as the unemployment gap.²⁸ Hence, in the second step, his actual reduced-form NKPC specification boils itself down to a regression of price inflation on its four next lagged values and the unemployment gap. We can

²⁵Alternatively, with the output gap instead of the unemployment gap, the coefficient would have to be positive. Yet, Rudd and Whelan (2005a) show that it is significantly negative in the US from 1960 to 2004 for what they call the “pure” New Keynesian Phillips curve.

²⁶A beneficial supply shock would be signified by a negative value for z_t , while an adverse supply shock corresponds to a positive value of z_t .

²⁷This is the price inflation rate based on the personal consumption expenditure index as computed by the Bureau of Economic Analysis in the United States.

²⁸More precisely, $E_t\{\pi_{t+1}\}$ is replaced by the following term $\pi_t = \sum_{i=1}^4 \lambda_i \pi_{t-i} + \phi \tilde{U}_t$.

see here, as Fuhrer (1997) argued, that in empirical applications forward-looking expectations are nothing more than regressions on past observations, and necessarily so.

It is interesting to note that, in reference to Rudd and Whelan (2005a,b), Gordon (2011, p. 35) laments that the procedure followed by Galí and Gertler (1999) of “adding additional variables like commodity prices and wage changes to the first-stage equation is entirely *ad hoc*” as they lack a justification from the basic model framework. The skeptical reader might feel inclined to ask how the selection of four instead of one, two, three, or five lagged values of price inflation is not *ad hoc* on exactly the same grounds.

The unemployment gap is then the last variable to be defined in terms of empirically observable magnitudes to render a regression analysis possible. Gordon simply replaces it by the observable unemployment rate and an intercept, which is identical to assuming that the natural rate of unemployment, that is, the non-accelerating inflation rate of unemployment (NAIRU),²⁹ remains constant over time. Ultimately, his NKPC specification incorporates five explanatory variables and is identical to a specification used in Roberts (2006).³⁰

The triangle model is operationalized in the following way. Gordon does not incorporate merely the four next lagged values of price inflation in order to provide a proxy for the price inflation expectations term, but decides to incorporate longer lags. More specifically, he incorporates the 1-, 5-, 9-, 13-, 17-, and 21-quarters lagged values of price inflation, that is, six lags in total. Moreover, he does not assume a constant NAIRU, but estimates a time-varying NAIRU based on supply shocks. He then incorporates not only the current unemployment gap, but also the next four lags of the unemployment gap.

This leads to eleven regressors plus the supply shock variables that describe the time-varying NAIRU, and then are incorporated again in the final regression. It is, however, impossible to extract the precise specification from the paper. We only know that supply shocks were used to estimate the time-varying NAIRU, and that they reappear in the final regression in some form or another. Presumably they add up to five additional explanatory variables, two of which are

²⁹Conceptually, the NAIRU is akin to the original natural rate of unemployment as described by Friedman (Ball and Mankiw, 2002). It is the rate of unemployment that for any given price inflation rate, and any given configuration of other relevant factors, would emerge in the long run.

³⁰It simply takes the form:

$$\pi_t = \gamma + \sum_{i=1}^4 \lambda_i \pi_{t-i} + \kappa U_t + \varepsilon_t,$$

which assumes the NAIRU to be $-\gamma/\kappa$. In Roberts (2006) the estimated value is about 7%. In the equation, current price inflation rates are simply regressed on their four next lagged values and the current unemployment rate.

dummy variables for price control legislation of the Nixon administration.³¹ Moreover, the reported equation 13 (Gordon, 2011, p. 35) suggests that some lagged values of supply shock variables were incorporated as well. There are thus, at the very least, sixteen regressors in the triangle model, as compared to only five in the standard NKPC.³²

Gordon (2011, p. 38) first engages in a simple goodness-of-fit comparison and notes “that the sum of squared residuals (SSR) for the triangle model [i.e. 64.6] is barely one-quarter that of the Roberts NKPC specification [i.e. 244.0].” Likewise, the reported R^2 is higher for the triangle model (0.93) than for its simpler alternative (0.78). Yet, this is a result that was to be expected. The triangle model incorporates almost the same and many more explanatory variables and thus will naturally end up with a better fit to the data.³³ We can see from all the regression summary statistics reported in Gordon (2011, p. 45) that there is only a minor increase of 0.01 in the R^2 statistic when a time-varying NAIRU is added to the standard NKPC. Similarly, using exactly the same lagged values for price inflation and the unemployment gap as in the triangle model, also only increases the R^2 by 0.01. The difference is thus almost entirely due to additional supply shock variables included in the final regression.

Next, Gordon (2011) engages in a simulation exercise to make a more persuasive argument for his triangle model. He estimates the coefficients of his curves using the data from 1962 to 1997 and then simulates the price inflation rates for the remaining ten years of his data set, 1998

³¹We can find a brief description of the supply shock variables in the paper. They include: “the change in the relative price of non-food non-oil imports, the effect on inflation of changes in the relative price of food and energy, the change in the trend rate of productivity growth, and dummy variables for the effect of the 1971-74 Nixon-era price controls.” (Gordon, 2011, p. 36) This adds up to at least five additional explanatory variables in the regression. From Table A.1 (Gordon, 2011, p. 45) that contains all the regression results, it is impossible to tell in what form precisely they entered the regression. We only find some clarifying remarks on the nature of the variables in a footnote:

The relative import price variable is defined as the rate of change of the non-food non-oil import deflator minus the rate of change of the core PCE deflator. The relative food-energy variable is defined as the difference between the rates of change of the overall PCE deflator and the core PCE deflator. The Nixon-era control variables [...] remain the same as originally specified in Gordon (1982b). The productivity variable is the eight-quarter change in a Hodrick–Prescott filtered trend of the change in non-farm private business output per hour (using 6400 as the Hodrick–Prescott smoothness parameter). (Gordon, 2011, p. 47, fn. 25)

Also the reference to Gordon (1982b) does not help us understand the used specification for the triangle model in Gordon (2011) entirely.

³²The final specification of the triangle Phillips curve in Gordon (2011) could have been something like this:

$$\pi_t = \sum_{i=0}^5 \lambda_i \pi_{t-(4i+1)} + \sum_{i=0}^4 \kappa_i \tilde{U}_{t-i} + \sum_{k=1}^3 \left(\sum_{i=0}^? \gamma_i^k z_{t-i}^k \right) + \theta_1 z_t^4 + \theta_2 z_t^5 + \varepsilon_t.$$

³³We do not know whether Gordon (2011) reports an adjusted or unadjusted R^2 statistic. For the latter, no matter what additional variable is added, one would always enhance the fit and end up with a higher R^2 .

to 2007. Next, he compares the simulated values to the actually observed price inflation rates for that period. Once again, the triangle version performs much better. In fact, the NKPC utterly fails to reproduce the observed time series and overestimates price inflation rates substantially. The mean error for the simulations of the triangle model is 0.29, meaning that price inflation was on average underestimated by 0.29 percentage points. In contrast, the mean error for the NKPC is -2.75. Price inflation was thus on average overestimated by 2.75 percentage points.

It is important to note, that the lagged values of price inflation for the simulation period were produced endogenously by the estimated curves. Hence, Gordon (2011) argues that since “the simulation has no information on the actual value of the inflation rate, there is nothing to keep the simulated inflation rate from drifting away from the actual rate.” This claim is in principle correct for the NKPC specification. However, it is arguably false that the simulation based on the triangle model has no information about the actual rate of price inflation. This is again because of the supply shock variables that are incorporated.

One of these variables included in the final regression is the difference between the overall PCE deflator and the core PCE deflator, which excludes the more volatile prices for food and energy. Its estimated coefficient is 0.89. As Gordon (2011, p. 38) himself notes, this variable is unsurprisingly highly correlated with the overall price inflation rate.³⁴ And this supply shock is not simulated endogenously. Hence, the actual values of a highly correlated variable are incorporated into the simulation of price inflation rates. It is therefore entirely unsurprising that the triangle version also performs better in the simulation exercise.

Gordon (2011, p. 39) counters that “this is an exercise not in forecasting but rather in determining whether a particular set of variables and lags adds to the explanatory power of the equation.” The explanatory power gained, however, is rather limited. What has been done effectively is the following. The author has simply taken a subset of goods, namely food and energy, for which we know that their prices tend to increase or decrease slightly more than other prices in times of general price inflation or deflation, respectively. Their price movements have been taken as the *explanandum* for the overall price movements.

This is as if one wanted to predict, or “explain” for that matter, the movement of the average temperature of the water in a lake. One simply keeps track of the water temperature close to the

³⁴When actual price inflation goes up the overall PCE deflator tends to go up a little bit more than the core PCE deflator and the difference between them is positive. When price inflation goes down, the overall PCE deflator tends to go down a little bit more than the core PCE deflator and the difference is negative. This is because the prices of goods included in the core PCE deflator are generally less volatile than food and energy prices. It is simply by construction of this shock variable that the simulation performance of the triangle model is increased.

surface. If it increases the average temperature of the lake increases as well and *vice versa*. But what have we learned about the causes?

5.3.3 Implications

The triangle model is ultimately a generalized NKPC. Nonetheless, these two versions of the Phillips curve have different policy implications on theoretical grounds. The triangle model emphasizes inertia, that is, longer lags for the impact of monetary policy. The NKPC postulates forward-looking expectations, according to which a credible change of monetary policy could, for example, immediately decrease price inflation without decreasing economic activity, and thereby easily offset too expansionary policies of the past. In a forward-looking model, the sacrifice ratio associated to disinflation tends to be lower. In contrast, one important consequence of inertia, within the framework of a DSGE model, is that the sacrifice ratio would be higher.

On the empirical-quantitative level where these two views are put to the test, however, there is no substantive difference between their specifications. Both aspects turn out to be modeled as regressions on a selection of lagged values of price inflation. In the study discussed above, the actual selection of lags plays only a negligible role for the empirical performance, both in terms of simulation and goodness-of-fit results.

More importantly, the incorporation of external supply shocks can change the implications for central bank monetary policy considerably. In the triangle model with supply shocks, the coefficients that describe the short-run trade-off between price inflation and unemployment are estimated to sum up to a value around -0.6. In the standard NKPC without supply shocks, the estimated coefficient is closer to zero. The curve is thus flatter, and the short-term benefits of monetary expansion are estimated to be lower. This empirical result is of course entirely dependent on the type of variables that are used as proxies for supply shocks.

In Gordon's study, the supply shocks are approximated by variables that capture themselves a part of the inflationary process, namely, the more volatile food and energy prices. In using their relatively high volatility as external shocks, one effectively assumes that central bank monetary policy has no impact on their development, but only on the residual of average price inflation for all goods that is not explained by the alleged shocks themselves. This is arguably false and leads to an underestimation of the costs of central banking in terms of increased price inflation. This is not to argue that there are no changes in relative prices that are independent of central bank policy decisions. To the contrary, as price developments are never homogeneous over all goods

and services, we will always find some prices that increase or decrease more than others, and that hence would potentially qualify as sources of external shocks.

With a detailed data set at hand, one could construct shock variables that would influence the empirical results in any direction. The fundamental problem is that the choice remains arbitrary, or, as Gordon himself might admit, *ad hoc*. Hence, the proportion of the changes of target variables that is assigned to external shocks, instead of the policy measures themselves, remains arbitrary too.

It is a curious fact that, while external shocks are explicitly incorporated as explanatory variables in regression analyses like the above, variables that reflect monetary policy decisions are left out of the equation. The impact of monetary policy on the inflation process is in that sense the residual that remains after the external shocks have done their work.

The policy conclusion is that If price inflation is high because of adverse supply shocks, monetary authorities should not try to decrease the latter, but accommodate the developments in order to prevent losses in real economic activity. The causal link between the expansionary monetary policy and the price inflation process is only a fill-in.³⁵

These analytical problems notwithstanding, external shocks remain the central feature around which the benefits and costs of central banking are discussed. From the perspective of New Keynesian economics, the task of central bank monetary policy is to reduce the costs of real disturbances or shocks in light of market imperfections, such as monopolistic competition and price rigidity. Moreover, additional disturbances from the monetary sphere have to be prevented, that is, monetary policy should not add any costly disturbances itself.

This means that the benefits of central banking, from this point of view, are seen as a reduction in the costs of necessary real adjustments. The costs in turn are again seen as either benefits foregone, in cases where the monetary authorities do not adequately reduce the costs of real adjustments, or as additional disturbances from misguided monetary policy decisions that lead to even more costly real adjustments.

More specifically, within the context of optimal policy rules, the research on the natural rate of interest within the DSGE framework has led to an important argument for monetary expansion. It has been suggested that the natural rate has fallen dramatically, over the past decades, even below the zero bound, and once again as a result of external forces. Hence, one alleged benefit of central banking under fiat money is that nominal interest rates can be actively manipulated

³⁵In chapters 7 and 8, we will explicitly begin a causal analysis from monetary policy itself and trace its implications for the economy deductively.

and kept close to zero in order to push the actual real rate of interest closer towards this natural rate. Within the New Keynesian DSGE framework, this policy is socially optimal.

Part II

A Causal-Realist Perspective on Monetary Policy Analysis

Chapter 6

Causal-Realist Economics and the Place of Econometrics

In the first part of this thesis we have presented the methodological foundations and main conclusions for monetary theory and policy of four important strands in modern macroeconomic thought: Keynesian large-scale macroeconometrics, Monetarism, New Classical and New Keynesian macroeconomics. All of these approaches share an important positivist element that becomes obvious in many of the internal criticisms that ultimately hinge on the conformity of a certain theory's predictions with observable empirical data. It is one of their separating characteristics from the causal-realist approach discussed in the second part that follows below.

The positivist element is enshrined in the instrumentalist view on economic theory, that is, economic theory as a tool for generating predictions about the future state of the economy. Roughly speaking, this view was sparked by the core postulates of modern econometrics (section 2.1.1, pp. 42ff.), established as the dominant view by Friedman (1953b) (section 3.1, pp. 88ff.), significantly revised but not rejected by Lucas (1976) and the contributions emerging out of his critique (sections 4.1 and 4.2, pp. 126ff.), and brought to a quasi consensus approach in New Keynesian DSGE modeling (sections 5.1 and 5.2, pp. 172ff.).

One can internally criticize the modern consensus approach, as has been done, for a lack of accurate predictions, for example with respect to the most recent financial and debt crisis starting in late 2007. Yet, the especially sobering result that the new models would not have performed better than the old ones had they been used instead back in the 1970s has strengthened the position of external critics. Indeed, it raises the fundamental question whether the gradual improvements in predictive power that the scientific process of instrumentalist-positivist economics has promised

are not merely short-term and feigned successes largely due to overfitting to particular historic circumstances and ex-post data estimation and manipulation. More and more economists feel inclined to answer in the affirmative. Romer (2016) is a testimony that received much attention.

An increased interest in alternative approaches has thus arisen over the past years. In particular, within the methodological literature we can observe a shift away from scientific instrumentalism and questions concerning the *what*, *where*, and *when* of economic phenomena, towards scientific realism and questions concerning the *why* and *how*. The causal-realist perspective presented here is intended to reflect this development and incorporate relevant revisions and criticisms into the analysis of monetary policy and the costs and benefits of central banking.

Chapter 6 will clarify some of the philosophical and methodological background. It contains an explanation of what we mean by causal realism as well as a methodological critique of the instrumentalist-positivist approach covered in the first part. Moreover, it suggests a purely descriptive conception of econometrics as the logical conclusion from that criticism.

In chapter 7, a causal-realist perspective on monetary theory and policy in the tradition of Austrian economics is explored. We emphasize aspects and dimensions of the costs and benefits of central bank monetary policy that are very difficult or even impossible to analyze properly under the methodological paradigm that span through the first part of the thesis, but also areas in which we find agreement and potential for reconciliation.

In chapter 8, we apply some of the insights of the previous two chapters in order to provide an explanation of a positive link between price inflation and long-run unemployment. This analysis will complement and revise the discussions of the Phillips curve trade-off scattered through the first part of the thesis. The closing chapter of the second part contains an application of the purely descriptive conception of econometrics as outlined below, in which we develop a cost accounting scheme for the financial and monetary system of the euro zone.

6.1 What is Causal Realist Economics?

Hands (2001a, p. 53) points out that “many different faces of ‘realism’ are now emerging within the methodological literature.” A closer look certainly confirms this claim. Hence, it will be important to explain as precisely as possible what we mean by *causal realism*. In particular, we have to address the question of whether Milton Friedman’s methodological contributions (Friedman, 1953b) can be regarded as standing in the tradition of causal realism, as has been suggested

recently (Mäki, 2009b; Hoover, 2009), in order to avoid possible terminological confusions. These points will be covered in subsections 6.1.1 and 6.1.2. We will show that causal realism, as understood here, is incompatible with Friedman (1953b) and the other approaches studied in the first part of the thesis, and that it represents a genuine alternative to the instrumentalist-positivist framework.

In the following sections 6.2 and 6.3, we present a criticism of positivist economics and modern econometrics from the methodological position of Austrian economics, and in particular the branch related to the work of Ludwig von Mises (1881-1973), which we regard as the best developed theoretical framework in line with our understanding of causal-realist economics. We also briefly present Pawel Ciompa's original conception of econometrics and show how it is reconcilable with Austrian economics in the Misesian tradition. These considerations have very important consequences for our evaluation of the numerous empirical works on monetary theory and policy. They help us identify both, elements that allow for reconciliation as well as insurmountable opposition between the two methodological camps discussed in this thesis.

6.1.1 The Causal-Realist Tradition in Economics

There are evidently many different philosophical doctrines that are called realist, and not only are there differences in kind, but also in degree.¹ We cannot flesh out the various forms of realism in much detail,² but two of their unifying elements are claims to *existence* and *independence*. This means that the objects of a subject matter are considered to *exist*, being in some sense real, having certain properties, and that they do so independently of how we talk and think about them, or how we conceptualize them.

Now, more or less realist positions on aspects and objects of the external physical world may seem very common. These, however, do not fall into the subject matter of economic science. Economics is concerned with elements of what might be called the internal or mental world,

¹Miller (2016) writes in his introduction:

Although it would be possible to accept (or reject) realism across the board, it is more common for philosophers to be selectively realist or non-realist about various topics: thus it would be perfectly possible to be a realist about the everyday world of macroscopic objects and their properties, but a non-realist about aesthetic and moral value. In addition, it is misleading to think that there is a straightforward and clear-cut choice between being a realist and a non-realist about a particular subject matter. It is rather the case that one can be more-or-less realist about a particular subject matter. Also, there are many different forms that realism and non-realism can take.

²The following discussion will focus only on the important elements for our study and must thus necessarily be fragmentary. We do not aim at a comprehensive presentation that a trained philosopher could expect.

namely, with human choice and action. The aim of the instrumentalist approach to economics is to predict choices and actions as well as their consequences, at least on an aggregated level, on the basis of observable external factors. It is reductionist to this extent. It tries to reduce choice and action to configurations of external factors. One could argue that it does not, at least within the framework of economic models, leave room for choice and action to exist in a more meaningful way than as the reflexive behavior of human beings to observable external impulses.

The “traditional opponent of instrumentalism” is realism (Mäki, 1990a, p. 311). There are, according to Mäki (1990a, p. 312), two schools of economic thought that “are obviously amenable to realist interpretation and reconstruction,” namely, the Marxian and the Austrian. Just like Mäki, we will focus exclusively on the Austrian school.³

It needs to be emphasized, however, that the non-Marxian realist tradition in economics is broader than merely Austrian. According to Salerno (2007), it can indeed be traced back to Carl Menger, founder of the Austrian school, but includes economists from various other countries, such as John Bates Clark (1847-1938), Frank A. Fetter (1863-1949) and Herbert J. Davenport (1861-1931) in the United States, or Maurice Block (1816-1901) and Paul Leroy-Beaulieu (1843-1916) in France.

One could go further back in time and argue that the tradition leads back to some earlier Spanish writers, like the late scholastics of the school of Salamanca, including Martín de Azpilcueta (1491-1586) and Diego de Covarrubias y Leyva (1512-1577), as well as the Jesuit priest Juan de Mariana (1536-1624) (Huerta de Soto, 1999).

A series of French writers could be added as well, like Nicole Oresme (1320/1325-1382), whose treatise on money, *Tractatus de origine, natura, jure, et mutationibus monetarum*, was the first book exclusively devoted to an economic topic (Hülsmann, 2010, p. 19), and later Richard Cantillon (1680-1734), Anne Robert Jacques Turgot (1727-1781), Jean-Baptiste Say (1767-1832) and Frédéric Bastiat (1801-1850) (Thornton, 1999; Hülsmann, 2001; Rothbard, 2006a, ch. 12 and 14; Rothbard, 2006b, ch. 1 and 14).⁴ Yet, in modern economics after 1936, which is where our focus lies, the realist tradition is predominantly held up by Austrians and

³While the Marxian framework does in fact provide some important insights into social reality, we consider an early critique of its economic aspects delivered by Austrian economist Eugen von Böhm-Bawerk (1851-1914) in his *Zum Abschluss des Marx'schen Systems* of 1896, an English translation of which can be found in von Böhm-Bawerk (1949), to be sufficient to disregard it for our current purposes. A very insightful discussion of Marxian political philosophy from an Austro-libertarian perspective can be found in Hoppe (2006, ch. 4), who argues that Marxism is essentially right in his theory of history, but derives its theses from a false starting point.

⁴Moreover, Menger's work was very strongly influenced by German economics, most notably the work of Wilhelm Roscher (the most cited economist in Menger's *Principles*), precisely on such central elements as subjective value theory (Streissler, 1990).

most notably by Ludwig von Mises and his followers.⁵

In another early piece on the topic, Mäki (1990b, p. 291-292) argues that although Austrian economics has been seen as a bit more realistic than neoclassical economics, it is still seen as being “on the ‘unrealistic’ side of the dividing line, at least when compared to American institutionalism or to Menger’s actual opponent, German historicism.” He further clarifies that “these sorts of assessment seem to be rooted deep in ordinary economists’ unreflected intuitions”, and sets out “to show that a case can be made for Austrian theories being realistic in a very ambitious sense and that therefore a radically realist view of Austrian economics is defensible.” We share this position.

Human choice and action are given a central position in Austrian economics. This has been seen as a unique feature by one of its most important proponents: “What distinguishes the Austrian School and will lend it immortal fame is precisely the fact that it created a theory of economic action and not of economic equilibrium or non-action” (von Mises, 2013, p. 24). In other words, the concept of action is what Austrian economics seeks to *explain*, not in the sense of *predicting* action and its outcomes or identifying its material causes, but analyzing what is logically implied in it. This includes adopting related concepts, such as purpose, means, ends, preferences and values, their relation to action and their subjective nature. Mäki (1990a, p. 315) explains how these concepts fit into a realist position:

Austrians characterize an essential element in their approach as ‘subjectivism’, and the import of this is simply that reference to mental entities such as valuations, purposes and expectations of human individuals should have a prominent role in economic theories and explanations.

Consequently, the relevant version of scientific realism should allow mental entities to exist as scientific objects. To exist in what sense? Clearly, we have to put aside those versions of realism which specify the concept of existence merely in terms of externality or independence with respect to the human mind. Mental entities - unlike material entities - do not exist externally to and independently of human minds. We

⁵Von Mises, one of the most important representatives of the Austrian school, has devoted more time and effort than most other economist to clarify the relation between economic theory and social reality. In the first chapter of *Human Action* he states:

The main question that economics is bound to answer is what the relation of its statements is to the reality of human action whose mental grasp is the objective of economic studies. (von Mises, 1998, p. 6)

can, however, say that purposes, expectations, etc. of economic agents may exist objectively, that is, independently of and unconstituted by economists' beliefs about them. Thus, it is the notion of existence as objective existence which should be part of the relevant version of scientific realism.

The fundamental theoretical concepts are considered to have objective existence. However, they are far from being completely explained on the basis of material factors, and hence are not observable empirically in an encompassing sense.⁶ Some of their consequences, i.e. material consequences, are observable, but a causal explanation of the latter presupposes an understanding of the meaning of action, related concepts, and its teleological nature, that is, its goal orientedness. These concepts are thus *a priori* (von Mises, 1962, p. 12; von Mises, 1998, pp. 32ff.; Hoppe, 2007). Action as such is not observable, but only the rearrangement and transformation of matter in the external world that it causes. Abstract propositions that relate to human action in general are thus not testable empirically, which is not to say that there is no way of evaluating their truth claims.⁷

They are arrived at by logical deduction from the self-evidently true proposition that humans act, that is, they purposefully employ means to attain chosen ends, and certain auxiliary assumptions, such as the disutility of labor (Rothbard, 2009a, p. 2). The truth claim of a theoretical proposition is then evaluated on the basis of the logical consistency of the chain of reasoning that leads to it. The proposition is applicable whenever and wherever the auxiliary assumptions are an accurate description of reality.

We see here that one central difference to the modern instrumentalist position lies in the use of assumptions or abstractions. Both approaches, as any scientific procedure, require abstractions, but they are of a very different kind. The instrumentalist-positivist position regards accurate empirical prediction as the highest goal of economic theory and modeling. Any assumption that

⁶In so far as the natural sciences succeed in reducing mental states, preferences, valuation, and ultimately human behavior to biochemical states of the human body and brain, they would render them observable in a meaningful way.

⁷It should be noted here, that the questions of testability and observability are in general independent of the philosophical position of realism, as (Mäki, 2002, p. 8) explains:

One can be a realist about the world and about theories of that world. Take *T* to be a theory, model, or assumption related to chunk *S* of the world. One is a realist about *S* in relation to *T* if one believes that *S* exists independently of accepting, believing, or uttering *T*. One is a realist about *T* in relation to *S* if one thinks that *T* and its constituents refer to *S* or that *T* in addition truly represents or should truly represent *S* - where truth is likewise independent of whether *T* is accepted, believed, or uttered. These definition sketches imply that, for example, the observability of an object and the testability of a theory are conceptually unconnected to realism.

is deemed conducive to this goal is acceptable. Modern DSGE modeling, for example, assumes specific forms of utility functions and inputs to these functions that determine consumer welfare and quantifiable optimal behavior, or identical production functions that allow for calculating precise marginal costs of production that hold for infinitely many firms, etc. In other words, the numerous factors potentially influencing the agents' preferences and behavior are either assumed away or assumed to have precise, quantifiable and measurable form. After all, whether these assumptions are realistic or not is irrelevant. They serve the purpose of formulating testable quantitative-empirical predictions about agents' behavior and market outcomes. This type of abstraction is called *precise*.

Likewise, the realist approach requires abstraction. Yet, our lack of knowledge about the causes of action, that is, the driving forces of preferences, utility or expectations, is not filled by precise and unrealistic assumptions or simply disregarded for the purpose of economic theorizing and modeling. It is explicitly acknowledged in taking human action and choice as an "ultimate given"⁸ (von Mises, 1998, pp. 17ff.), not to be traced back to its causal factors, at least not in the field of economics.⁹ Instead of giving action a precise shape of unrealistic specifications, it is made the cornerstone of economic theory in its general and abstract form. This type of abstraction is *nonprecise*.¹⁰ Long (2006, p. 7) describes the distinction as follows: "In short, a precise abstraction is one in which certain actual characteristics are *specified as absent*, while a nonspecific abstraction is one in which certain actual characteristics are *absent from specification*."

Nonprecise abstractions are characteristic of the realist approach to economics. Theoretical economics in the sense of von Mises (1998) takes a non-precise abstraction of action to be the logical starting point from which to analyze all economic phenomena: "The starting point [...] is not a choice of axioms and a decision about methods of procedure, but reflection about the essence of action" (von Mises, 1998, p. 39).

The theoretical science of economics is thus occupied with the essence or the *universals*, that is, the time-and-place invariant aspects of human action. Economic history, while always

⁸This is not to argue that there are no factors that influence action or even cause it. Von Mises in fact seems to hold a determinist position (von Mises, 1998, p. 46; von Mises, 2007, ch. 5).

⁹Explaining preferences, needs, choices and action on the basis of different determining factors is of course a worthy scientific endeavor in itself. It is the proper task of psychology (von Mises, 1998, p. 12).

¹⁰On the distinction between precise and nonprecise abstraction, and in particular their use in economic theory in line with Friedman's and von Mises's methodological positions, respectively, see Long (2006). The distinction goes back to Aristotelian philosophy which had an important impact on Austrian economics via the Viennese philosopher Franz Brentano (Smith, 1994).

employing economic theory, uses additional methods of inquiry in order to analyze the *particular*, time-and-place contingent, circumstances of human action (von Mises, 2007).¹¹

Moreover, individual human action and subjective valuation are always taken to be the causes of the phenomena to be explained in economics. The importance of cause-and-effect analysis in understanding economic phenomena, in fact, all phenomena, is reflected in the very first sentence of the first chapter of Menger's *Principles*: "All things are subject to the law of cause and effect."¹² Hence, we have adopted the more precise attribute causal-realist instead of merely realist.

Evidently we cannot discuss the entire theoretical edifice here.¹³ Instead, we will focus on the general aspects of a monetary economy, and more specifically, the consequences of a legal monopolist in money production, that is, a central bank, in chapter 7, before entering into applications that do in fact fall under economic history in chapters 8 and 9.

6.1.2 Was Friedman a Causal Realist?

A recent argument remains to be discussed briefly. While Uskali Mäki has persuasively argued that Austrian economics stands in the tradition of philosophical realism, he has also recently suggested that Milton Friedman's methodological contribution can be interpreted as a realist statement, although this, in Mäki's own words, might require some "rereading" and in fact "rewriting" of his famous essay on *The Methodology of Positive Economics*.¹⁴ We suggest that we stick to what Friedman had actually written himself, although rewriting his essay might render things more entertaining at times.

Hoover (2009, p. 319) echoes Mäki's interpretation of Friedman and concludes that the text "is best read as advocating causal realism." The standard view, however, as outlined in

¹¹We will elaborate on this distinction in section 6.2.

¹²The cause-and-effect analysis of the Austrians stays in stark contrast to mutual determination in systems of simultaneous equations, characteristic of the Walrasian neoclassical approach. As Stigler (1946, p. 181), in criticizing the causal-realist theory of price formation developed by Eugen von Böhm-Bawerk, a student and follower of Menger, pointed out: "Mutual determination is spurned for the older concept of cause and effect" (cited in Rothbard, 2009a, p. 327). In other words, neoclassical economics tries to do without cause-and-effect analysis.

¹³It is developed in much detail in von Mises (1998) and Rothbard (2009a).

¹⁴Mäki (2009b, p. 91) describes his attempt as follows:

To the extent that my *rereading* fails to be a matter of unbiased discovery of what is already there, hidden in the text of F53 [(Friedman, 1953b)], it can also be taken as a project of *rewriting* the essay. It is a matter of rewriting by selection and correction so as to eliminate its flaws and to make it more agreeable to a variety of audiences. On this rereading (or rewriting) F53 emerges as a realist (rather than instrumentalist) manifesto with strong fallibilist and social constructivist sensitivities (in contrast to standard textbook positivism).

Hutchison (1992, 2000) and Blaug (2002a,b), is obviously in conflict with this controversial claim. According to these authors, the essay has sparked the formalist revolution of the New Classicals. Their analysis is based on the more common interpretation of Friedman as advocating methodological instrumentalism, not being concerned with the realism of underlying assumptions. Mäki (2009a, p. 63) acknowledges that “[t]he instrumentalist interpretation of F53 [(Friedman, 1953b)] used to be the dominant one” and suggests that it nonetheless “may have to give way to a diametrically opposing realist reading.” In his article he concludes:

I have reread F53 by focusing on a selected set of ambiguities that open up opportunities for reinterpretation. I have exploited these opportunities by highlighting the partly hidden realism in F53’s conception of economic science. On this basis, F53 could be rewritten as an unambiguous and consistent realist manifesto. It conveys a methodology of economics that conforms to the tradition of viewing theories or models as partial but potentially true descriptions of causally significant mechanisms. Their primary service is to convey explanatory understanding (answers to why- and how-questions) and only secondarily to yield predictions (answers to what-, when-, and where-questions). (Mäki, 2009b, p. 113)

Sure enough, if it were possible to literally rewrite Friedman’s text, we could make it a manifesto of whatever is desired. Alternatively, one can stretch definitions. As pointed out above, there are different kinds of realism, and Mäki would probably not argue that Friedman falls into the same camp as Austrians. So one might simply blame it on semantic divergences. Yet, it is rather difficult to convince oneself of the alleged secondary role of empirical prediction in Friedman’s stated methodology after a disinterested reading and interpretation of his essay, not based on its “ambiguities” but on what he explicitly states.¹⁵

Empirical prediction is very openly declared the only relevant benchmark for the assessment of models, which Mäki (2009b, p. 95) does in fact acknowledge, but he squares this view with a “realist” position by simply reducing the meaning of realistic assumptions, or what he calls “approximate truth of assumptions” (p. 95), to precisely their predictive performance. He

¹⁵Friedman (1953b, p. 7) clearly writes that it is the “ultimate goal of a positive science” to develop “a ‘theory’ or ‘hypothesis’ that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed.” For him it is a “fundamental methodological principle that a hypothesis can be tested only by the conformity of its implications or predictions with observable phenomena” (p. 40). Also he claims that, with respect to the criterion of accurate prediction, as a general rule “the more significant a theory, the more unrealistic the assumptions” (p. 14). But maybe these passages have been subject to rewriting in Mäki’s account. For a detailed discussion of Friedman’s essay, go back to section 3.1 on page 88 in the first part of the thesis.

clarifies that “one is advised [by Friedman] to pay attention” to the assumption’s “actual degree of realisticness and to judge whether it is sufficiently high for the purposes at hand” (p. 95). And the purpose is empirical prediction. Anybody, who feels so inclined, is of course free to choose and can call this a “realist” position. After all, it focuses exclusively on what is observable, measurable, and hence existent in the material world from our own limited perspective. However, we regard this as a rather confusing use of language.

It is more properly called an empiricist or positivist position, and more precisely even, an instrumentalist-positivist position. The feasibility of empirical prediction as benchmark is a corollary to an actual understanding of the *nature* or *essence* of the subject matter, given the current limitations of our knowledge. But Friedman, nonetheless, declared prediction to be the primary purpose without much reflection on the actual subject matter of economics. The assumptions on which a theory or model is based are supposed to serve this purpose. And Mäki defines their “realisticness” or approximate truth very pragmatically by how well they do.

Hoover (2009, p. 319) acknowledges that Friedman’s essay “was a contributing cause in the suppression of causal language in economics.” He shows in his article that Friedman barely ever mentions the terms “cause” and “causal” although he uses some words that can be regarded as synonymous. Friedman himself stated that he tries to avoid the term “cause” for being “tricky and unsatisfactory” (as cited in Hoover, 2009, p. 306). This should not come as a surprise, since, strictly speaking, there is no way of identifying causal relationships in the social sciences based on empirical analysis. This is widely accepted despite the use of misleading terms like *Granger causality*, which really boils down to an assessment of predictive power of one variable for another over a specific historical period.¹⁶ Unless one wants to reduce the meaning of causality in Humean spirit to empirical prediction, the predicate “causal-realist” for Friedman’s stated methodology seems to be as misleading as a mere “realist.”

It thus needs to be emphasized, for the purpose of clarity, that Friedman’s position, and by extension the methodological basis of instrumentalist-positivist economics, is very distinct from what we refer to as the causal-realist approach. The latter approach in general, and Austrian economics in particular, might, given the dominance of the former, seem extraordinary, or after a less charitable reading even “cranky and idiosyncratic.”¹⁷ However, plagiarizing Shakespeare,

¹⁶The term refers to a statistical hypothesis test and is named after Granger (1969).

¹⁷Blaug (1980, p. 93) wrote on von Mises’s methodological writings: “His writings on the foundations of economic science are so cranky and idiosyncratic that one can only wonder that they have been taken seriously by anyone.” Skousen (2016, ch. 4), an author very sympathetic to the practical conclusions of von Mises, boldly adds: “cranky and idiosyncratic indeed.”

we confer “as Polonius would say, there is a method to this madness” (Hartley, 1997, p. 105).

6.2 The Lucas Critique Reconsidered

One of the shortest bridges we can build between the two camps crosses the Lucas critique. It seems reasonable to assume that very few modern Austrians would find the gist of Lucas’s critique of policy analysis based on large-scale econometric models hard to accept. In fact, they would probably regard it as a basic insight, hardly worth mentioning, and essentially contained in the methodological position of von Mises. A point of contention may be, however, the interpretation of the critique as either empirical claim or *a priori* proposition on the limitations of empirical economics. As we have shown in the first part of the thesis, the accepted interpretation within the instrumentalist-positivist camp is the former one. Yet, by giving it a slight twist, we can clarify why modern DSGE models are not really a solution and still vulnerable to the critique. In section 6.2.1, we provide a restatement of the critique along Misesian lines as an *a priori* proposition. In section 6.2.2, we extend the analysis to a more general critique of positivism in economics. We end with a brief note on the potential dangers involved in sticking to the instrumentalist-positivist approach in light of that critique in section 6.2.3.¹⁸

6.2.1 The Misesian Core of the Lucas Critique

We can reformulate the central idea of the Lucas critique under close consideration of the methodological writings of Ludwig von Mises and one of his close intellectual followers, Hans-Hermann Hoppe.¹⁹ According to his biographer, von Mises’s works on the epistemological and methodological foundations of economics are the most neglected and least well understood among his many contributions (Hülsmann, 2007, p. 950; see also Rothbard’s preface to von Mises, 2007, p. 12). Yet, as we will show, his views can help shed new light on one of the

¹⁸Parts of the following sections have been published in Israel (2015).

¹⁹We will focus on the writings contained in von Mises (1933, 1962, 2007, 1998, ch. 1-3) as well as Hoppe (1983, 2007, 2006, ch. 10). It needs to be pointed out that while both authors are members of the Austrian school of economics, their views on methodology are somewhat controversial even among other economists considered to be members of this school of thought. For example, Hutchison (1981) has argued that Friedrich August von Hayek (1899-1992), Nobel laureate of 1974, was more influenced by Popper’s falsificationism, especially in the more mature stages of his career, and he even called Hayek’s convergence to Popper’s position a “U-turn.” Although Caldwell (1992) argues that Hutchison overstated Popper’s influence on Hayek’s methodological thought, it is without doubt that the late Hayek (“Hayek II”) and his intellectual followers disagree with von Mises’s methodology. In his intellectual biography of Hayek, Caldwell (2004, p. 420) claims that even the early Hayek (“Hayek I”) was opposed to Mises’s views. He writes: “Both Hutchison and I agreed that ‘Economics and Knowledge’ [(von Hayek, 1937)] contains a criticism by Hayek of Mises’s position. But I claimed that Hayek had never been a follower of Mises’s apriorist approach.”

most important and acknowledged methodological contributions in modern macroeconomics. It will help us substantiate the claim that New Keynesian DSGE modeling is not a solution to the fundamental problem implied in the Lucas critique, and that the problem will remain as long as the instrumentalist-positivist research program is pursued in macroeconomic analysis.²⁰

Von Mises is a proponent of methodological *dualism*, that is, he believes that economics, which deals with human choice and action, as a discipline is categorically different from the natural sciences, which deal with inanimate objects. Therefore, a different method of inquiry is appropriate in economics. His view is opposed to methodological *monism*, which holds that the same methods used in the natural sciences are also applicable to economics. Advocates of this view consider the positivist approach of hypotheses building and testing against empirical data as adequate across all scientific fields.

Notice, that the importance and adequacy of purely naturalistic analyses of the human body are thereby not disputed. Physiology and modern neuroscience analyze certain functions of the human body from the point of view of natural sciences and follow a positivist approach. Yet, as we have explained earlier, the important elements of the subject matter of economics cannot be empirically determined. We cannot, at least not yet, physiologically explain purposes, goals, motives, ideas, thinking, choice and action. So, there is at least a part of it that must be taken as given - as an "ultimate given." As von Mises (2007, p. 183) points out, humans will always be facing such an ultimate given, which cannot, for the time being, be traced back to its causes. It must be taken as the temporary starting point of scientific inquiry.

Again, this is not some mystical argument against determinism, but simply an acknowledgement of the limitations of our quest for knowledge. Human choice and action might therefore not be free, but we can stay agnostic about those puzzling questions. The fact is that we are still far away from being able to explain them in naturalistic terms. It is impossible to relate actions to any sensible number of ascertainable external factors, in the sense that the action is the inevitable effect of the external factors as the cause. "There is nothing else that could be said about a definite instance of a man's acting and choosing than to ascribe it to this man's individuality" (von Mises, 1962, p. 58).²¹ Individuality then implies an absence of constant

²⁰Again, this is, even if accepted, not a definitive argument against it, but merely a call for caution and modesty.

²¹It is for this emphasis on individuality and the claim that all economic phenomena are the outcome of individual human action "that we can describe the Austrians as the ultimate in microfoundationalists" (Hartley, 1997, p. 107). However, the term microfoundations "is not a word created because it was needed to explain Austrian economics; in fact, from the Austrian perspective it is rather redundant. The foundations of economics are by definition in the microeconomic agents" (Hartley, 1997, p. 119). The important distinction, again, lies in the rejection of precise abstractions, such as the use of a representative agent or firm, which precisely is abstracting from individuality.

relationships between observable variables as the potential causes and other variables that are the products of human action as effects. This lack of constancy in economics poses serious and unavoidable problems for the instrumentalist-positivist research program.²² It is the Misesian core of the Lucas Critique.²³

Empirical relationships observed in the past are subject to change in the future as long as at least one of those variables is the outcome of human action. Yet, relationships of this type are an essential part of all modern macroeconomic models, either in the form of *ad-hoc* assumptions like the price setting behavior of agents (Calvo, 1983), or as derived “rules of choice” from a specified optimization problem. Calibrated DSGE models are then still vulnerable to the Lucas critique, that is, to the invariance of model coefficients, as were the old econometric models. If one still advocates the instrumentalist-positivist approach, it would be important to at least openly acknowledge this fundamental problem and not to claim, as has been done frequently, that the most recent models are no longer affected by it.

This is by no means the only deficiency. Other problems concern the measurability of certain concepts, measurement errors, and the pecuniary and non-pecuniary costs of data collection. However, the most fundamental problem still lies in the way we look at human beings; whether we assume their individuality away by means of *precise* abstractions and look at them much like atoms, or whether we accept their capacity to think, choose and act, and deduce insights from there, using *nonprecise* abstractions that exclude the possibility of precise empirical predictions. On two occasions, von Mises (1962, p. 25; 2007, p. 91) contrasts his views of methodological dualism with the monistic interpretation of philosopher Bertrand Russell, who with some sense of nuance admits that there still is a difference between the behavior of atoms and human beings. However, von Mises decidedly rejects Russell’s views. It is worthwhile quoting both authors at some length to illustrate the conflict. Russell (1997, [1935], pp. 152-153) writes:

²²In his *magnum opus*, von Mises (1998, p. 56) puts it as follows:

The impracticability of measurement is not due to the lack of technical methods for the establishment of measure. It is due to the absence of constant relations. If it were only caused by technical insufficiency, at least an approximate estimation would be possible in some cases, But the main fact is that there are no constant relations. Economics is not, as ignorant positivists repeat again and again, backward because it is not “quantitative.” It is not quantitative and does not measure because there are no constants.

It cannot be overemphasized that this statement refers to economic theory, not to economic history. Measurement and statistical analysis are very important for economic history.

²³Of course, Lucas did not consider himself to be a follower of Misesian economics and this is not what we want to suggest with the phrase “Misesian core of the Lucas Critique.” Lucas acknowledged von Hayek as an intellectual influence on him (Lucas, 1977). Von Hayek, as pointed out in footnote 19, did not accept Mises’s methodological position either, at least in his later career. Strictly speaking, there are two different traditions within the Austrian School, the Hayekian and the Misesian. On this divide see Salerno (1993).

According to quantum mechanics, it cannot be known what an atom will do in given circumstances; there are a definite set of alternatives open to it, and it chooses sometimes one, sometimes another. We know in what proportion of cases one choice will be made, in what proportion a second, or a third, and so on. But we do not know any law determining the choice in an individual instance. We are in the same position as a booking-office clerk at Paddington, who can discover, if he chooses, what proportion of travelers from that station go to Birmingham, what proportion to Exeter, and so on, but knows nothing of the individual reasons which lead to one choice in one case and another in another. The cases are, however, not wholly analogous, because the booking-office clerk has his non-professional moments, during which he can find out things about human beings which they do not mention when they are taking tickets. The physicist has no such advantage, because in his unprofessional moments he has no chance to observe atoms; when he is not in his laboratory, he can only observe what is done by large masses, consisting of many millions of atoms. And in his laboratory the atoms are scarcely more communicative than the people who take tickets in a hurry just before the train starts. His knowledge, therefore, is such as the booking-office clerk's would be if he were always asleep except in working hours.

Russell correctly informs his readers that the booking-office clerk may find out about his clients' reasons to go to Birmingham or Exeter, and that the physicist has no such option when investigating the behavior of atoms. This, however, is not merely due to a lack of communicativeness on the part of the atoms. The clerk does in fact know that ticket buyers have *reasons* for buying certain tickets. He can find out about their motives for going to one place rather than another. The fundamental difference is that he knows that human beings choose and act, and that their motives for choosing and acting in a certain way may change. So, von Mises (1962, pp. 25-26) counters:

It is characteristic of the reasoning of Russell that he exemplifies his case by referring to the mind of a subaltern clerk to whom the unvarying performance of a strictly limited number of simple operations is assigned. What such a man (whose work could be performed as well by a vending automaton) thinks about things that transcend the narrow sphere of his duties is without avail. To the promoters who

took the initiative in advancing the project of the railroad, to the capitalists who invested in the company, and to the managers who administer its operations, the problems involved appear in a quite different light. They built and operate the road because they anticipate the fact that there are certain reasons that will induce a number of people to travel from one point of their route to another. They know the conditions that determine these people's behavior, they know also that these conditions are changing, and they are intent upon influencing the size and the direction of these changes in order to preserve and to increase their patronage and the enterprise's proceeds. Their conduct of business has nothing to do with a reliance upon the existence of a mythical "statistical law." It is guided by the insight that there is a latent demand for travel facilities on the part of such a number of people that it pays to satisfy it by the operation of a railroad. And they are fully aware of the fact that the quantity of service they are able to sell could be drastically reduced one day to such an extent that they would be forced to go out of business.

Bertrand Russell and all other positivists referring to what they call "statistical laws" are committing a serious blunder in commenting upon human statistics, i.e., statistics dealing with facts of human action as distinguished from the facts of human physiology. They do not take into account the fact that all these statistical figures are continually changing, sometimes more, sometimes less rapidly. There is in human valuations and consequently in human actions no such regularity as in the field investigated by the natural sciences. Human behavior is guided by motives, and the historian dealing with the past as well as the businessman intent upon anticipating the future must try to "understand" this behavior.

Statistical and empirical analysis for studying human action, in von Mises's view, is then a descriptive tool, applicable only to historical events of the past. It does not allow for inferring universal "statistical laws." It can neither validate nor falsify economic theory, which should instead be derived deductively from the logical implications of human choice and action. But economic theory can help us understand the past. It can help us to make sense of, for example, a negative correlation between price inflation and unemployment over certain periods of time and a positive link between them in other periods, or with a certain time lag between the time series. The monetary theory of the business cycle, developed in von Mises (1953), can explain

the empirical phenomenon of the short-run Phillips curve through the initial boom period that follows central bank induced credit expansion.²⁴

It would however be false to declare the empirical observation of a Phillips curve to be a necessary consequence of any monetary expansion. The observable reality is complex and several forces work at the same time in the same or opposite directions. Technological progress and innovation can have an impact on both employment and prices and change the empirically observable time series and their correlation. Economic theory therefore cannot make exact scientific predictions of the form: policy X will lead to an increase or decrease of variable Y by an amount of Z, as long as Y is the result of individual action. We will now go into a more recent and improved justification of this position.

6.2.2 A Critique of Positivist Economics

However compelling or non-compelling von Mises's views on the foundations of economic theory might be for any contemporary reader, it is true that he did not provide a sufficiently elaborated logical argument for his rejection of monism. According to Hoppe (1983, p. 8), it was Karl R. Popper (1902-1994) of all thinkers, by many seen as the intellectual father of modern positivism, who unwillingly provided the groundwork for such an argument. It was Hoppe's achievement to reconstruct and incorporate this argument into the Misesian framework.

In the preface of Popper (2002, [1957]), we find the crux of his argument in refutation of historicism (in particular, the views of Marx and Spengler as he points out in the preface to the German language edition): because human knowledge can grow and we cannot scientifically predict the state of our future knowledge, but our knowledge on the other hand influences the path of history, "there can be no scientific theory of historical development serving as a basis for historical prediction." This insight, however, can not only serve as a basis for the refutation of historicism, but also as a justification for the Misesian claim that there is a lack of constancy in human action. It is after all human action that shapes the path of history. The argument implies a logical refutation of the positivist approach to economics. But let us now recapitulate Hoppe's argumentation step by step.

The first step of the argument lies in demonstrating the necessity of the *constancy principle* for the positivist approach of hypothesis building and testing. It is in principle only possible to

²⁴We will present the Austiran Theory of the Business Cycle in chapter 7 and then apply it to the explanation of the relationship between historical time series data in chapter 8.

falsify a hypothesis if we assume constancy in the relationship between the observable causes and effects: *the same configuration of causes produces the same effect, and differences in the effects imply different causes.*

If we employ the same econometric method to identify a relationship between one “explained” variable as effect and one or more “explanatory” variables as causes in two different data sets, and find that the relationships are different in the two sets, then we implicitly assume the constancy principle, when we conclude that there must have been at least one ignored causal factor at work in the generation of one data set, but not or not to the same extent in the generation of the other. This is exactly what the positivist does when he revises his initial hypothesis in the hope of correctly incorporating those missed factors.

However, this constancy assumption can neither be falsified nor verified by experience. If we observe two different effects, we cannot exclude the possibility of eventually identifying different causes, in accordance with that assumption, that produced the different effects. If we want to conclude from observing two equal effects that their causes must have been equal, we implicitly assume the constancy principle. If we do not assume it, then, from observing equal effects nothing follows about the causes, and for a verification of the principle we would have to empirically investigate all potential causes in the whole universe and identify them as being equal, which is in principle impossible (Hoppe, 1983, pp. 11-12). Now if this is so, then how can one justify the Misesian claim that the constancy principle does not hold in the realm of human choice and action? It can only be justified through a logical argument.

The second step consists of such a logical argument. As Popper claimed, human knowledge can change and we cannot scientifically predict our future state of knowledge, that is, in other words, we can learn new things and we cannot scientifically predict what we will learn in the future, not even the near future. If we could, it would precisely not be learning, since we would have to know what we will “learn” beforehand.²⁵ The statement that human beings learn is again not falsifiable, since its falsification would precisely be an act of learning. The truth of the statement, like for any other logically true statement, is not dependent on experience. It is *a priori* true. One cannot argumentatively deny the truth of the statement that human beings learn, without implicitly assuming its truth, since any argumentation presupposes the possibility

²⁵In fact, knowledge can not only grow, but we could also lose or forget already acquired knowledge. Every student of any subject who is not exceptionally gifted knows that acquired pieces of knowledge may get lost quickly, if they are not repeatedly and actively kept in mind. On a larger scale, the Renaissance, for example, is considered to be a period in Western history that is marked by a rebirth of antique Roman and Greek culture and knowledge that had been lost or forgotten during the Middle Ages.

of answers and rebuttals that are contingent on the arguments presented - even if it is only an affected: “Okay, I see you are right.” And then in fact someone would have learned something, which is a contradiction, since we cannot learn that we cannot learn. We may in fact consider every conscious sense experience and every thought as an act of learning - learning the fact that a certain sense experience was made or that a certain thought occurred in our mind at definite points in time and space. And not only positivist but all inquiries in the social sciences have the declared purpose of learning something about the world.

Human action, defined as the employment of means to attain chosen ends, is contingent on our knowledge and beliefs over what means are suitable to attain certain ends. If knowledge is not scientifically predictable and human action is contingent on knowledge, then human action itself is not scientifically predictable. The constancy principle applied to the field of human action would imply that human beings cannot learn. It would imply that human beings cannot incorporate new information and knowledge into their choices and actions. Since this is an indefensible position and human beings can in fact learn, the constancy principle must be rejected as false in the realm of human action by logical contradiction.

The third and final step is simply the logical combination of the first two steps. The constancy principle is the necessary condition for falsification, and thus for the positivist approach in general. Yet, the constancy principle must be rejected if the “explained” empirical phenomenon is the result of human action. This is because human beings learn. Hence, the positivist approach in economics is contradictory.

Strictly speaking, we could not even think of certain individuals as literally being in a state of “non-learning,” a state of “evenly rotating” daily routine for a time period of some length, however intuitively illustrative this description might seem in certain cases. The idea of “non-learning,” of no change, becomes even more absurd for any person over the span of her lifetime, or for all individuals forming a society over time, from one generation to the next. There is significant change in the ends we value, in our knowledge about the means suitable to attain these ends, in fashion, in ideology, and in our culture in general. In particular, there are changes in the way we are dealing with money, not exclusively because of technological progress and learning, but also because of the changing political and societal environment into which we are born.²⁶ It is therefore rather inappropriate to treat macroeconomic time series of the past 100 years or more as being generated by some homogeneous and self-repeating mechanism that would allow for

²⁶For a description of the cultural changes in an inflationary environment for example, see Hülsmann (2008, ch. 13) and Hülsmann (2013, ch. 10).

generalizations of the kind commonly drawn in modern macroeconomics, the naive interpretation of the Phillips curve trade-off being the most prominent example. To the contrary, the lack of constancy explodes the basis for this positivist research program.

6.2.3 The Danger of Relying on the Instrumentalist-Positivist Program

The important conclusion that we tried to convey in the previous passages is that the hard core of the Lucas critique, carved out using the epistemological and methodological contributions of von Mises and Hoppe, remains relevant for any model of modern macroeconomics discussed in the first part of the thesis, even those advocated by Lucas himself. It should therefore not come as a surprise that more traditional models as well as modern DSGE models are all used side by side in public policy institutions, including central banks, and that at least some researchers were reluctant to incorporate the alleged lessons from the Lucas critique into their models (Ericsson and Irons, 1995; Crucolini, 2010; Goutsmedt et al., 2015), as the radical implication would have been to abandon their positivist foundations altogether, if one really wanted to avoid the problem. Ultimately, all of these models suffer from the same deficiency, but some might be more useful than others under certain conditions. The critique as understood and refined above is not so much a critique of any particular group of modern macroeconomic models, but rather one of the entire positivist research program.

Yet, some clarification might still be necessary. Even though scientific monism is from the point of view of the learning human actor a path riddled with contradictions, we can of course not eliminate the possibility that all phenomena ultimately follow the same laws and that there is in fact a monistic structure underlying everything, the external world as well as the human mind. As von Mises (2007, p. 1) writes:

Mortal man does not know how the universe and all that it contains may appear to a superhuman intelligence. Perhaps such an exalted mind is in a position to elaborate a coherent and comprehensive monistic interpretation of all phenomena. Man - up to now, at least - has always gone lamentably amiss in his attempts to bridge the gulf that he sees yawning between mind and matter, between the rider and the horse, between the mason and the stone. It would be preposterous to view this failure as a sufficient demonstration of the soundness of a dualistic philosophy. All that we

can infer from it is that science - at least for the time being - must adopt a dualistic approach, less as a philosophical explanation than as a methodological device.

The argument presented here is valid only from the perspective of a learning human being. Since presumably we all belong to that group, it carries some weight. However, it is entirely possible to acknowledge the problem and nonetheless stick to the instrumentalist-positivist approach. One might say that even though the strict constancy principle is not satisfied, the errors in the models we develop are sufficiently controlled, or the lack of constancy has only a minor effect on the usefulness of the models. This, however, is merely an assertion.²⁷ The transition from traditional models to microfounded DSGE models can be seen partly as an instance of this attitude. However, if the alleged solution to the Lucas critique is really only superficial, as both empirical research and logical argument suggest, what is the practical danger of sticking to the approach?

The problem is that the program itself contains no substitute for more or less arbitrary judgment on what kind of models and empirical evidence should be given more weight in guiding policy decisions. Strictly sticking to the program, it should be the model with the best record of predicting the relevant economic magnitudes. Yet, there is no way of knowing whether this model will also remain the best in the future, and, in fact, what exactly qualifies as “best” in the first place.²⁸ It is a matter of judgment on the part of the economists, statisticians, and political authorities. We can argue that given the above discussion, a quantitative macroeconomic model is in an important sense always *overfitted*, because it is necessarily built on the *particular* circumstances of a certain region and period empirically manifested in the data that was used for its estimation or calibration. When and how this becomes a problem is again a matter of subjective judgment.

This is not adequately acknowledged, certainly not, when claims are made to the effect that the problem indicated by the Lucas critique is no longer relevant. It, moreover, opens opportunity for a public choice analysis of the incentives faced by monetary policy authorities, incorporating concepts like *moral hazard*. To the extent that monetary policy measures are not transparently grounded in accepted and objective criteria, but are the outcome of expert judgment, one has to

²⁷And strictly speaking, being able to probabilistically control error likewise requires constancy of the form that action can be described by well-defined probability functions, which can also be rejected by means of the argument from learning. This is the reason why the probability approach in econometrics presented in Haavelmo (1944) is not fundamentally different from Frisch’s conception of econometrics and does not solve the problem.

²⁸It could be a model that yields the lowest average deviation between predicted and observed values, a model that is very close most of the time but sometimes very far-off, or a model satisfying many other conditions deemed important.

ask whether the measures taken might not simply be in the best interest of the experts instead of the public.²⁹ And it might of course be questioned whether something like a homogeneous public, in a relevant sense, exists in the first place.

Finally, using econometric analyses within the instrumentalist-positivist program for making generalized claims about how central bank policy should be conducted is problematic on two accounts. First, it can only focus on observable and measurable factors. Any abstract notion like *systemic risk* or *moral hazard* can only be incorporated into the analysis by means of empirical proxies. But as we pointed out for the estimates of the unobservable *natural rate of interest* in the previous chapter, empirical tests can never at the same time test the model as such as well as the adequacy of the proxy.

Second, if empirical prediction is made the benchmark for economic theory, then there will be an inherent bias towards short-term analyses, since predictions are usually better when they concern the near future instead of the long term. This is one reason why the short-term benefits of monetary expansion are analyzed well within the instrumentalist-positivist approach, but the long-term costs as explained in the following chapter tend to be overlooked.

6.3 Econometrics as Purely Descriptive

The strong criticism raised by some Austrian economists does not imply that econometric tools and statistical analyses are without merit. It simply suggests that their use should be narrowed down to historical studies. Econometrics should be purely descriptive, avoiding inductive generalizations. Interestingly, what we might call the “Austrian conception of econometrics” is compatible with Pawel Ciompa’s original definition of the term (Ciompa, 1910), which has to our knowledge never been discussed in any detail in the relevant literature. We will take the opportunity to briefly introduce his conception of econometrics in section 6.3.1 and show how it is compatible with Austrian economics in section 6.3.2.³⁰

6.3.1 Pawel Ciompa’s Conception of Econometrics

Polish economist Pawel Ciompa (1867-1913) was a professor at the Higher School of Economics in Krakow and director of accounting at the federal state bank of Galicia based in Lemberg. The full name of his home region was Kingdom of Galicia and Lodomeria and the Grand Duchy

²⁹We will also discuss this problem in the next chapter.

³⁰Parts of the following sections are contained in Israel (2016).

of Krakow with the Duchies of Auschwitz and Zator. It was also known as the Halychyna or Austrian Poland and was part of the dual-monarchic Austro-Hungarian Empire at that time. Today the region belongs partly to Poland and partly to the Ukraine. Its former capital Lemberg is today called Lviv and part of western Ukraine, whereas Krakow lies in Southern Poland.

Ciompa's conception of econometrics seems to be long forgotten, if it had ever exerted any noteworthy effect on the broader discipline of economics at all. There is no entry on Ciompa in any of the major English, French or German language encyclopedias. There is only a very short entry in the first volume of the third edition of the Polish *Wielka Encyklopedia Powszechna PWN* from 1983 (Wiśniewski, 2016, p. IX). Yet, in the more recent editions of its successor, the *Wielka Encyklopedia PWN*, the entry on Pawel Ciompa has been removed.³¹ It is true, his name is sometimes mentioned when it comes to the origins of the term "econometrics", but usually not more than the equivalent of a brief footnote is devoted to his work. Not even Ragnar Frisch, the originator of econometrics in the modern sense (Bjerkholt, 1995), who famously defined the "new discipline" in 1926, was initially aware of Ciompa's work and his use of the same term for something rather different some sixteen years earlier. Frisch has admitted this in a brief note in his journal *Econometrica* (Frisch, 1936).

It was professor Tomasz Lulek of the University of Krakow, who informed the editor of *Econometrica* about the fact that the term econometrics has already been used and defined by Ciompa in 1910 as the "geometrical representation of value",³² which was considered to be closely related to the principles of accounting. It is interesting to note that Frisch (1936, p. 95) lamented:

It still seems, however, that, taken in the now accepted meaning, namely, as the unification of economic theory, statistics, and mathematics, the word was first employed in the 1926 paper [(Frisch, 1926)]. Pawel Ciompa seems to emphasise too much the descriptive side of what is now called econometrics.

In the above quote, Frisch in effect points to the fundamental difference between the two conceptions of econometrics.³³ Ciompa published his German language book *Grundrisse einer Oekonometrie und die auf der Nationaloekonomie aufgebaute natürliche Theorie der Buchhaltung* (Outline of econometrics and the natural theory of accounting based on economics,

³¹We thank Dr. Łukasz Dominiak from Nicolaus Copernicus University in Toruń, Poland, for his assistance in this inquiry.

³²In the German original: "die geometrische Darstellung des Wertes" (Ciompa, 1910, p. 5).

³³For a discussion of the Frischian conception, go back to section 2.1.1 on page 42 in the first part of the thesis.

from now on simply referred to as *Grundrisse*) in 1910, in which he defended an economic, as opposed to a juridical, approach to the theory of accounting (Mattessich, 2008, p. 270). In the *Grundrisse*, one finds the earliest mention and definition of the term econometrics, more precisely, its German language equivalent “Oekonometrie” (also “Ökonometrie”), as far as we know. Ciompa describes the new term vividly:

Just like mechanical, acoustical, dynamic, and other such phenomena in physics, and mass phenomena in geometry, also economic phenomena should be represented and displayed following a doctrine, which I envision as a sort of *economographics*. This *economographics* would constitute a descriptive economics; it would have to be based on economics, mathematics and geometry. The foremost task of such a doctrine would be the geometrical representation of value. This part of *economographics* I call *econometrics*. The practical application of econometrics to the mathematical representation of values and their changes would be accounting. Put differently, econometrics would then just be the theory of accounting. [emphasis added]³⁴

Econometrics in Ciompa’s vision, and what it adds to the more general field of economics, would therefore be strictly descriptive in nature. Its purpose is to describe and depict the changes and evolution of economic values. More specifically, he thought of it as being a collection of mathematical and graphical tools by which we could describe and depict the evolution of assets and liabilities in business accounting. He further explains the scientific status of *economographics* and *econometrics* as follows:

Similar to trigonometry which is a subfield of geometry, *econometrics* would be a subfield of *economographics*. Accounting and *econometrics* would be related to

³⁴All quoted passages from the *Grundrisse* have been translated into English to the best of our knowledge. In the German original (Ciompa, 1910, p. 5) we read:

Wie die mechanischen, akustischen, dynamischen und dgl. Erscheinungen durch die Physik oder wie die Massenerscheinungen durch die Geometrie, so sollten auch die volkswirtschaftlichen Erscheinungen durch eine Lehre, die ich mir als eine Art Oekonomographie vorstelle, zur Darstellung gebracht werden. Diese Oekonomographie waere eine Art darstellende Nationaloekonomie, sie muesste auf der Nationaloekonomie, Mathematik und Goemetrie aufgebaut werden. Einer solchen Lehre wuerde dann vor allem die geometrische Darstellung des Wertes zufallen. Diesen Teil der Oekonomographie nenne ich Oekonometrie. Die praktische Anwendung dieser Oekonometrie auf die mathematische Darstellung der Werte und deren Veraenderungen ist dann die Buchhaltung. Umgekehrt ist dann die Oekonometrie nur die Theorie der Buchhaltung.

In the following, the original German quotations only are provided, when our English translations run the risk of becoming too inaccurate for a proper understanding of what Ciompa actually wrote.

each other just like mathematics and algebra. (pp. 5-6)

This latter comparison is very thought-provoking. Algebra, in its broadest sense, combines elements from almost all of mathematics. It provides us with the rules of how to manipulate mathematical symbols and formulas in general, and so it enters into almost any other subfield of mathematics. It contains the basic armamentarium that any layperson interested in mathematics, and *a fortiori*, any professional mathematician, needs in order to communicate mathematical results to an audience. In this sense, algebra might be understood as containing the tools of communication for mathematical insights and knowledge. In very much the same way, it seems, Ciompa considered econometrics to be the apparatus which we should use to communicate and illustrate information in accounting.

In order to clarify the subject matter of business accounting, and hence of econometrics, Ciompa starts the *Grundrisse* with a brief discussion of economic value theory and the theory of goods. Although he is a bit imprecise in his exposition, we find several classical definitions and well-known classifications of economic concepts: a good is simply defined as any means, material or immaterial, that is conducive to the satisfaction of human wants. Goods are classified into *economic goods* that have an exchange value, *free goods* that have no exchange value, and *services*, which are defined as any outflow of human activity that has an exchange value (pp. 1-2).³⁵ Ciompa points out, that only economic goods, which according to his own definitions subsume services, are relevant for business accounting.

Next, Ciompa makes the twofold distinction of value into *use value* and *exchange value*, both of which can be either of *subjective* or *objective* nature.³⁶ From these distinctions he obtains the terms *subjective use value* that is assigned to a good in accordance with the subjective importance of the wants that it satisfies, *subjective exchange value*, which is assigned to a good in accordance with the importance of the wants that can be satisfied by those goods that can be obtained in exchange for it, *objective use value* that describes the factual or natural scientific potential of a good to satisfy wants, and *objective exchange value*, which is simply the market value or the price of the good as the result of the subjective evaluations of buyers and sellers. Ciompa

³⁵Obviously, according to this classification, services are just a subgroup of economic goods. Technically speaking, the classification is therefore not a partition. It is not clear why Ciompa mentions services at all on this level of his classification scheme, but this should not disturb us too much. In the remainder of the book, he is almost always only referring to economic goods, which includes services.

³⁶This classification scheme is known for example from Ciompa's fellow countryman Carl Menger (2007, [1871], pp. 121ff. and 226ff.), but at least the distinction between use and exchange value has a much longer tradition in the history of economic thought, for example over von Pufendorf (1744) to the scholastic doctors of the Middle Ages, all the way back to the old Greeks (Rothbard, 2006a, ch. 1; Schumpeter, 2006, part II, ch. 2).

further points out, that value is not inherent in the good, but contingent on needs, inclinations, the economic situation of the individual, and also the social environment. It is therefore subject to constant change, and the only way to reliably gain information about the value of a good is through exchange. It is then the objective exchange value with which accounting is primarily concerned (p. 4).

What Ciompa defines as the normal value (*Normalwert*) of a good or service corresponds to its cost of production. A rent or a profit is earned whenever the realized objective exchange value on the market exceeds the normal value. A loss is incurred whenever the realized objective exchange value is below the normal value of the good.

In order to be able to uniformly quantify and express objective exchange values, profits and losses, Ciompa has emphasized the importance of money and money prices. He simply defines money as just another good, and prices as proportions of values, and here surely means objective exchange values.³⁷ He writes:

This proportion of values between two goods we call price. The price, therefore, is nothing other than the expression of the value of one good in terms of the value of another; usually the price is expressed in money. The value of money is not stable, but changes, as does the value of any other good, because money itself is a good. (p. 9)

The next fundamental distinction that Ciompa makes is between wealth (*Vermögen*) and capital (*Kapital*). Wealth is understood as the set of all economic goods that one freely disposes of (p. 9). Wealth turns into capital if it is put to some productive use. Therefore, capital is understood as productive wealth, or as the resultant of the “combination of wealth and labor” (p. 10).³⁸ It is then clear that we might also think about capital as being a subset of wealth. There

³⁷This is another example of an imprecision in Ciompa’s exposition. Although he made the above mentioned distinctions into different kinds of values, he is in the remainder of his book usually only writing about “the value” as if there were only one. These inaccuracies notwithstanding, here again, we might argue that Ciompa follows his countrymen Carl Menger and Eugen von Böhm-Bawerk, although he is not referring to them and their work directly. Böhm stated that “organized exchange gives almost every good a second value” (von Böhm-Bawerk, 1930, [1891], p. xxxiii), which is of course the exchange value, and then bluntly emphasized the indissoluble connection between price and exchange: “The law of Price, in fact, contains the law of Exchange Value” (p. 132). Furthermore, Menger (2007, [1871], p. 257) has presented an account of the emergence of money as just another good with certain properties from the original state of barter exchange.

³⁸The careful reader realizes that at least a person’s own labor according to the proposed definitions must be seen as a part of wealth, as it falls under the definition of services and therefore is an economic good.

can be no capital without wealth, but there can be wealth without capital.³⁹ Wealth is, in fact, “the basis for all economic life” (pp. 9-10).

Economic life in Ciompa’s vision is composed of economic actions (*wirtschaftliche Handlungen*), all of which are directed towards the use of some economic goods, or some capital, in order to consume or create new wealth and capital. He is now able to pin down the proper function of accounting and econometrics as follows:

The task of accounting is to mathematically calculate, and econometrically illustrate the yields from wealth and capital as results of economic life. Economic life consists of processes and actions, which use up old wealth v_1 in order to create new wealth v_2 . As a result, the initial endowment of wealth V_1 diminishes, and it remains a wealth of $V_2 = V_1 - v_1$, which then increases to the wealth $V_3 = V_2 + v_2$. The same holds true for the capital of this wealth; in the first instance, it simultaneously diminishes $C_2 = C_1 - c_1$; in the second instance, it simultaneously increases with the wealth: $C_3 = C_2 + c_2$. (p. 14)

In Ciompa’s view, economic life can be broken down into individual economic actions like the one schematically described in the above quotation. In order to graphically represent economic actions, he developed what he called the *econometric Quadrigon*. He also made use of standard wealth and capital accounts representing economic actions.⁴⁰

At this point our brief digression shall stop. A more detailed exploration of Ciompa’s econometrics is not necessary in order to retain the essential point that the original Ciompanian conception of econometrics is entirely descriptive and relates only to actually observable magnitudes, namely money prices. Hence, we can reconcile the Misesian position on methodology and Ciompa’s econometrics, and we may even extend the latter into a broader notion of descriptive econometrics, akin to what Ciompa baptized economographics.

³⁹It is clear that in practice, and from a broader perspective, it might be very difficult to decide what a productive use of wealth is - an argument which has been brought forward against the idle resources argument for macroeconomic policy interventions during crises and recessions. However, for Ciompa’s purposes, it seems, the concepts are sufficiently well defined, as we could relatively easily find out which economic goods are used, and therefore constitute capital, in any specific and well-defined economic action, for example, the production of a wooden chair.

⁴⁰The interested reader can find more about the *econometric Quadrigon* and Ciompa’s accounting scheme in Israel (2016).

6.3.2 Econometrics as a Tool for Economic History and Accounting

For Ciompa (1910) econometrics was merely an application of economic theory, not a way to develop it further. In sharp contrast, the modern Frischian conception of econometrics, which has been discussed in the first part of the thesis, calls for a reformulation, even a genuine transformation, of economic theory in mathematical and quantifiable terms. One essential role of statistical analysis (the *empirical-quantitative*) within this modern conception is the verification or falsification of theoretical propositions (the *theoretical-quantitative*). However, Austrian economists von Mises and Hoppe have emphasized that the necessary condition for inductive statistical analysis is the *constancy principle* as explained above. There is ample reason to question the validity of this principle for the problems that econometrics typically deals with, at least on the rather superficial level of empirical detail and accuracy that it is capable of achieving.

Although the two conceptions of econometrics share a common element, namely the descriptive side of statistical analysis, it is the inductive side of modern econometrics that puts them far apart. The possibility of inductive generalizations from any empirical analysis in economic theory is decidedly denied by von Mises and his followers:

History cannot teach us any general rule, principle, or law. There is no means to abstract from a historical experience a posteriori any theories or theorems concerning human conduct and policies. (von Mises, 1998, p. 41)

But this is not to say that statistics cannot teach us anything at all. Quantitative-statistical analyses deal with and condense information about the particular empirical manifestations of economic phenomena that are changing all the time. Elsewhere, we read:

In the field of human action statistics is a method of historical research. It is a description in numerical terms of historical events that happened in a definite period of time with definite groups of people in a definite geographical area. Its meaning consists precisely in the fact that it describes changes, not something unchanging. (von Mises, 2007, p. 89)

So we might extend Ciompa's econometrics, which deals with market prices of economic goods, profit and loss, specifically from the perspective of a business firm, to the statistical analysis of any measurable economic magnitude on any level of aggregation, such as unemployment

rates, price inflation rates, or asset price indexes. It would be the role of the extended Ciompanian econometrics to take account of the historical development of any such magnitude of interest. Such a broader notion of descriptive econometrics would be fully compatible with the Misesian stance on methodology. To the extent that modern econometric studies engage in descriptive analysis, they are too. To the extent that they go beyond historical accounting and description, they are not.

Descriptive econometrics as part of economic history can help us *speculate* about what causal factors contributed how much to observed historical phenomena, such as financial crises. They can help us determine whether stated public policy goals have been achieved or not. But it is economic theory that informs us about causal links and whether specific public policy goals are feasible in the first place.

Chapter 7

The Theory of Money and Monetary Policy

In the previous chapter, we have put the causal-realist approach to economics, and specifically the Austrian stance on methodology, into perspective. This chapter will present some of the central insights into monetary theory and policy from the modern Austrian school. Section 7.1 is devoted to the nature of money and its different forms. Next, we will analyze the general consequences of expansionary monetary policies, more precisely, central bank induced credit expansion. In particular, two aspects will be discussed in more detail: business cycle fluctuations in section 7.2.1 and redistributive effects of money creation in section 7.2.2. Finally, in section 7.3, we will elaborate on the link between central banks and commercial banks, and how the former institution contributes to *systemic risk*, *moral hazard*, and hence the relative instability of financial markets.

7.1 Money and Its Different Forms

In order to obtain a systematic analysis of the costs and benefits of central bank monetary policy, we have to reflect on the entity that is politically controlled by central banks, namely, money.¹ What is money, how is it used, and in what different forms does it exist? Answers to these questions have strong repercussions on the theoretical investigations that follow. As von Mises (1998, p. 38) points out: “In the concept of money all the theorems of monetary theory are already implied.” Hülsmann (2000, p. 26) adds that “[v]irtually all issues relating to monetary

¹By “politically controlled” we mean that money, at least base money, is essentially provided by a public policy institution that holds a legal monopoly. Its production is not merely politically regulated.

theory and policy, for instance, can be discussed by reference to the nature of money.” We thus have to devote some space to its exploration.

7.1.1 The Nature of Money

In the *Theory of Money and Credit*, von Mises classified money as being neither a consumer good nor a producer good, but as belonging to a third and distinct class of goods that he called media of exchange.² Adopting Menger’s, or for that matter Ciompa’s, definition of economic goods as being scarce and hence having an exchange value (Menger, 2007, [1871], pp. 94ff.), a medium of exchange is an economic good that is acquired but not intended to be used directly, neither in consumption nor production, but to be exchanged at some point for another economic good. It is thus used in indirect exchange. Money is a commonly accepted medium of exchange within a certain community.

What exactly “common” means in this context is not important for the theory of money. It is sufficient to have an exact definition of medium of exchange, since all the theoretical results for media of exchange apply also to money.³ Rothbard (2009a, p. 193) simply refers to all media of exchange as money. We can certainly stipulate without much controversy that for example the US dollar, the British pound and the euro are commonly used media of exchange and hence moneys.

Money is necessary in any complex economy based on private property, the division of labor and free exchange, even if regulated to some extent. Primitive economies without an extended network of exchanges, or economies under a socially planned division of labor and distribution of products, that is, common ownership in the means of production and the produced consumption goods do not require money (von Mises, 1953, p. 29). The latter serves its primary function of facilitating exchange in the context of a complex market economy:

²The discussion can be found in von Mises (1953, ch. 1). On this see also von Mises (1998, ch. 17) and Rothbard (2009a, ch. 3).

³Whether or not a medium of exchange is referred to as money is a matter of historical judgment. As von Mises (1998, p. 395) explains:

A medium of exchange which is commonly used as such is called money. The notion of money is vague, as its definition refers to the vague term “commonly used.” There are borderline cases in which it cannot be decided whether a medium of exchange is or is not “commonly” used and should be called money. But this vagueness in the denotation of money in no way affects the exactitude and precision required by praxeological theory. For all that is to be predicated of money is valid for every medium of exchange. It is therefore immaterial whether one preserves the traditional term theory of money or substitutes for it another term. The theory of money was and is always the theory of indirect exchange and of the media of exchange.

The balancing of production and consumption takes place in the market, where the different producers meet to exchange goods and services by bargaining together. The function of money is to facilitate the business of the market by acting as a common medium of exchange. (von Mises, 1953, p. 29)

Money overcomes the problem of the *double coincidence of wants* that emerges in direct exchange, for which a person willing to trade a good or a bundle of goods *A* against another good or a bundle of goods *B* has to find a person with exactly reversed valuations, willing to trade *B* against *A*. If all economic goods are commonly traded against money, *A* can be sold for money, and the acquired money can be used to buy *B*, without having to find any one individual person or group that happens to be willing to buy *A* and at the same time offers *B* for sale. Moreover, even if the double coincidence of wants is given, it might not be possible to divide the goods that are to be exchanged into adequate units that accommodate the subjective evaluations of the potential trading partners and render a mutually beneficial exchange possible. Money is typically highly divisible and solves this problem too.

Indirect exchange and the implied use of a medium of exchange or money therefore render more mutually beneficial exchanges possible than would be the case exclusively under direct exchange. Hence, the use of money extends the division of labor, fosters specialization, increases physical productivity, and “thereby contributes to the material, intellectual, and spiritual advancement of each person” (Hülsmann, 2008, p. 22). This is a conclusion from the mere existence of money, without reference to its quantity or specific form. It is drawn from or implied in the nature of money as a commonly used medium of exchange.

Other functions of money have been classified as corollaries or “secondary functions” to facilitating trade as a medium of exchange (von Mises, 1953, pp. 34ff.). These include money as a *unit of account*, a *facilitator of credit transactions*, and money as a *store of value*. Money can serve as a good unit of account because as a commonly accepted medium of exchange it is traded against virtually all other economic goods on the market. Hence, there exist money prices, or expressions of what has been called objective exchange value for almost all other economic goods in terms of money.⁴ While not being a measure of value in the subjective sense of the

⁴The attribute “objective” does of course not imply that this value is fixed or constant. To the contrary, it is constantly changing as Ciompa (1910, p. 9) pointed out. Measuring and accounting for its historical development is precisely the role that he ascribed to his descriptive notion of *econometrics* as discussed in the previous chapter. We do not go into how these objective exchange values or money prices form on the market. The marginal pairs analysis of price formation has been presented in von Böhm-Bawerk (1930, pp. 203ff.). It is accepted by most modern Austrian economists. For a pedagogical exposition see Rothbard (2009a, pp. 238ff.) and Egger (1998).

word, money could be called a measure of objective exchange value.

Likewise, while money is not necessary for credit transactions, it is obvious how it can facilitate them by virtue of being commonly accepted as a medium of exchange.⁵ Just like it renders more mutually beneficial exchanges in general possible, money specifically renders more mutually beneficial credit transactions possible, since the loan amount received as well as the repaid loan amount plus gross interest rate can be exchanged against virtually any desired economic good at the ongoing market price. Instead of agreeing on a specific set of economic goods in terms of which the credit contract is fixed, it can be negotiated in terms of money. Finally, when a good becomes gradually more accepted in exchange, it thereby gains value and becomes more useful as a store of value, that is, as a means of preserving purchasing power over time.⁶

Any good to be used as a medium of exchange needs to have some value in the first place, that is, there has to exist some demand for it. It can become money only on the basis of that preexisting non-monetary value. The latter allows an estimation of the demand for the good in the future and makes it possible that additional demand for the good as medium of exchange emerges. The good can then become a money as more and more people use it as a medium of exchange and hence increasing *monetary demand* renders the good more and more marketable. The monetary demand adds to the non-monetary demand, which may in comparison be negligible. It may even completely vanish as is the case in modern day fiat money systems.

This is the gist of the Mengerian-Misesian theory of the origins of money.⁷ In combination

⁵On the claim that credit transactions are possible without money, see Graeber (2014), who argues that credit has indeed historically existed before money. One has to emphasize, however, that the so-called *credit theory of money* (Mitchell-Innes, 1914) that Graeber seems to support and according to which money is essentially the same thing as debt or credit, has serious flaws. That money is not the same thing as debt can be seen from the historical existence of commodity money systems in which money was a commodity like silver, or effectively backed by it. The existence of the commodity and hence its use as money in such a system is independent of the existence of debt. Yet, the conception of money as debt is not completely without merit, especially in our modern financial system, in which new money is indeed often created in the form of credit. We will dwell on this point in section 7.2. But even for our current system that emerged out of a commodity standard, it would be wrong to regard all money as representing debt. The cash money in circulation, for example, does not represent debt in the economic sense of the word, although it is classified as a liability in central bank balance sheets.

⁶In the words of Menger (2007, p. 280) the above functions of money are “merely accidental”:

But it appears to me to be just as certain that the functions of being a “measure of value” and a “store of value” must not be attributed to money as such, since these functions are of a merely accidental nature and are not an essential part of the concept of money.

⁷On this theory, see Menger (1892), Menger (2007, pp. 257ff.) and von Mises (1953, pp. 30ff.). A note of clarification might be required here. This theory is not a historical account of how money emerged out of barter in a free society. It is a logical or praxeological reconstruction of how money emerges. It does not deny the possible role of a powerful entity like the state in bringing about a particular kind of money. It is in fact compatible with historical accounts of how states have established certain moneys. It would even be compatible with a completely hypothetical

with von Mises's *regression theorem* (von Mises, 1953, ch. 2; von Mises, 1998, pp. 405ff.), it allowed to solve a challenge posed by Helfferich (1903), who pointed out that Austrians had not yet solved the circular reasoning in the determination of the purchasing power of money: Money is demanded in trade because it has purchasing power and it has purchasing power only because it is demanded. Adding the time dimension, expectations and the Mengerian insight that some initial non-monetary value had to exist in the first place allowed von Mises to break out of the circle.

Money's future value or purchasing power, that is, the array of money prices for goods and services on the market, is determined by the current total demand for money as well as the total stock, which we assume for the time being to remain constant. The current demand for money is based on the expectations of the future purchasing power of money that is formed on the observed purchasing power of money in the past, and potentially other factors like observed and expected changes on markets for other goods. In this way, the determination of the purchasing power of money can be traced backward in time to the point when there existed only the initial non-monetary value of the good that later became money.

Thus the value of money as a commonly accepted medium of exchange can be explained, just like the value of any consumer or producer good, on the basis of the theory of marginal utility. This integration of money into the general theory of value is seen as one of von Mises's greatest theoretical achievements.⁸

7.1.2 Different Forms of Money and Its Production

Historically many different kinds of money have existed. Precious metals like silver, copper and gold have often served as *commodity moneys*. We have already mentioned *high divisibility* as

scenario of the following kind: "The state *X* has introduced and enforced a law according to which every citizen has to pay two green pieces of paper (as such virtually without value) every month. Every citizen received an initial endowment of 10 green pieces of paper. The state offers additional pieces in exchange for all kinds of goods and services on the market. After some time the green pieces of paper circulate as commonly accepted media of exchange, that is, money." This would not be a violation of the praxeological theory of the origins of money mentioned above, which simply argues that there has to be an initial non-monetary value attached to the good before it becomes money. This value could be "created" or decisively determined by government force. The *state theory of money*, according to Knapp (1905), then has some merit understood as a historical account of the emergence of particular kinds of money. After all, state involvement in money has been very widespread. Yet, it does not capture the nature of money. State involvement is not a necessity for the emergence of a commonly accepted medium of exchange.

⁸Von Mises's contribution is partly based on the work of Friedrich von Wieser (1851-1926), a fellow Viennese economist, who had incorporated the time dimension, that is, the past value of money, into the explanation of the current value of money before von Mises (von Wieser, 1909). For a more detailed discussion, see Hülsmann (2007, pp. 236ff.).

one typical property of money. It is satisfied by precious metals. There are a number of other properties that render certain economic goods more suitable to facilitating trade and serving as a commonly accepted medium of exchange, although this ultimately remains a matter of subjective evaluation on the part of the money users. These may include, for example, *durability*, *homogeneity across space and time*, *a relatively stable value over space and time*, *a high value to weight ratio* as well as *a low volume to value ratio*, and hence *easy transportability* or *transferability*.

Precious metals do arguably perform well on all of these dimensions. Another kind of money is *credit money*, which comes into existence when certificates of indebtedness, claims against physical or legal persons, denominated in units of some economic good, typically an underlying commodity, or so-called IOUs (“I owe you”) serve directly as medium of exchange. These IOUs can perform equally well on all the mentioned accounts, and probably even better when it comes to transportability and transferability, than most commodities. Yet, this form of money needs to be distinguished from mere money certificates that can be redeemed into the underlying money, for example, a commodity like gold, at any point in time at par. Money certificates as perfect substitutes for the underlying money can satisfy all the above criteria equally well as debt certificates, since they can be virtually identical with respect to their specific physical properties.

Credit money often emerges when redeemability of money substitutes is suspended, for example, during war times,⁹ but the money users do expect it to be restored at a later point in time. When they keep using the former money certificates like actual money under this expectation, we have an instance of credit money.

A third kind is *paper money* or *fiat money*, which can come into existence, and historically did so, when redeemability is suspended for good.¹⁰ In that case, the underlying commodity or base money is changed by government decree, typically from some precious metal into paper

⁹Hülsmann (2008, p. 29) mentions the American Continentals (1775-1781) used to finance the War of Independence and the *assignats* (1789-1796) of the French revolutionaries as historical examples of credit money.

¹⁰Again, the careful reader notices that whether or not some money is credit money or fiat money is a matter of historical judgment. As von Mises (1953, p. 61) wrote:

It can hardly be contested that fiat money in the strict sense of the word is theoretically conceivable. The theory of value proves the possibility of its existence. Whether fiat money has ever actually existed is, of course, another question, and one that cannot off-hand be answered affirmatively. It can hardly be doubted that most of those kinds of money that are not commodity money must be classified as credit money. But only detailed historical investigation could clear this matter up.

The money used today is however commonly referred to as fiat money, which is indicative of the extraordinary circumstances we can observe in the modern monetary system. There do not seem to exist strong expectations of a return to the gold standard. The money we use is valued independently as what it is: a pure medium of exchange by government decree and force, not backed by any commodity.

and check or digital units. Fiat money as we know it today is not a market phenomenon. Its implementation has historically always been a political one. It came into existence under the strong influence of governments on the monetary system (Selgin, 1994; Chown, 1994, part 3; Dowd, 2001; Rothbard, 2008b; Hülsmann, 2008, pp. 29ff.; Bragues, 2017).

Other important examples of digital money are modern cryptocurrencies, such as bitcoin or ethereum. Just like the underlying check and digital units of publicly provided fiat money, they can, in principle, perform best when it comes to the criteria of *high value to weight ratio*, *low volume to value ratio* and hence *easy transferability*.¹¹ However, the *durability* for both is very hard to estimate and rather uncertain, given that they do not have a material existence other than bits and bytes on computer hard drives and electronic signals while transferred.

The modern experience of digital money and cryptocurrencies brings us to another important dimension on which to distinguish different forms of money, namely, their production and legal status. Cryptocurrencies are privately produced media of exchange and are offered for sale on the market without enjoying any special legal status. The digital money comprised in the base money of the modern monetary system, in fact, the entire base money stock, M_0 , is created by political institutions called central banks. Modern commercial banks are allowed to create digital money themselves, so-called commercial bank money, while holding a specified fractional reserve ratio of base money in cash or at their central bank accounts. The money created by central and commercial banks enjoys a privilege as *legal tender*. This means that it has to be accepted, by force of law, for the settlement of various financial obligations.

There is no free competition in the production of media of exchange today. Quite to the contrary, there exist monopolists in the production of legal tender in various areas of the world, namely, central banks. The legal tender is in virtually all countries the most marketable and commonly accepted medium of exchange, and hence money. One cannot, however, take this to be an indicator that modern day central bank controlled fiat money as compared to privately produced alternatives is beneficial from the perspective of money users, precisely because of its legal privileges. It is only perceived to be beneficial, given these legal privileges, as demonstrated by the fact that it is indeed used by the vast majority of people every day.

Hence, there is of course an element of political force in determining what good becomes money, specifically due to *legal tender laws*. As a consequence, the legal tender, in order to

¹¹It should be emphasized again that we are not concerned with clarifying whether or not these alternatives to conventional moneys are “commonly” accepted. For an interesting discussion with respect to von Mises’s *regression theorem* see Davidson and Block (2015). For an exceptionally interesting discussion from a very different perspective see Bjerg (2016). For us it is only important that they are used as media of exchange by some people.

become or stay effectively the most commonly used medium of exchange, can perform less satisfactorily with regard to desirable properties of money from the perspective of money users than its available market alternatives. For example, being an inherently inflationary money, that is, having a growing money stock over time, might be considered desirable up to a certain growth rate,¹² and undesirable if the growth rate exceeds this point. A fiat money that enjoys legal tender status can more easily go beyond that point, since it does not have to compensate completely for this perceived disadvantage by other potentially desirable properties, simply by virtue of being to a certain extent forced upon the money users.

Another desirable property of a medium of exchange might be the degree of *privacy* it allows upon use. There has very recently been an argument for eliminating cash and digitizing the legal tender money completely (Rogoff, 2015). As a consequence, supervision of every single money transaction would become possible and hence perceived privacy might diminish. Some privately supplied cryptocurrencies try to prevent this problem by employing various methods of cryptography. The producers of the digital legal tender would not have to make the same efforts to satisfy user preferences in order to ensure that their product remains the most commonly used medium of exchange.

Sure enough, digitization and the easier supervision of transactions that it makes possible may also be considered an advantage in the form of increased *security*. But our point is the following. Whatever might be considered beneficial from the point of view of money users, a legally privileged medium of exchange does not have to cater to these subjective valuations *to the same extent* as its disadvantaged market alternatives, in order to remain the dominant medium of exchange. Now, the *extent* is of course determined by the specific laws in place. For example, the credible threat of a death penalty for both trading partners using alternative media of exchange in a transaction would render it rather large. The legal obligation to pay taxes in fiat money renders it relatively large, but probably not as much.

The above argument is very simple and its implications for the analysis of the costs and benefits of central bank controlled fiat money therefore rather limited. We can, however, reject any argument for the alleged superiority of the current legal tender based on the observation that it is indeed the most commonly used medium of exchange. What we cannot deduce is that central bank controlled fiat money is in fact worse than its alternatives. It might be better, but the mere fact that it is predominantly used does not suffice to justify this claim. Such a claim

¹²Bitcoins are also an inherently inflationary medium of exchange, since they are designed in a way that the total number of existing bitcoins is growing over time at a predictable and in fact ever decreasing rate (Hays, 2017).

would be defensible only if the dominant medium of exchange competed on equal footing over an extended period of time. This latter qualification is important, since it would be expected that there is also an element of sluggishness due to confirmed habits in the use of money.

The kind of money underlying the legal tender, that is, fiat money in modern monetary systems, as well as the legal framework that defines its competitive privilege over other potential media of exchange affect the boundaries in which the legal tender can be manipulated by political will, while remaining the commonly used medium of exchange. Indeed, where exactly these boundaries are is *a priori* uncertain, since they are also contingent on the subjective evaluation of the money users, but they make a discussion of monetary policy and its consequences all the more important.

7.2 Monetary Policy and Its Consequences

Given a specific legal framework, monetary policy refers to all measures affecting the supply or total stock of the legal tender in the economy. Expansionary policies refer to measures that increase the money stock. The kind of money that serves as legal tender determines the physical potential of monetary policy measures, that is, the degree to which the stock of money can be altered by political will. The legal framework as well as subjective evaluations of money users set the practical limits within which monetary policy can operate, while ensuring that the legal tender remains a functional and commonly accepted medium of exchange.

The underlying kind of money in our current system, as pointed out above, is immaterial fiat money. The most important specific property of fiat money from the perspective of monetary policy is that its marginal costs of production are virtually zero,¹³ which implies that any radical change in the stock of money is essentially as easily brought about as any small adjustment or no adjustment at all. The current legal framework likewise allows for radical changes in the supply of money when deemed necessary by the monetary policy authorities. Hence, the characteristic feature of such a fiat money system is that politically induced increases in the supply of money are higher than they would have been under the limitations of a commodity standard, simply because of the removed physical constraints and the reduced marginal costs of production.¹⁴

¹³As we will see in chapter 9, this does not imply that its total costs of production are necessarily negligible.

¹⁴Imagine the monetary policy authority has come to the conclusion that an increase of the base money stock by X units is optimal, when the marginal costs of production of these units are ignored for a moment. Under a commodity standard such an increase might not even be physically possible. In that case it is clear that the expansion under fiat money would be larger. But even if it was possible to increase the stock by that amount under a commodity standard, there would be a trade-off involved between generating a slightly lower increase in the base money stock

There are at least two very general consequences of monetary expansion. First, the objective exchange value of money as determined by the subjective evaluations of the marginal monetary units from the perspective of money users, that is, its purchasing power, will fall below the level it would have obtained under abstention from monetary expansion. As a corollary, any fixed sum of money saved as such, for example, in the form of cash under the pillow, will be worth less in real terms than it would have been otherwise.¹⁵

Second, not all monetary incomes and market prices will adjust simultaneously in exact proportion to the increase in the money stock. While some incomes and prices rise sooner and faster than others, the real disposable income of some people will be higher and that of others will be lower than it would have been otherwise, at least in the short run, if we consider a one-shot increase in the money stock. Hence, there will be a redistribution of wealth in real terms as compared to the counterfactual situation without money creation.

These propositions relate a factual or realized scenario to a counterfactual or unrealized one. They are useful examples for illustrating a central difference between hypotheses, formulated in the instrumentalist-positivist analysis of monetary policy studied in the first part of the thesis, and propositions formulated in the causal-realist approach covered here. The former can in some form or another be brought to the empirical test, while the latter ultimately elude it.

An unrealized scenario is not observable and propositions of the latter type are for that reason not empirically testable.¹⁶ Empirically testable hypotheses must relate not only to the factual scenario that actually materializes, but must also be restricted to empirically observable magnitudes describing that scenario, in so far at least as one takes the problems involved in the use of proxy variables seriously.

One can of course observe the purchasing power of money in a meaningful sense, namely,

and saving the marginal costs of production.

¹⁵Indeed, we have to add a caveat here. The argument only holds under the assumption that the increase in the money stock does not itself cause an increase in the subjective value of money on the part of money users and an increase in the demand for money that offsets the increase in supply. However, there is no good reason to believe it would.

¹⁶To be more precise, one can of course engage in estimations of counterfactual outcomes and use these as proxies describing the unrealized scenario. However, in such a case, taking a comparison of the proxies with the realized observable variables as an empirical test of the proposition falls short for exactly the same reason that the use of proxies in general is problematic in empirical research. One never knows whether one is testing the proxy or the proposition. Testing both at the same time is impossible and testing one is only possible when the other is assumed to be correct. In that sense an economist convinced of the truth of the above propositions might subject his proxies to the test (but why would she really come up with proxies when she believes in the propositions anyway?). A skeptic willing to empirically disprove the propositions must assume his proxies to be accurate representations of the counterfactual scenario, which is ultimately nothing but a presumptuous assertion. Attempts of coming up with accurate estimations of the counterfactual scenario in these situations are subject to the entire critique of positivist economics outlined in section 6.2.2. So, one should be skeptical about the proxies too.

as the array of market prices for goods and services, but one cannot, strictly speaking, decide objectively whether it has increased or decreased when certain prices go up and others fall.¹⁷ We can, however, make a general proposition of the above form based on the law of diminishing marginal utility, which must itself be understood as a counterfactual law.¹⁸

The more specific consequences of monetary expansion are contingent on the way the new money enters the economy. The redistributive effects might mostly be accidental, unsystematic and negligible, if the new money is simply handed out in equal proportions to all individuals according to their current monetary income, the practical difficulty in implementing such a policy notwithstanding. However, new money in the modern financial system enters the economy through credit markets, which has a systematic effect on the distribution of wealth. Before we enter into a discussion of these effects in section 7.2.2, we will explore another possible consequence of monetary expansion as credit expansion, namely, business cycles, in the following section.¹⁹

7.2.1 Austrian Business Cycle Theory

The primary cause of business cycles according to modern Austrian economics is the expansion of the credit supply under a certain condition that we will specify below. The most important way the credit supply can in fact be expanded is through fiat money creation by central banks. The first outlines of what later became the Austrian Theory of the Business Cycle was published in von Mises's habilitation thesis *Theorie des Geldes und der Umlaufsmittel* in 1912. Von Mises developed his theory essentially out of three components (Rothbard, 1988, pp. 21-22; Hülsmann, 2007, ch. 6).

First, he incorporated elements from the cycle theory of the *currency school*, which was

¹⁷Indeed, new products that have not been sold on the market before and for which no money price existed so far render things even more complicated.

¹⁸For a pedagogical outline of the law of diminishing marginal utility see Rothbard (2009a, pp. 21-33). For a discussion of the law in relation to money see Rothbard (2009a, pp. 311-315). The law is plainly wrong if understood as an empirically testable hypothesis, even when we abstract from the impossibility of measuring utility. This can be shown by simple logical reasoning. An additional unit of a homogeneous good might not in every case coincide with a diminishing utility of the marginal unit. If for example technological knowledge is revealed simultaneously that renders the good far more useful for the actor under consideration, or if some other potentially unobserved causal factor comes into play, it might coincide with increasing utility of the marginal unit. This however does not falsify the proposition as formulated above. The common way of navigating theoretical statements around this problem is the *ceteris paribus* qualifier. The latter is however unnecessary when we simply clarify the counterfactual nature of economic propositions. The causal relationship stressed is then completely independent of the particular circumstances that might alter the observable outcomes. For a detailed discussion see Hülsmann (2003).

¹⁹Elements of the following sections have been published in Israel (2017).

basically a theory of liquidity shortages on international financial markets under fractional reserve banking, but did not extend to the real economy. Second, he made use of an adapted version of the differentiation between the *natural rate of interest* and the *money rate of interest* as introduced by Wicksell ([1898] 1962). The natural rate of interest in von Mises's work is defined as the rate at which capital markets, in real terms, are in equilibrium, that is, a state in which demand for and supply of real savings are equal to each other. The latter is simply the gross rate of interest that actually occurs on the credit market. It is through this distinction that credit expansion gains its theoretical importance in explaining business cycles. The third component is the capital theory of von Böhm-Bawerk (1930, [1888]).

Eugen von Böhm-Bawerk emphasized the importance of the *structure of production* and the heterogeneous nature of capital. He separated production into different stages in which different kinds of capital goods are used. As more stages and capital goods enter the structure of production, output and physical productivity increase. In the course of economic progress, there is a transition to what von Böhm-Bawerk called *roundabout* methods of production.²⁰

Von Böhm-Bawerk noticed that the transition to more roundabout methods of production can only be successful when there are the necessary means of subsistence for the time it takes to build up the new structure of production, in other words, when there is a sufficient amount of real savings (see also von Strigl, 2000, pp. 6ff.). They are the basis for the completion of investment projects. Von Mises recognized that it is in fact the role of interest rates to coordinate investment projects according to the available subsistence fund in the economy.

If people save more, that is, if they forego consumption opportunities today, interest rates tend to decrease, as more funds become available for investments and are offered on the credit markets. If people save less, interest rates tend to increase. It is the rate of time preference which determines the willingness to save. Interest rates paid on the financial markets can then be understood as an aggregate of individual time preferences, or a reflection of *societal time*

²⁰See for example von Böhm-Bawerk (1930, pp. 18-19), where he describes the concept of *roundaboutness* for the first time:

We either put forth our labor just before the goal is reached, or we, intentionally, take a roundabout way. That is to say, we may put forth our labor in such a way that it at once completes the circle of conditions necessary for the emergence of the desired good, and thus the existence of the good immediately follows the expenditure of the labor; or we may associate our labor first with the more remote causes of the good, with the object of obtaining, not the desired good itself, but a proximate cause of the good; which cause, again, must be associated with other suitable materials and powers, till, finally, - perhaps through a considerable number of intermediate members, - the finished good, the instrument of human satisfaction, is obtained.

preference.²¹ Hence, the crucial point of von Mises' theory is that interest rates are not arbitrary numbers that should be interfered with politically. They should be allowed to reflect the natural rate of interest, which accommodates investment projects, and hence the roundaboutness of production, to the amount of real savings available in the economy.

When central banks intend to expand the money supply they usually lower the central rate of interest that commercial banks have to pay for base money reserves from the central bank. They essentially increase the supply of credit and push the money rate of interest below the natural rate of interest. This is the central condition that causes the business cycle. Under these conditions, investment projects that would have appeared unprofitable otherwise seem to be profitable and are undertaken. In particular, capital intensive long-term investments that include more stages and more roundabout methods of production are affected, since they are more sensitive to changes in market interest rates.²² This means that investments are undertaken that cannot be sustained given the amount of real savings available in the economy, because people do not necessarily consume less.²³ They might even consume more, especially when money is also borrowed for consumptive purposes (Salerno, 2012a). Although the economy lacks the necessary means of subsistence, a transition from the existing structure of production towards a more roundabout structure of production is set in motion.

Initially, this leads to an economic boom as more investment projects, particularly more roundabout ones, are started than would otherwise be the case. Consequently, the demand for labor tends to increase, which lowers unemployment. Hence, the relationship of the short-run Phillips curve can be explained using this theoretical framework (Bellante and Garrison, 1988;

²¹Market interest rates are prices that can be explained just like any other market prices using the marginal pairs analysis mentioned in footnote 4. Time preference is a concept describing the subjective evaluation of present and future goods. For a detailed discussion and overview of the *Pure Time-Preference Theory of Interest*, see Herbener (2011).

²²Imagine a market rate of interest of 5%. If we have to lend 1000 euros for an investment project that enables us to repay the loan after one year, we have to bear costs of 50 euros due to interest payments. For an investment project that enables us to repay the loan only after 10 years, interest payments would amount to 629 euros. Yet, if the interest rate was only 2.5%, interest payments for both investment projects would be 25 euros and 280 euros, respectively, which corresponds to relative cost reductions of 50% and 55%, respectively.

²³This is not to say that these types of errors would never be committed without credit expansion. They would almost always be committed to some extent, but credit expansion explains the accumulation or cluster of these entrepreneurial errors that characterize the cycle (Rothbard, 2009b; Hülsmann, 1998). Against the argument that entrepreneurs would be able to learn from experience and theory that these types of investment projects would be unsustainable, a game theoretic response can be given (Carilli and Dempster, 2001). For an alternative defense, see Murphy (2017). If experience and theory teaches anything at all, then that it can never be known *a priori* whether a risky investment project will be successful or not. Any particular project might be extremely successful, even under and maybe precisely because of credit expansion, but the argument here is that not all of the projects can. One might call this a *rationality trap*. It makes sense from the individual perspective to take out the loan and invest. After all one might gain a competitive advantage over other market participants by doing so. But from a global perspective this behavior is unsustainable.

Mulligan, 2011; Ravier, 2011, 2013). If the expansion of money and credit exceeds a certain level that offsets possible price diminishing factors such as productivity gains, it will lead to price inflation that coincides with the boom period and increased employment over the short run.

However, sooner or later the mismatch between real savings and investments will become apparent, when relative prices adjust accordingly. This is when the heterogeneous nature of capital goods and their application in different stages of production come into play.

Through the investment projects that are stimulated, an accelerated bidding process for labor and available non-specific capital goods that can be employed in many if not all stages of production begins. Yet, the means of subsistence and non-specific capital goods are scarcer than it is reflected by interest rates, since real savings have not actually increased. Higher demand will push prices further up and render the costs of investment projects higher than initially expected. It turns out that not all the investment projects can be finished given the amount of real savings in the economy.²⁴

Labor and non-specific capital goods have however been attracted to those projects in order to produce specific capital equipment needed in certain stages of production. Necessarily, some of the projects have to be liquidated. Businesses go bankrupt and employees lose their jobs. The capital has to be redirected into productive and sustainable methods of production if possible. However, to the extent that specific capital goods have been produced, that cannot simply be used in other stages of production and other investment projects, and are now useless, society has been impoverished. It takes time to actually rebuild a sustainable production structure, during which unemployment will tend to be higher than prior to the initial monetary expansion, due to frictions in the movement of labor. This phase constitutes the economic bust.

Paul Krugman (2013), by no means an adherent to Austrian economics, has drawn attention to the asymmetry problem of booms and busts: the phenomenon that increased unemployment occurs during the structural adjustments of the bust, but not during the structural adjustments of the boom, which he explains by reference to downward wage rigidity. During a boom period

²⁴A vivid illustration is given by von Hayek (2008, p. 272) who compares this situation to a hypothetical scenario of a people on an isolated island:

The situation would be similar to that of a people of an isolated island, if, after having partially constructed an enormous machine which was to provide them with all necessities, they found out that they had exhausted all their savings and available free capital before the new machine could turn out its product. They would then have no choice but to abandon temporarily the work on the new process and to devote all their labor to producing their daily food without any capital. Only after they had put themselves in a position in which new supplies of food were available could they proceed to attempt to get the new machinery into operation.

wages tend to rise, but during the bust they do not fall as much and as rapidly as they should in order to prevent increased unemployment.

An alternative explanation is provided by Andolfatto (2013), who argues that the most obvious cause for asymmetry is not to be found in nominal rigidities, but rather in the mass destruction of productive relationships, which takes place during the bust. In his view, the labor market is a market for productive relationships, or what he calls *relationship capital*. Just like physical capital, relationship capital is redirected onto unsustainable paths during the boom. Relationships are built up, intensified, replaced or adjusted during the boom, merely to get destroyed during the bust. In his own words:

The basic idea is very simple. [...] [T]he labor market is a market for productive relationships. It takes time to build up relationship capital. It takes no time at all to destroy relationship capital. (It takes time to build a nice sandcastle, but an instant for some jerk to kick it down.) (Andolfatto, 2013)

During the bust there is essentially a matching problem. It takes some time until new productive relationships are discovered. If the bust comes along with credit defaults and disturbances on the financial markets, bank credit deflation may be a consequence, as banks refuse to extend loans further, due to increased economic risk and uncertainty (Salerno, 2003, pp. 86-87; Bagus, 2011, pp. 3-4; Bagus, 2015, pp. 67ff.). Moreover, people tend to demand higher cash balances in response to increased economic risk and uncertainty, which might lead to cash-building deflation (Salerno, 2003, pp. 85-86.; Bagus, 2011, pp. 2-3; Bagus, 2015, pp. 42ff.). Hence, towards the end of the cycle increasing unemployment and price deflation, or at least very low price inflation, may coincide as indicated by the short-run Phillips curve relationship. However, the above explanation suggests that the cause of increasing unemployment is not to be found in the deflationary tendencies of the bust, but rather in the inflationary tendencies of the boom period which occurred before.

The increased unemployment of the bust phase is also a temporary phenomenon. Eventually, after the boom period with lower unemployment and the bust period with higher unemployment, the rate returns to its natural level as determined by the institutional environment (Bellante and Garrison, 1988). The latter may indeed have changed, but to establish causal link between credit expansion and the natural rate of unemployment in the long run requires much more than the above analysis.

Hence, starting from an economy in which the actual and natural rates of unemployment coincide, we can argue that the expansion of money and credit, over the course of the business cycle, pushes the actual rate of unemployment below the natural rate in the short run, but eventually causes unemployment to increase above the natural rate in the medium run. After the business cycle, that is, in the long run, the actual rate returns to the natural rate of unemployment. The analysis is therefore compatible with the Friedman and Phelps natural rate hypothesis of a negatively sloped short-run Phillips curve and a vertical long-run Phillips curve, but adds a third element: a positively sloped medium run Phillips curve.

Interestingly, Ravier (2013) argues that there might be a permanent positive impact on unemployment. However, he does not make sufficiently clear that his argument is contingent on politics. He starts from a situation with minimum wage legislation, arguing that, due to capital consumption and destruction during the business cycle, labor productivity may have fallen so much that the existing minimum wages lead to increased unemployment. Yet again, in the long run, through a genuine process of capital accumulation based on real savings, labor productivity may reach and even exceed its prior level.

Moreover, nominal wages might have risen so much in the inflationary process that paying minimum wages, which are fixed in nominal terms, does not actually pose any problems for employers. Abstracting from minimum wages and unemployment benefits, it is even conceivable that employment increases after the business cycle, namely when capital destruction has impoverished society to such an extent that it precipitates lower wage elasticities of the supply of labor. To be fair, strictly speaking, in this scenario we would not be dealing with a reduction in unemployment of the kind that we are really concerned with, that is, *forced* or *institutional unemployment* (von Mises, 1998, pp. 598ff.; Hutt, 2011, p. 73). What we would have here is a case in which *voluntary* or *preferred unemployment* (Hutt, 2011, pp. 38ff.) in a society that enjoys relatively high living standards has been transformed into *forced unemployment* in the impoverished society after the business cycle. On a free market for labor this would lead to increased employment (von Mises, 2000, p. 57).

These considerations show that it is a rather difficult theoretical endeavor to establish a necessary, time and place invariant long-run relationship between price inflation and unemployment that is independent of further political interventions. Ultimately, the height of unemployment is determined by restrictions and rigidities that are politically forced upon labor markets (Sennholz, 1987). As von Mises (1990a, p. 125) points out: “At the equilibrium wage rate unemployment is

only a transitory phenomenon.” Long-term mass unemployment occurs when wage rates are not free to equalize supply and demand for labor, either directly through minimum wage legislation, or indirectly through pressure from labor and trade unions (Hutt, 1954). Rueff (1925a, 1931) emphasized the role of unemployment benefits, which can themselves be interpreted as quasi minimum wages, below which incentives to work are drastically diminished. Unemployment from this perspective remains primarily a matter of fiscal policy.

If the above account of the monetary theory of the business cycle according to modern Austrian economics is accurate, we can establish a short-term benefit of credit expansion in the form of increased economic activity and lower unemployment that comes at the costs of a subsequent economic downturn with higher unemployment.²⁵ It is unquestionable that modern central banking under fiat money renders credit expansion larger than it would be under the constraints of a commodity money or any competitive money that does not enjoy the privilege of legal tender laws. However, political will is of course not the only factor influencing the supply of credit in a market economy. Every voluntary increase or decrease of the supply or demand of any economic good causes a readjustment of the processes of production, but there is a distinction to be made on two accounts.

First of all, suppose that the supply of credit is increased by people lowering their consumption out of current income. The saved funds are directly offered on the credit markets. This is an example of a genuine shift towards lower time preference. The consumption to savings ratio has decreased and the structure of production will be adjusted accordingly to more roundabout and capital-intensive processes of production. The physical productivity of the economy will grow as a result, and hence its potential to satisfy the preferences of a great number of people.²⁶

Next, suppose that the supply of credit is increased out of a previously hoarded sum that had been detracted from circulation for a while. The hoarding decision has effectively lowered the supply of money and credit in the economy. Let us assume that all real economic consequences of the hoarding decision have played out. In what way would the subsequent decision to offer the hoarded sum of money on the credit market be different from a politically induced credit expansion? Neither would constitute a genuine shift in time preference but still, in both cases,

²⁵Strictly speaking, we would have to specify from what point of view exactly the postulated “benefit” is a benefit after all.

²⁶There might be people who subjectively prefer a less productive economy to a more productive one in a certain sense. It must be noted, however, that higher productivity is not to be confused with a greater quantity of physical goods produced. There might be fewer goods, different goods and goods of a higher quality. An increase of leisure might be one result of higher physical productivity.

an increase in the supply of loanable funds occurs.²⁷ Other than that the former would most likely be smaller in degree and not perpetual, there is from a purely economic perspective no difference.²⁸ The same economic effects follow from a voluntary credit expansion of this type as from a central bank induced credit expansion, if only on a smaller scale.

Now interestingly, every voluntary act of credit expansion seems to be a mixture of the above cases. This means that the empirically observed business cycle as caused by credit expansion is not fundamentally different from the steadily ongoing adjustment processes of the market economy in the absence of politically induced credit expansion.²⁹ This suggests a policy conclusion akin to the one drawn in some New Classical contributions, namely, that the bust in a business cycle, once it occurs, should be seen as a necessary and important adjustment process that might be painful in the short run, but should not be countered with expansionary monetary policy, that is, a lowering of the central rate of interest and an increase of the credit supply. Such a measure will instead of solving the problem only postpone the necessary adjustment (von Mises, 1998, pp. 792-794; von Hayek, 2008, pp. 253-276; Rothbard, 2009a, pp. 994-1004). Moreover, central banks should in general abstain from inducing unsustainable booms by pushing the market rate of interest below the natural rate of interest.

Supposing that the natural rate of interest remains roughly constant, the business cycle is caused by an accelerated expansion of the money and credit supply. Once the economy has adjusted to an increased rate of money growth, the cycle is over, too. This might at first glance suggest a policy conclusion close to Friedman's *k*-percent rule, but it really is very different. The intermediate conclusion that one can reach at this point is that interest rates should not be regarded as tools for active policy at all. The obvious problem is that they will inevitably be affected by arbitrary choices if the quantity of money is subject to political will.

The next consequence of money creation via credit is relevant in all conceivable cases, whether the growth rate remains constant, increases, or diminishes, whatever the development of other magnitudes in the economy.

²⁷It is true that the hoarding decision is a reflection of time preference, but once the sum of money is hoarded and out of circulation, say for a hundred years to make the point clear, it has no repercussions on how time preference determines demand and supply on the credit market.

²⁸From the perspective of political philosophy there is a considerable difference between the two cases - one being political, the other within the confines of voluntary exchange.

²⁹One could construct a similar example under a gold standard, where money is produced competitively. Imagine that a banking company finds easily accessible gold reserves, say in South American colonies, and imports them at very low costs to Europe and offers the acquired funds directly on the credit market. In that case too, one would have the symptoms of the business cycle, possibly on a lower scale.

7.2.2 Redistributive Effects of Credit Expansion

In general, any kind of money creation under any conceivable monetary system leads to redistributive effects. Hence, any persistent depreciation of the exchange value of money vis-à-vis other goods and services, induced by monetary expansion, has unintended side-effects from the perspective of stated public policy goals. The redistributive effects may be more or less important and more or less randomly distributed. However, credit expansion as a specific type of money creation has specific and systematic consequences. Let us go through the argument step by step.

First of all, we have to abandon the mechanistic view that all prices and wages within the economy grow synchronously under inflation, which is also central to the business cycle theory explored above (von Hayek, 2008, pp. 197ff.). There will always be some wages and prices that increase faster and earlier than others. When money is created, it does not increase all cash balances and incomes in exact proportion to the cash balances and incomes as they existed before. Therefore, it will benefit some - those who receive disproportionately more - at the expense of others - those who receive disproportionately less. In particular, those who receive the newly created money first benefit, as they are able to acquire more goods for still relatively low prices. As the additional money is spent, it gradually bids up prices. Those who have not yet received any of the newly created money or only receive their shares later are worse off, as they are confronted with rising prices but still constant or relatively low incomes. These distributive effects of monetary inflation have become known as Cantillon effects.³⁰

Under modern central banking, commercial banks and other financial institutions are usually the first receivers of the newly created money. It is not astonishing that historically under fiat money systems, price inflation rates tend to be higher, and financial markets tend to grow much faster than under commodity money standards with full reserve requirements (Levine, 2005). The first beneficiary of monetary expansion in the current system, therefore, is the financial industry itself. There are three main reasons why the growth of financial markets is triggered by monetary expansion:

- (1) because financial titles are particularly useful collateral in debt contracts;
- (2)

³⁰They are named after the Irish-French economist Richard Cantillon, who analyzed these effects in his *Essai sur la nature du commerce en général* (Cantillon, 1755). It should also be mentioned here that these redistributive effects do not depend on the actual emergence of price inflation. As pointed out earlier, as long as the money supply is expanded, prices will be higher than they would otherwise have been, and a redistribution of wealth from the late receivers to the early receivers of the new money takes place.

because foreseeable price-inflation, a common consequence of fiat money systems, discourages money hoarding and encourages both the demand for, and the supply of, financial titles; (3) because the production of money through central banks is a matter of sheer human will and therefore creates moral-hazard problems leading to both an artificially high demand for financial titles, and an artificially big supply thereof. (Hülsmann, 2014, p. 130)

The growth of financial markets and the increase of the relative value of financial assets leads to a higher wealth to income ratio (Piketty, 2014, pp. 164ff.). This is not problematic for those who are already wealthy and possess assets, but for those who do not, it diminishes the chances of catching up. Consequently, monetary expansion decreases upward social mobility, and “thus contribute[s] to turning a free society into a caste society” (Hülsmann, 2014, p. 130).

An important leverage effect lies in the selective and discriminatory nature of granting commercial bank credit (Doumposa et al., 2002). Commercial banks create money on top of the base money supply through the extension of loans to businesses and private individuals. Relatively wealthy people, who have stable streams of income, can service higher debts and they usually have to pay lower interest rates as they are more credit-worthy and exhibit lower default risks. Hence, they are able to acquire a larger share of the newly created money. The economics of Cantillon effects tells us that they benefit disproportionately, since they can purchase more goods, services, and real assets for still relatively low prices. We might interpret this effect as a redistribution from bottom to top.³¹

We have thus an unintended consequence of credit expansion, namely, a rising gap between rich and poor. This is an aspect that virtually no one would support. It must be seen as a specific cost of monetary policy under central bank controlled fiat money, at least to the extent that this institutional setup leads to a larger expansion of the money supply than its alternatives and that the expansion occurs in the form of credit.

That there is a rising gap between rich and poor is widely acknowledged. However, very little attention is given to the role of expansionary monetary policy in that development. Interestingly, Piketty (2014, p. 214) writes:

³¹It should be mentioned that recent empirical studies support the connection between credit expansion (or leverage) and inequality (Malinen, 2014; Kumhof et al., 2015). In contrast, these studies commonly assume that inequality is the causal factor that leads to higher leverage and ultimately to economic crises (Rajan, 2010). Credit expansion must however be recognized as a tool of monetary policy, and hence excessive leverage as a political phenomenon. It is true that rising inequality tends to call forth political responses. Yet, these responses, as we will argue in chapter 8, are mostly fiscal and not monetary.

But when inflation remains high for a considerable period of time, investors will try to protect themselves by investing in real assets. There is every reason to believe that the largest fortunes are often those that are best indexed and most diversified over the long run, while smaller fortunes - typically checking or savings accounts - are the most seriously affected by inflation.

In this assessment Piketty is right, but he does not put much effort into elaborating on this point, although he recognizes that “[s]ince the 1970s, income inequality has increased significantly in rich countries, especially the United States, [...]” (p. 22) That the 1970s involve a fundamental change in the financial order after the Nixon Shock, which was followed by very high price inflation rates, however, is not discussed in this context.³²

As pointed out above, the beneficiaries of monetary expansion are determined by the mechanism through which the new money enters the economy. It would be entirely conceivable to change the mechanism from credit expansion to any conceivable alternative. For example, one could give equal shares of the created sum of money to each registered citizen, or use the created sums of money to finance welfare state or refugee programs, to mention a very timely topic. One could thereby directly benefit people who are in need. This could diminish the gap between rich and poor. However, the stated purpose of a loose monetary policy is to stimulate investments and business activity, and not merely consumption. Another problem would be that not only the quantity of new money, but also the recipient would be determined directly by the political authorities. This would, on the one hand, trigger a much more aggressive democratic distribution conflict as the recipient group would have to be directly chosen and specified. The favor to one group and the neglected favor to another would be much more openly visible.³³ On the other hand, it would leave political authorities in a much more powerful position that might morally corrupt not only them but also the chosen recipients.

7.3 Financial Market Instability, Systemic Risk and Moral Hazard

The concepts of *systemic risk* and *moral hazard* are very important and closely connected when it comes to the production and use of money. They also tie into the consequences of money creation

³²We will examine some of the relevant data in the next chapter.

³³This might indeed be seen as advantageous with respect to obtaining a more transparent system.

as discussed above, especially when we abstain from the idealizing assumption of the benevolent social planner in charge of money production and monetary policy, but take an arguably more realistic public-choice perspective.³⁴ After all, one realizes intuitively that the privileges of a monopolist in legal tender production under a fiat standard engender incentives that can lead to reckless behavior. Selected parties are easily made beneficiaries of monetary policy interventions at the expense of others. In such cases, there is effectively a redistribution of wealth. The latter is based on the creation and distribution of new money and can under certain circumstances cause inflationary booms and business cycles. This is just one important empirical manifestation of higher systemic risk.

Moreover, von Mises's rejection of central banking in his more mature thought on monetary economics as contained in *Human Action* can be interpreted as an argument from moral hazard. He thought that a free banking system would restrict monetary expansion much more effectively than any political institution (Salerno, 2012b). A legal monopolist who effectively restricts the money supply is of course not entirely inconceivable, but very unlikely given the incentives it faces.

For a systematic exposition we first have to clarify the meaning of moral hazard and systemic risk, as well as their relationship, which is the subject of sections 7.3.1 and 7.3.2. We will then elaborate on the claim that the very existence of central banks in the modern monetary system is a contributing factor to both moral hazard and systemic risk, leading to an increased instability of financial markets and the economy as a whole.

7.3.1 On the Meaning of Systemic Risk

According to Dwyer (2009) the first use of the term “systemic risk” in the title of a publication in economics or finance registered on *EconLit* was made in 1994. It is thus a concept that rather recently entered the academic discussion. It originated as a term in policy debates used by practitioners and is notoriously lacking a precise definition that a systematic analysis could be based on.³⁵ There are, however, different attempts to render the meaning of systemic risk down to something more precise than our intuitive understanding (Bisias et al., 2012).

³⁴For an excellent introduction to public-choice theory see Tullock et al. (2002). Arthur Seldon suggests in the introduction that government in general has transformed into government “of the Busy (political activists), by the Bossy (government managers), for the Bully (lobbying activists)” (p. x).

³⁵At a Mercatus Center workshop on Financial Market Regulation in Chicago in January 2017, an employee of the Chicago Fed, when asked about the definition of systemic risk, assured us that “if there is anything we can agree on, it is that there is no generally accepted definition of the term.”

In general, one can say that “[s]ystemic risk refers to the risk or probability of breakdowns in an entire system, as opposed to breakdowns in individual parts or components, and is evidenced by comovements (correlation) among most or all the parts” (Kaufman and Scott, 2003, p. 371). As such, while the meaning of breakdown undoubtedly requires some clarification, it can occur in any kind of economic system. In the financial system specifically, it refers to “high correlation and clustering of bank failures” (Kaufman and Scott, 2003, p. 371).

In order to develop a more precise notion, a brief clarification of the term system seems helpful. Zigrand (2014) defines a system as a working mechanism, not characterized by a specific observable outcome, but by serving a specific purpose. In that spirit, the economic system can be seen as the array of voluntary exchanges under a specified set of rules within a specified geographical area, serving the purpose of mutual benefit and want satisfaction. The financial system as a part of the latter can be seen as the array of contractual relationships for the purposes of saving, investing, risk sharing, and external funding of economic activities, such as production and consumption. These include immediate exchanges between lenders and borrowers as well as exchanges mediated by financial institutions.

Both of these systems are social systems and as such complex (von Hayek, 1967), in the sense that they contain many components - many individuals, families, businesses, banks or government institutions - and that their specific properties are emergent and thus constantly changing over time. They have essentially the character of exchange networks connecting individual components. The term systemic risk then refers to the probability of the breakdown of the entire network or substantial parts of it,³⁶ that is, an event that prevents the system at least partly from properly serving its purpose. This is the case during economic and financial crises. The risk of the latter can be assessed approximately after the fact, for specified geographical areas and historical periods of some length. It cannot be quantified *a priori*.³⁷ There are, however, structural features that render an economic or financial system prone to higher systemic risk.

³⁶In the abstract, it is possible to think about a world in which all individuals live in self-sufficiency, which would be the case after a complete breakdown of the economic system. In practice, however, this seems rather unlikely. The breakdown in form of an economic crisis will almost always be partial. In the case of financial systems, however, there exist historical experiences that justify the label of complete breakdown. The hyperinflation periods during the French Revolution (White, 1896; Aftalion, 1987; Spang, 2015) or the Weimar Republic (Fergusson, 1975) are important examples.

³⁷Given the distinction between risk and uncertainty proposed by Knight (1964, [1921]), one might argue that a more appropriate label would be *systemic uncertainty*. Knight’s notion of risk refers to a situation in which all possible outcomes and their probabilities are known beforehand. Following von Mises (1998, pp. 107ff.) distinction between *class* and *case probability*, the probability of an economic crisis would be a matter of the latter. It is as such not open to any kind of precise numerical evaluation. Yet, by ignoring the particularities of historical crises we could form a class and look at the frequency of crises occurring in order to approximately evaluate their probability.

In the previous section, we have already analyzed the role of an extended politically induced push of the market rate of interest below the natural rate as the primary cause that gives rise to the business cycle. The latter can be interpreted as an accumulation of unsustainable exchange relationships that break down in the crisis. However, there is more than just the political power to induce an extended credit expansion that determines systemic risk. The particularities and especially the magnitude of a crisis depend on other structural features of the system, most notably the interdependency of its individual components and their exchange relationships. One might here also speak about the danger of *contagion*. How does the breakdown of certain exchange relationships and components of the system, for example, the failure of a commercial bank like Lehman Brothers in 2008, affect other exchange relationships and components of the system? How is a crisis or shock propagated through the system?

The failure of one component in the system - an investment bank, business firm, or any other institution - may cause another component to default on its obligations, if its own assets are sufficiently diminished by the initial failure, which in turn may cause still others to default. Such a chain reaction is more likely to occur the higher the leverage ratio of the individual components in the chain, that is, the higher the proportion of borrowed funds in asset purchases and investments, and hence the lower their ability to absorb losses.³⁸ While leverage increases potential profits, it also increases potential losses, and thereby risk and the danger of spillover effects and contagion. Thus, one can put it vividly like Kaufman (1995, p. 47): “systemic risk is the risk of a chain reaction of falling interconnected dominos.”

The perceived advantage of a central bank operating under a fiat money standard is that it

³⁸A famous example is the hedge fund Long Term Capital Management (LTCM) founded in 1994, incidentally around the same time that the term systemic risk was introduced into academic debates. The fund obtained returns of more than 20% during the first year and more than 40% the second and third year. In 1998, LTCM nearly defaulted and caused massive turbulence on the international financial market. It was re-capitalized with more than \$3 billion from 16 of the most important international banks and investment houses under supervision of the Federal Reserve System in order to allow an orderly liquidation in 2000. At the time of its near-default the fund operated on a 25-to-1 debt-to-equity ratio (Lowenstein, 2001; Belke and Polleit, 2009, pp. 207-208). Greenspan (2007, p. 194) even stated that its leverage ratio was according to the best estimates “well over \$35 for every \$1 it actually owned.” Greenspan comments further:

Ordinarily a business that makes a fatal blunder ought to be left to fail. But the markets were already spooked and skittish; Bill McDonough worried that if a company of LTCM’s size had to dump its assets on the market, prices could collapse. That would set off a chain reaction that would bankrupt other firms. So when he called to say he’d decided to intervene, I wasn’t happy with the idea, but I couldn’t disagree.

The interesting aspect about this incidence is that the recapitalization of LTCM was completely financed by the 16 private institutions, including Deutsche Bank, Goldman Sachs, JP Morgan and Lehman Brothers. As Greenspan writes “[n]o taxpayer money was spent (except perhaps for some sandwiches and coffee)”. One could say that the system in that incidence handled the risk, but it illustrates the potential spillover effects very well. The leading banks were willing to recapitalize LTCM and allow it to dissolve orderly, because they expected to lose more otherwise.

can, in principle, prevent the chain of dominos from falling by creating funds *ex nihilo* and recapitalizing the banks and businesses in need. This goes back to the traditional argument of the lender of last resort already discussed by Smith (1990, [1936]) and mentioned in the introduction to this thesis. However, it is clear that the leverage under which a business operates is subject to an entrepreneurial decision, taken under the influence of the institutional framework. If the expected costs of business failure and insolvency are systematically diminished by potential bailouts, the willingness to engage in riskier ventures and to operate on higher leverage increases. This is where moral hazard comes into play.

7.3.2 The Problem of Moral Hazard

Moral hazard is a much discussed concept in economics and other disciplines (Rowell and Connelly, 2012; McCaffrey, 2017). It is commonly regarded as an incentive problem that may lead to recklessness, irresponsibility, or as the term would suggest immoral behavior. It describes a situation in which not all the relevant detrimental consequences or costs of a possible and known action are borne by the beneficiaries, most notably the actors themselves, but can be imputed on others against their will.

Let us try to make this rather loose definition somewhat more precise. Moral hazard occurs in situations where some person or group is aware of a possibility to use or alter in some form, for their own benefit, directly or indirectly, resources that are at least partly owned by some other person or group against their will. Moreover, the other person or group has no way of immediately preventing this use or alteration.³⁹

Moral hazard is often regarded as a symptom of market imperfection. According to standard theory, a primary cause is asymmetry of information (Laffont and Martimort, 2002, p. 3). While it is certainly correct to regard asymmetry of information, in particular, the relative lack of information or false information, as the cause of error and hence “disequilibria” in human affairs, it is a universal condition and as such unavoidable. If one were to proclaim that asymmetry of information is a sufficient condition for moral hazard, the latter would itself be universal. It would then not be a problem to solve, but a general condition of human life, and the notion of

³⁹Hülsmann (2006, p. 35) defines the concept as follows: “A genuine moral-hazard problem appears [...] if A has the possibility to use B’s resources against B’s will and if he knows this.” Hence, “the essential feature of moral hazard is that it incites some people A to expropriate other people B.” The author clarifies in a footnote that “expropriation of B” is used as a short-cut for “using B’s property against his will with impunity” used as a technical term following Rothbard (1998) and Hoppe (2006). We see that a precise definition of moral hazard in that sense must be based on a theory of property rights. Moral hazard is then always characterized by incentives to infringe on the property rights of others. These infringements need not be criminal but can be legal.

market imperfection due to moral hazard would be completely meaningless. Hence, a second condition has been stipulated that causes moral hazard jointly with asymmetry of information. This is the separation of ownership and control, which can occur either under co-ownership of resources or in principal-agent contracts (Mirrlees, 1976; Holmstrom, 1979; Arrow, 1985; Grossman and Hart, 1992).

Hülsmann (2006) argues that the separation of ownership and control is in fact the decisive condition. Indeed, the well-known problem of the *tragedy of the commons*⁴⁰ which comprises an example of moral hazard, is completely independent of information asymmetries. The problem that an unregulated common resource may be over- or misused by some co-owners at the expense of others emerges even under the condition that all agents involved possess exactly the same set of information. Hence, asymmetry of information as such, while always existent in some form, is not even a necessary condition for moral hazard.

It might still play a very important role, especially in cases where the harmed person or group is unaware or uninformed of the moral hazard. This might arguably be the case for most people when it comes to central bank monetary policy. In contrast, the existence of moral hazard in voluntary principal-agent contracts is usually well-known and anticipated. If it turns out to be an unbearable cost they can be avoided by refraining from entering into such contracts with the respective person or group. Such an option is not given when it comes to moral hazard from politics.

7.3.3 Central Bank Monetary Policy as a Cause of Moral Hazard and Systemic Risk

As Hülsmann (2006) argues, the most important cases of moral hazard emerge as a consequence of government interventionism. There are various ways in which governments can intervene into the economic affairs of their subjects. An intervention is generally defined as a policy measure that interferes with the private property order without completely abolishing it, such as, for example, prohibition of certain substances. As a result, people make different choices for the use of their property than they would without the intervention. The government does not directly declare itself the owner of certain resources, but rather constrains their range of feasible uses or, as the case may be, widens it. For some people certain transactions may become possible that

⁴⁰The concept is often traced back to Lloyd (1833) who provided an example of privately owned cows grazing on the “commons.” The term then was popularized by Hardin (1968). However, it refers to a problem that had been discussed for much longer (Ostrom, 1990, pp. 2-3).

would have been impossible under the absence of the intervention.⁴¹ The government thereby becomes a *de facto* co-owner of the resources in question even though the affected people remain exclusive property owners *de jure*.⁴²

There is thus a separation of ownership and control. The *de jure* property owners control the resources in question but the government is a *de facto* co-owner that can influence and alter its use. One can argue that this is the case even in absence of any specific interventions. The mere possibility of government intervention, simply by virtue of the existence of an institution that enjoys the legal powers of a modern nation state, constitutes a separation of ownership and control and hence a moral hazard. The government knows that it has the possibility to use and alter the resources of its subjects against their will within the boundaries of the prevalent constitutional framework, and its subjects do not have the means to prevent it immediately.⁴³

In the financial system, legal tender legislation represents the most basic intervention (von Mises, 1998, pp. 777ff.). It dictates that a certain medium of exchange has to be used in the settlement of certain financial obligations. In the terminology of Rothbard (2009a, pp. 877f.) it qualifies as a *triangular intervention*, since the government is not only intervening into the direct relationship between itself and its subjects, but is also altering the relationship between any two or more potential or actual exchange partners.

More precisely, legal tender laws are a form of *price controls*. When they are effective they lead to an overvaluation of the legal tender and thus change the use of private property in

⁴¹The economic system of interventionism describes a whole continuum of possible arrangements between the two poles of capitalism, characterized by complete private ownership in the means of production, and socialism, characterized by common ownership in the means of production. It is, however, conceptually distinct from a mixed economy, in which the government owns some of the means of production and participates in market exchange with private property owners. In practice all economies are more or less interventionist mixed economies, that is, all governments own some and intervene in the use of other means of production. In his *Critique of Interventionism*, von Mises (1977, p. 1) describes the latter as follows:

Interventionism seeks to retain private property in the means of production, but authoritative commands, especially prohibitions, are to restrict the actions of private owners. If this restriction reaches the point that all important decisions are made along lines of authoritative command, if it is no longer the profit motive of landowners, capitalists, and entrepreneurs, but reasons of state, that decide what is to be produced and how it is produced, then we have socialism even if we retain the private property label. [...]

However, interventionism does not want to go that far. It does not seek to abolish private property in production; it merely wants to limit it.

Neither government measures necessary to sustain the private property order nor government activities as seller or buyer of goods and services within the confines of that order are seen as interventions by von Mises.

⁴²Hoppe (2011) used the interesting term *fiat property* to describe this situation somewhat more drastically. Governments can intervene and even directly expropriate people at any time. Their property is thus only property by government *fiat* or discretion.

⁴³Eminent domain or the possibility of compulsory purchases, that is, the government's legal authority to seize private property for public use is a standard example that generates a moral hazard in the above sense. This could of course be changed in the long run if it became a consensus view that eminent domain is undesirable.

exchange relationships. Following Gresham's law,⁴⁴ the use of the legal tender is encouraged and the use of market alternatives is discouraged.

Now, strictly speaking, legal tender laws today are not merely price controls in the narrow sense of the word, but historically they had precisely this character. They established a fixed relationship between a monetary substitute and some underlying money commodity and granted the same legal status to both. Inflation of the supply of money substitutes, that is, the production of unbacked substitutes or *fiduciary media* led to a systematic overvaluation of the substitutes, which drove out the commodity itself from circulation, because the latter in turn was systematically undervalued (Rothbard, 2008b, pp. 62-64). It was worth more than the fixed exchange rate to the substitutes would suggest. Today the most important aspect of legal tender legislation is that it makes a certain medium of exchange the compulsory means of payment for debts, taxes, public charges, and dues of all sorts. In that sense the legal tender is overvalued and tends to drive out market alternatives from circulation.

A central bank as the monopolist producer of the base supply of legal tender (M_0) enjoys thus a privilege over competitors in the production of media of exchange. It is by this simple fact that we can argue that a central bank is a public institution and not a private organization as is sometimes argued in the case of the Federal Reserve System. It is also the reason why central banks can never be seen as truly independent of the government.⁴⁵ The legal privilege that a central bank enjoys is fundamental to its role in the economic system. As long as it persists, the term central bank independence cannot, in the final analysis, be considered anything but an oxymoron. It is highly unlikely that an institution enjoying legal privileges from the government refuses to ultimately stand in its service (Hoppe, 2006, ch. 3).

As we have explained above, legal tender laws determine jointly with the underlying monetary standard the range of possible monetary policy measures that can be implemented without the chosen legal tender losing its role as the most commonly used medium of exchange. Within that range central banks can impose costs of their action onto others, namely, the users of the legal tender. Central banks can increase the base money reserves of selected market participants and hence strengthen their relative economic position, but the community of legal tender users has to deal with a lower purchasing power than would have been the case otherwise. As a result,

⁴⁴It states that when a fixed exchange ratio between two moneys is established by law, the overvalued money will drive out the undervalued money, which is in turn held back from exchange. It was named after Sir Thomas Gresham (1519-1579) by Macleod (1858, p. 476-478), but discussions along these lines can be found in earlier works, for example, by Nicole Oresme (Selgin, 2010). In fact, Rothbard (2006a, p. 282) points out that Sir Thomas Gresham was not even the first in England to formulate the law. Rather, it was Sir Thomas Smith (1513-1577).

⁴⁵For our earlier discussion of central bank independence, see section 4.3.4 starting on page 164.

property will be redistributed and used differently. We thus have a genuine moral hazard on the part of central bank authorities.

The moral hazard extends, however, also to the users of the legal tender. First, we have to consider commercial banks in their role as intermediaries. They are directly linked to the central bank as they can create commercial bank money, that is, demand deposits in excess of the base money received from central banks. They demand and hold the latter as fractional reserves backing their client's demand deposits.⁴⁶ Demand deposits and physical money in the form of coins and bills in circulation together form the monetary aggregate M_1 . In a fractional reserve system the latter is in general bigger than the base money stock M_0 . Central banks do not have full control over the money stock M_1 . They have control over the base money. For the determination of the broader monetary aggregate, not only the base money stock but also credit transactions of commercial banks with their clients are important.

The fact that commercial banks can create demand deposits over and beyond the base money stock provided by central banks engenders a moral hazard on their part. They can expand their business and increase their revenues by only holding fractional reserves and thereby make it impossible for their clients to simultaneously withdraw their funds in cash if they so desire.⁴⁷ Commercial banks not only make themselves co-owners of their client's deposits, but essentially make all their clients co-owners of the amount of reserves they hold. Moreover, they grant other clients control over these reserves, namely, those to whom they extend credit. There is then also a moral hazard on the part of their clients, most visible in the case of bank runs. Clients withdraw reserves knowing that others will not be able to withdraw their deposits as well.

Fractional reserve banking thus creates a risk of liquidity shortages and as such contributes

⁴⁶The current legal reserve requirements in the eurozone lie at 1%, meaning that for every euro held as reserves (either in cash or on reserve deposits at the central bank) a commercial bank can create 100 euros of demand deposits for their clients.

⁴⁷Selgin and White (1996) argue that fractional reserve banking is a non-fraudulent and beneficial phenomenon of the free market and seek to defend "the freedoms to issue and use fiduciary media of exchange" (p. 83). Their broader views on competitive banking can be found in Selgin (1988) and White (1989). Their position on fractional reserve banking has been criticized by Hülsmann (1996) and Hoppe et al. (1998) who argue that fractional reserves are both fraudulent and harmful. For more studies along these lines see, for example, Block (1988), Huerta de Soto (1995), Salerno (2010, ch. 1) and Rothbard (2011, ch. 47). See also White (2014) for an interesting debate between some of the contenders. Interestingly, both camps claim to stand in the Misesian tradition on this issue and are not mistaken in that assessment. In his earlier work, von Mises is hesitant to condemn fiduciary media altogether (von Mises, 1912, Buch 3). This changed in his later career (von Mises, 1998, ch. XX). However, whether or not one regards fractional reserve banking as a violation of property rights and thus fraudulent is, although generally of great importance, irrelevant for the existence of the moral hazard as described above. Sympathizers of fractional reserve banking could regard it as a voluntary and anticipated moral hazard, while opponents of fractional reserve banking would see it as the result of a forced separation of ownership and control. We come down on the latter side, precisely because of the existence of legal tender legislation. For more references in support of full reserves across various different schools of economic thought, see footnote 27 on page 110.

to the systemic risk in financial markets. Under a fiat money standard, however, all the reserves required to pay out commercial bank clients can in principle be created almost immediately. The immediate systemic risk caused by fractional reserve banking can thus effectively be countered under fiat money. Indeed, a merely formal transition from fractional reserves to full reserves that would not change the workings of the modern banking system at all is perfectly conceivable. Such a transition would eliminate the risk of liquidity shortages caused by fractional reserves. The latter is thus not inherent in central and commercial bank induced credit expansion.

In fact, many countries have implemented partial or full deposit insurance for the case that commercial banks are unable to fulfill their obligations. The goal of these insurances is to promote financial market stability, but they do in fact incentivize clients to disregard the liquidity positions when choosing to patronize certain banks. Whether or not commercial banks operate prudently becomes less important from the perspective of legal tender users. It also becomes less important to gain information about the actual business operations of a bank in the first place. Commercial banks can thus engage in riskier behavior without having to fear a decline in patronage. In particular, they can operate on a higher leverage ratio.

We have already pointed out above that demand deposits can be created by commercial banks and extended to their clients in the form of credit, just like reserves are extended to commercial banks by their respective central bank in the form of credit.⁴⁸ Expansion of the money stock thus goes hand in hand with credit expansion. The latter is achieved by decreasing the central rates of interest, that is, the rates at which commercial banks can refinance their activities at the central bank. Lower central rates of interest allow lower market rates of interest, which in turn cause a higher credit volume overall. Credit financing becomes relatively more attractive and equity financing becomes relatively less attractive. There will thus be a transition from the latter to the former. The overall leverage ratio increases not only in the banking sector but in the economy as a whole. Hence, systemic risk as defined above will be higher than otherwise.⁴⁹

⁴⁸Commercial banks can also obtain reserves from cash deposits of their clients. These, however, are usually small as compared to the newly created reserves. Moreover, they are a zero-sum game, that is, cash deposits at one bank mean less cash deposits for other banks. Alternatively, there is the interbank market for refinancing. During the recession beginning in 2007 this market dried out, which led to unconventional central bank policies in order to recapitalize commercial banks (Giannone et al., 2012). For studies on the impact of unconventional monetary policies and quantitative easing, see Kapetanios et al. (2012) and Hashem Pesaran and Smith (2016). Giménez Roche and Janson (2015) argue that the expansion of commercial bank operations to areas outside of traditional lending activities has led to unconventional monetary policies that effectively transform central banks from *lenders of last resort* to *market makers of last resort*.

⁴⁹Gentier (2003) shows how the provision of credit in the regulated and legally privileged modern banking system itself is procyclical and argues that a free banking system without legal privileges would lead to a relatively more stable provision of credit. The latter would thus stabilize the economic system as a whole.

Mian and Sufi (2014), although not standing in the tradition of Austrian economics, have persuasively shown the central role of leverage for the severity of economic crises, in particular, for the Great Recession of 2007. They thereby follow and complement the important work of Kindleberger and Aliber (2005) who have argued that “the main driver of asset-price bubbles was almost always an expansion in *credit supply*” (Mian and Sufi, 2014, p. 107). They develop what they call the “levered-losses framework” (pp. 50-52) in order to explain the severity of the most recent housing bubble. It is essentially an adaptation of Irving Fisher’s famous debt-deflation analysis (Fisher, 1933), which they turn into an argument against debt instead of deflation. Their analysis and especially their conclusion are worth a closer look as they illustrate a broader point about central bank monetary policy interventions that we wish to make.

Their framework is based on two ingredients. First, they make the distinction between borrowers and savers, the former being highly leveraged homeowners with relatively low net worth. In contrast, savers have relatively high net worth. They lend to the borrowers and have the senior claim on the collateral that underlies the debt contract, for example, a house. They do this either directly as creditor or indirectly through financial assets they purchase. Second, the authors assume a collapse in housing prices.

It is through the leverage of borrowers that the effect of a collapse in housing prices on consumer spending is amplified. The leveraged household may lose its complete real estate equity. Their debt may even be much higher than the price at which the house could be sold on the market. As a result, they will reduce consumption expenditures in order to restore their net worth. This is an important empirical symptom of economic crises in general, and may be referred to as “forced saving” (von Hayek, 2008, pp. 118ff.; von Hayek, 1975, ch. VII). Underwater households may default on their debt for economic or strategic reasons, leading to foreclosures that push housing prices further down.

Moreover, Mian and Sufi (2014, p. 50) make the rather odd claim that “[t]he rich [the creditors] are protected against house-price declines not only because they are rich but also because they have a senior claim on housing.”⁵⁰ This perspective leads them to their proposed solution of what they call the “Risk-Sharing Principle” (p. 168). Its implementation would boil

⁵⁰It might well be the case that they can more easily deal with the price shock in terms of keeping a decent living standard, but they are of course fully exposed to the risk of price changes, precisely because houses serve as collateral in the debt contracts and their prices have an impact on the financial assets, such as collateral debt obligations, that they might have bought as investments. While their consumption expenditures might not change as drastically in response to the price shock, the latter will have an impact on their future investment decisions potentially away from real estate, which may likewise depress prices even further and engender reallocation processes characteristic of economic crises.

down to another intervention into the credit market. The unintended consequences of the initial intervention are countered with the next. This is the underlying idea of interventionist spirals, which we apply in section 8.3 in the next chapter.

The authors thus suggest that creditors should bear a higher risk in the form of an automatically diminishing principal for the debt contract if a specified price index for houses of the respective type falls below the original level at which the contract was signed.⁵¹ Mian and Sufi (2014, p. 173) correctly state that “[t]his downside protection comes at the expense of the lender. So the lender will need to charge a higher upfront interest rate to compensate for the downside risk.” While this would certainly lead to a smaller volume of credit and hence lower leverage overall, the authors do not engage in any discussion of the systematic manipulation of market interest rates by central banks in the first place. They completely neglect the role of central bank monetary policy in the determination of market interest rates and the overall volume of credit. In fact, they do not emphasize at all that the modern monetary system is capable of providing credit without actual savings, that is, without an actual sacrifice in the form of foregone consumption.

They ignore that the *savings glut* that they refer to as a cause of the housing bubble might be a phenomenon of monetary policy, that is, of politically induced credit expansion, rather than an outgrowth of genuine savings and consumption decisions by economic actors. Now, it goes without saying that once the created monetary funds are handed out in the form of credit, they will be spent on various goods and hence different components of the economic system will receive increased nominal revenues. Here, the created monetary funds will be split into savings and consumption expenditures. Credit, in the modern monetary system, is thus not necessarily the result of increased savings, but will in turn induce increased nominal savings afterwards.

Another peculiar feature of our inflationary monetary system is that the legal tender itself is likely to cease to be a suitable store of value from the perspective of its users as it systematically loses purchasing power. Savers are thus driven towards financial assets and derivatives that seem promising in protecting them against price inflation. Monetary expansion is thus conducive to the emergence of asset price bubbles not only because it increases the amount of available funds that

⁵¹They provide the following numerical example. Assume a borrower buys a house for \$100,000 with a \$20,000 down payment and a 30 year fixed-rate mortgage of \$80,000 at 5 per cent interest rate. This arrangement would require an annual payment of \$5,204 including interest and principal. If the respective house-price index falls by 30%, the annual payment for the next year would fall by 30% as well, leading to \$ 3,643. The borrower would keep \$ 14,000 (previously \$20,000) plus the proportion of the principle already payed back in real estate equity, instead of losing everything. If the price index increases again, the annual payments increase simultaneously, but never above the original amount of \$ 5,204. They call these credits “Shared-Responsibility Mortgages”. They can easily be generalized for all kinds of collateral. The authors advocate a similar type of debt contract for student loans that are made contingent on the job market situation after graduation.

can amplify them, but also because it incentivizes savers to actively seek investments with higher nominal returns to compensate for price inflation. As a general rule, higher nominal returns tend to be associated with higher risk.

As a specific example relevant to the most recent crisis, one can point to an increased demand for credit default swaps. They allow a transfer of uncertainty-bearing in a credit transaction and therefore have an amplifying effect on at least two accounts. First, they lead themselves to an increase of the supply of credit as the lender can transfer the costs associated with a potential credit default. Hence, the overall leverage in the economy will increase even further. Second, lenders face incentives to be less careful in selecting their borrowers. This magnifies the moral hazard problem we have identified for fiduciary credit expansion. More risky debt contracts become feasible as some of the costs can be imposed on others (Méra, 2014). An asymmetry of information about the creditworthiness of the borrowers has arguably contributed to the problem.⁵²

It is important to notice, that the latter is an additional source of higher risk. Of course, credit expansion as such always tends to be associated with higher default risks even if the selection of borrowers follows all dictates of economic prudence. The additional credit allows for the realization of investment projects that would not have been financed otherwise, precisely because they were perceived to be more risky than the ones that would have been realized either way.

We can thus conclude that interventions into the monetary sphere in the form of legal tender legislation and politically induced credit expansion systematically cause moral hazard and increased systemic risk in financial markets and the economy as a whole. Both materialize in more frequent and severe banking and economic crises (Calomiris and Haber, 2014). They cause unintended consequences from the perspective of stated public policy goals and thus motivate further interventions. The proposed credit market reform according to the risk-sharing principle (Mian and Sufi, 2014) is not yet an implemented intervention, but it illustrates the underlying dynamic well. This tendency towards further political interventions allows for a thorough revision of several aspects related to the cost-benefit analysis of central bank monetary policy. It allows, for example, a reinterpretation of one of the most discussed empirical relationships in modern macroeconomics.

⁵²The off-setting factor here is the price of credit default swaps on the financial market. But as pointed out above, demand for financial assets tends to be higher in an inflationary environment.

Chapter 8

Price Inflation and Unemployment: A Retrospective View on the Phillips Curve

If initial interventions lead to further interventions, then the immediate consequences of the latter are to be taken into account in the cost-benefit evaluation of the former. This is the underlying idea in the following reconsideration of the Phillips curve relationship. It adds an additional layer to the analysis. Not only are the immediate and inevitable economic consequences of monetary policies, as outlined in the previous chapter, considered, but also the reactions of policy makers to those consequences. The analysis is thus contingent on the choices political authorities make.¹ As such it leaves the narrow realm of economic theory as understood by von Mises (2007) and enters into the field of history. It does not claim an apodictic or *a priori* validity that holds across time and space.

Moreover, as a historical analysis it will represent an application of the Ciompanian notion of purely descriptive econometrics, presented in chapter 6. We will quantitatively describe and analyze the evolution of specific variables and their relationship for selected geographical regions over a chosen time span. It is in a sense an exercise in accounting for selected national economies.

We will look into data from France, Germany, the United Kingdom and the United States

¹It was Milton Friedman, among others, who suggested the importance of the political process in explaining the connection of unemployment and price inflation. In his Nobel Memorial lecture, he showed some discontent with his earlier work on inflation-unemployment dynamics and gave an interesting outlook: “[T]he third stage [of the analysis of price inflation and unemployment] will, I believe, be greatly influenced by a third major development - the application of economic analysis to political behavior [...]” Friedman (1977, p. 460). This research project has, however, not been undertaken systematically so far. In the following we present some efforts into that direction.

that reveal a positive correlation between price inflation and future unemployment in section 8.1. Next, we look into data on monetary growth, inequality and indebtedness in the respective regions. We find a rising gap between rich and poor that is positively correlated with money growth and indebtedness. These results are thus illustrative of the redistributive effects of credit expansion discussed in section 7.2.2 as well as the tendency towards increased leverage mentioned in section 7.3.3.²

8.1 Price Inflation and Unemployment over the Past 70 Years

Figure 8.1 shows more or less fragmentary time series of unemployment rates for the United Kingdom, the United States, France and Germany covering roughly the past 150 years. We can clearly observe the disturbing effects of the Great Depression during the 1930s with unemployment rates of up to 30 per cent in Germany and above 15 per cent in the United Kingdom. After the Second World War a convergence of unemployment rates towards a level which we might call *full employment* occurred. By 1960, unemployment rates were down to 1.7 per cent and 1.3 per cent in the United Kingdom and Germany, respectively. Until 1970, they remained below 3 per cent in both countries. In Germany unemployment was even below 1 per cent most of the time. Only in 1967 and 1968, it was 2.1 and 1.7 per cent, respectively.

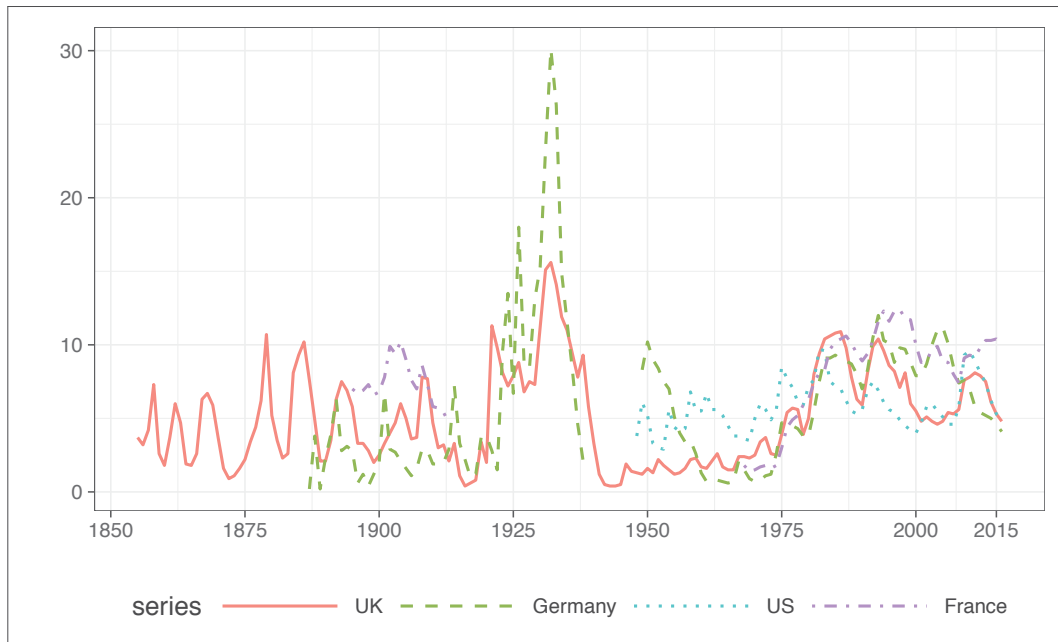
In the 1970s, unemployment rates initially remained at relatively low levels, but by 1976, they were up to 5.4 per cent in the United Kingdom and 4.6 per cent in Germany, which is still comparatively moderate by the standards of the decades to come. The unemployment rate in France at that time was around 4.4 per cent. However, subsequently we can observe a trend towards rates well above 10 per cent. In 1993, they are up to 10.4 per cent in the United Kingdom, 12.0 per cent in Germany and 11.6 per cent in France.³

The available data for the US tell a slightly different story. Although one might argue that they follow the same overall pattern, that is, a rising trend over the 1970s and 80s until the early 1990s, their amplitude is clearly smaller. Unemployment rates have neither been as low as in the UK, France and Germany in the 1950s, nor have they been as high in more recent decades.

²Parts of the following chapter have been published in Israel (2017).

³The chosen countries are no exceptions. We can observe the same trend in almost any other European country. An extremely drastic example is Spain, where we had an unemployment rate of only 1.5 per cent in 1968. Yet, from 1994 until 1997 it was above 20 per cent (Mitchell, 2007).

Figure 8.1: Unemployment rates in Germany (1887-1938; 1949-2016), France (1895-1913; 1968-20016), the United Kingdom (1855-2016) and the United States (1948-2016)



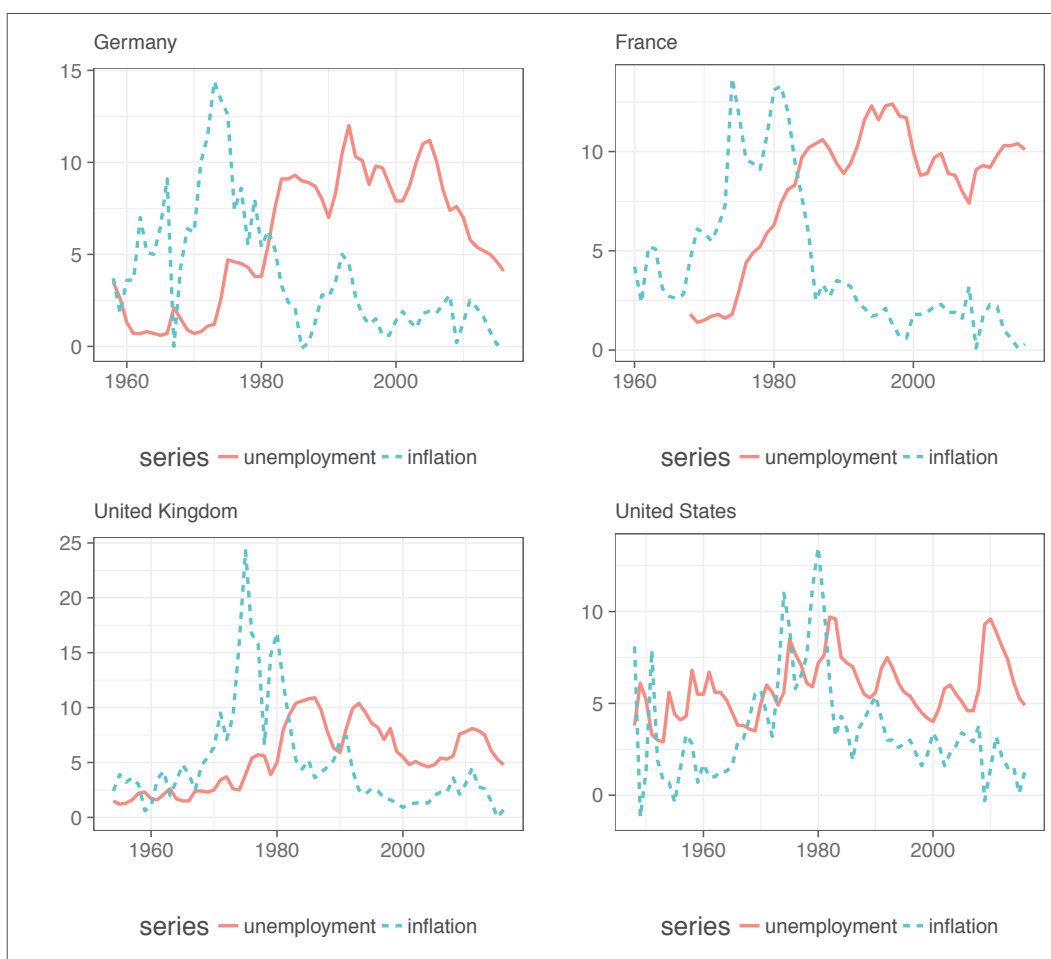
Sources of data: Mitchell (2007), for more recent data see Eurostat, for US and French data see data bases of the Bureau of Labor Statistics (BLS) and the Insee

How do these more recent developments of unemployment compare to fluctuations of price inflation rates? The top left panel of Figure 8.2 shows unemployment and price inflation rates in Germany from 1956 to 2004. If we look carefully, some episodes which might vindicate the short-run Phillips curve analysis become visible. In particular, around 1973, 1981 and 1991, decreasing rates of price inflation coincided with increasing unemployment. From 1986 to 1990, price inflation rates increased and unemployment fell. Yet, those periods never lasted longer than five years. We also find years in which price inflation and unemployment rates move in the same direction, contradicting the short-run Phillips curve. After 1990, both series seem to follow almost synchronous paths.⁴ Hence, the short-run relationship is empirically ambiguous, although it is negative overall (see Table 8.1 on page 276).

The top right panel shows the same plot for France. Again, we encounter various episodes in which unemployment rates and price inflation rates tend to move in opposite directions, as for example from 1981 to 1986, but also some in which they move in the same direction, like around 1973. Analogously, for the United Kingdom and the United States which are shown

⁴The two series still fail a formal test for cointegration, both for the entire period as well as the sub-period after 1993. If we fit a linear model between both series and apply the augmented Dickey-Fuller test to the residuals, we cannot reject the null hypothesis of non-stationarity on the 10 per cent confidence level. The p-values are 0.35 and 0.26 for the entire series and the sub-period, respectively.

Figure 8.2: Unemployment rates and price inflation rates for Germany, France, the United Kingdom and the United States between 1960 and 2016



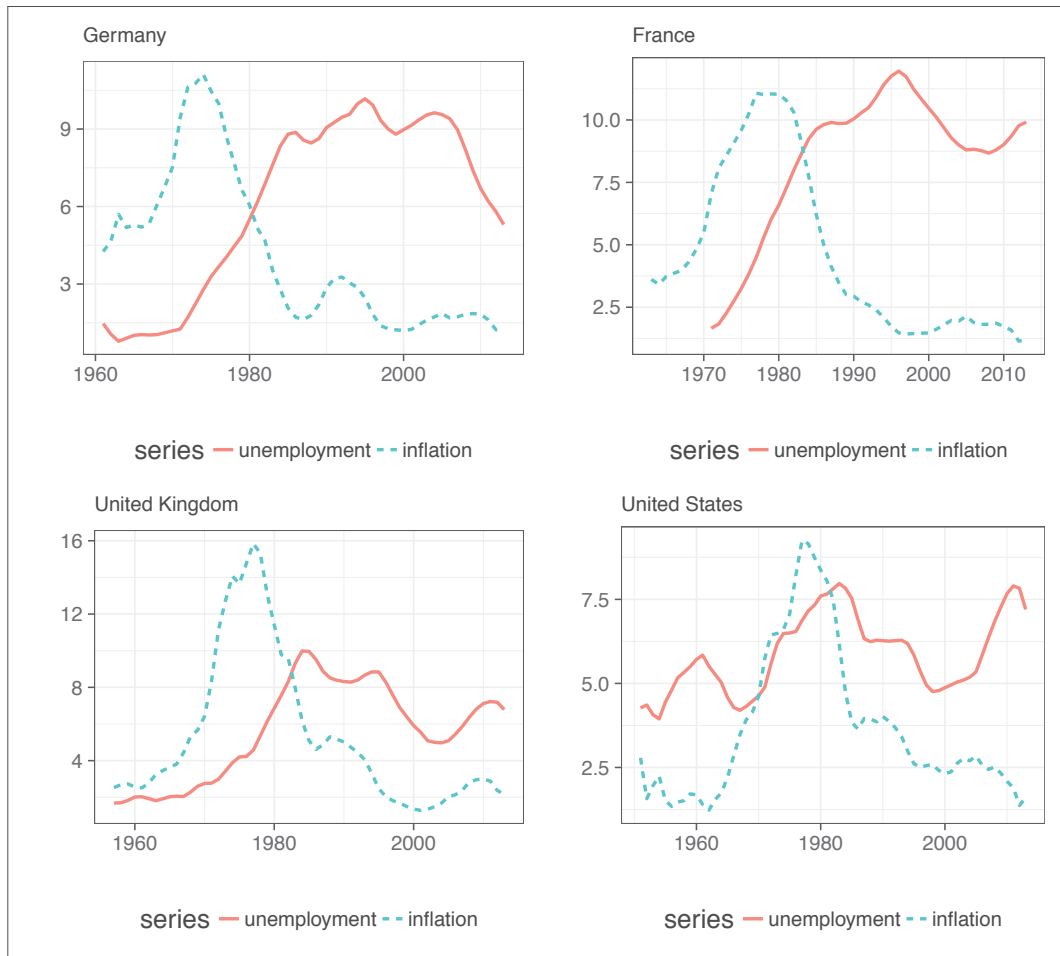
Sources of data: price inflation rates from Reinhart and Rogoff (2009); for unemployment rates of the UK and Germany see Mitchell (2007); for more recent data see Eurostat, for US and French data on unemployment see data bases of the Bureau of Labor Statistics (BLS) and the Insee

in the bottom panels of Figure 8.2, we find both, episodes vindicating and contradicting the short-run Phillips curve.

We note that the overall pattern in all four countries is astonishingly similar. In Figure 8.3, short-run fluctuations have been smoothed out by computing seven-year moving averages of both series.⁵ Far from being perfectly connected, it seems as if unemployment is following price inflation with a considerable time lag. Price inflation rates in Germany increased from 1960 to 1973. So did the rate of unemployment from 1970 to 1985. Price inflation rates after 1973 show a decreasing trend and unemployment remains rather stable, around 9 per cent, after 1985. It fell

⁵Each observation has been replaced by the arithmetic average of the seven observations closest to it with respect to time, which includes the observation itself, as well as the three preceding and subsequent observations.

Figure 8.3: Seven-year moving averages of unemployment and price inflation rates for Germany, France, the United Kingdom and the United States between 1960 and 2016



Sources of data: price inflation rates from Reinhart and Rogoff (2009); for unemployment rates of the UK and Germany see Mitchell (2007); for more recent data see Eurostat, for US and French data on unemployment see data bases of the Bureau of Labor Statistics (BLS) and the Insee

after 2004. The time lag between the three easily visible peaks in each series lie between 12 and 23 years.⁶

For France and the United Kingdom, the smoothed price inflation series can be separated into an upward sloping segment, followed by a downward sloping segment. The smoothed unemployment series also have a relatively steep upward sloping part. It is followed by a slightly downward sloping or rather stagnant segment. In France, there is again a time lag of about the same size as for Germany between the two maximum values - almost two decades. For the UK,

⁶For the smoothed price inflation series the three visible peaks occurred in 1963, 1974 and 1992. For the unemployment series they occurred in 1986, 1995 and 2004. The lag between the two global maxima is 21 years (1974 for price inflation and 1995 for unemployment).

the time lag seems to be smaller. It is less than a decade.⁷ If we shift the moving average of unemployment rates some seven years backward in time, its path would almost synchronously follow the moving average of price inflation rates, especially for the early half of the plotted time series. Since the 1990s they seem to co-move fairly synchronously without any shifts.⁸ In the case of the United States, the pattern is not as clear-cut. In fact, the time lag between both series seems to be substantially shorter, even shorter than for the UK.

Table 8.1: Bravais-Pearson correlation coefficient for unemployment rates and price inflation rates in Germany, France, the United Kingdom and the United States between 1960 and 2016; unemployment rates have been shifted backwards in time by the respective time lag.

time lag	Germany	France	US	UK
0	-0.56	-0.63	0.11	-0.06
1	-0.47	-0.50	0.37	0.10
2	-0.41	-0.37	0.48	0.19
3	-0.37	-0.26	0.48	0.24
4	-0.34	-0.16	0.39	0.28
5	-0.33	-0.10	0.32	0.36
6	-0.31	-0.05	0.37	0.47
7	-0.28	0.02	0.35	0.53
8	-0.22	0.08	0.25	0.52
9	-0.10	0.12	0.16	0.51
10	0.03	0.17	0.18	0.53
11	0.12	0.23	0.15	0.56
12	0.21	0.31	0.18	0.55
13	0.26	0.37	0.20	0.47
14	0.25	0.42	0.08	0.40
15	0.20	0.45	-0.05	0.36
16	0.20	0.47	-0.13	0.40
17	0.23	0.49	-0.17	0.42
18	0.30	0.49	-0.23	0.39
19	0.36	0.49	-0.28	0.31
20	0.45	0.41	-0.35	0.18

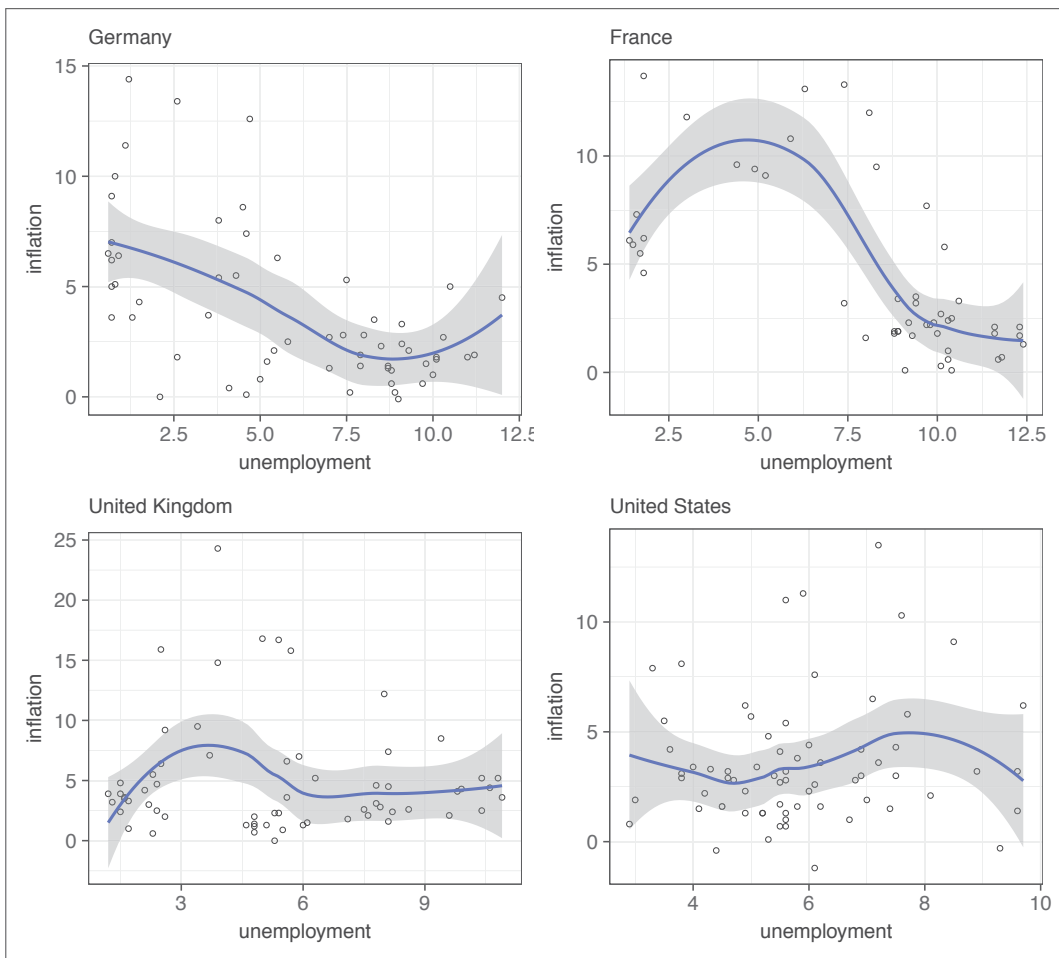
Sources of data: price inflation rates from Reinhart and Rogoff (2009); for unemployment rates of the UK and Germany see Mitchell (2007); for US and French data on unemployment see data bases of the Bureau of Labor Statistics (BLS) and the Insee

This is also reflected in Table 8.1, where the correlation coefficients between unemployment and price inflation in the US are maximized (0.48) when unemployment is shifted two to three years backward in time, instead of 11 to 20 years as for the other countries.

⁷In France the global maxima for price inflation and unemployment occurred in 1977 and 1996, respectively. In the UK it is 1977 and 1984. Hence, the lags between them are 19 and 7 years, respectively.

⁸A formal test for cointegration following the Engle-Granger method as previously used for the raw data from Germany leads to a rejection of the null hypothesis, for both the entire and truncated smoothed series of the UK.

Figure 8.4: Scatterplots of unemployment rates and price inflation rates for Germany, France, the United Kingdom and the United States between 1960 and 2016; loess smoother* (blue) with 95 per cent confidence band (grey)



*Loess or sometimes lowess (locally weighted scatterplot smoothing) is a non-parametric local regression method by which a smoothed curve is estimated from weighted local averages of observed values. Sources of data: price inflation rates from Reinhart and Rogoff (2009); for unemployment rates of the UK and Germany see Mitchell (2007); for more recent data see Eurostat, for US and French data on unemployment see data bases of the Bureau of Labor Statistics (BLS) and the Insee

Moreover, the table shows that for the United States and the United Kingdom there is very little evidence for a short-run Phillips curve trade-off. The Bravais-Pearson correlation coefficient for the UK without shifting the unemployment series (time lag equal to 0) has a negative sign (-0.06), being so close to zero that it is better interpreted as exhibiting no linear relationship at all. This is also shown in the bottom left panel of Figure 8.4. All other coefficients are positive and increase up to a certain point (0.56 for a time lag of 11 years) as we shift the unemployment series backward in time.⁹

In contrast, the data for Germany and France reveal a negative short-run relationship between unemployment and inflation as shown in the top panels of Figure 8.4. From Table 8.1 we see that the correlation coefficients gradually increase as we shift the unemployment series backward in time. The effect is somewhat weaker for Germany. The correlation coefficient is just slightly above zero (0.03) for a time shift of ten years but increases further with a growing time lag. The highest value is obtained for a two-decade shift (0.45).

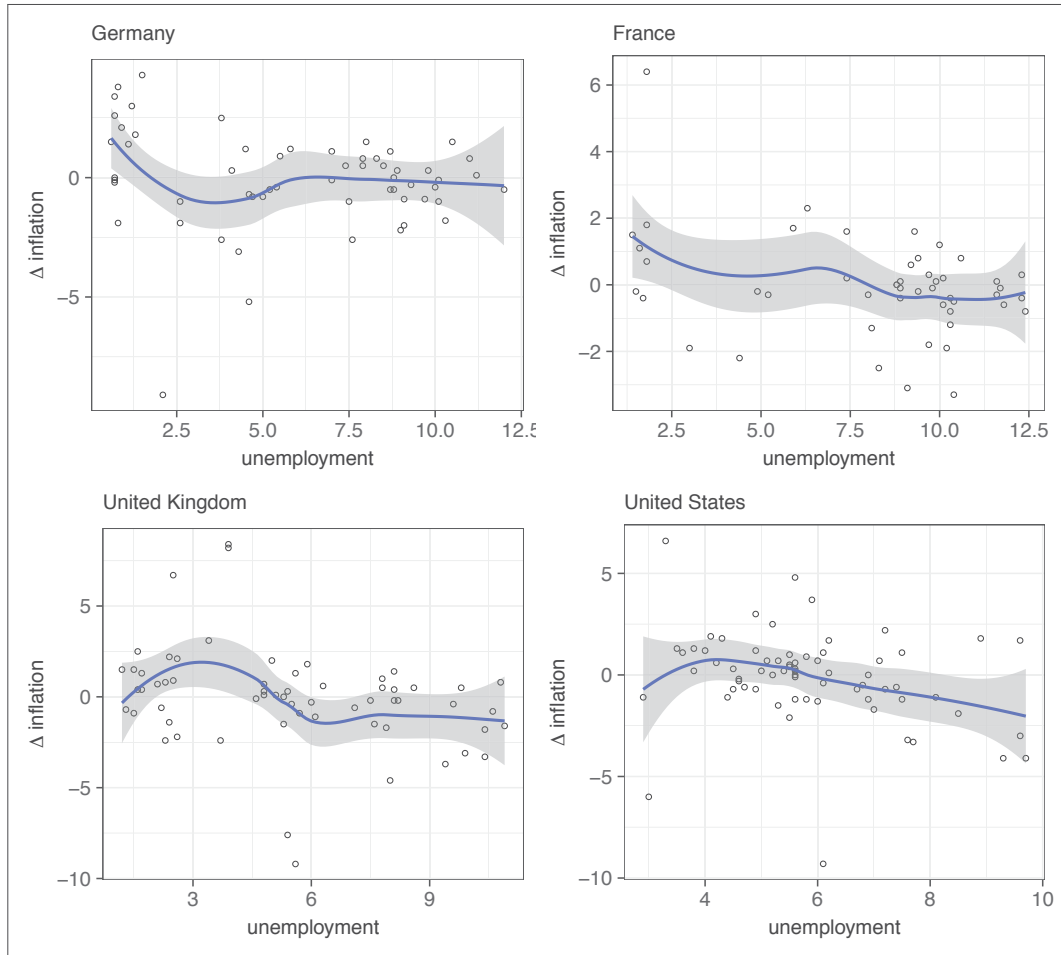
If we consider first order differences of the time series as shown in Figures 8.5 and 8.6, that is the changes of price inflation and unemployment rates from one year to the next, we also find evidence for a short-run Phillips curve trade-off in the UK and the US. An accelerating rate of inflation is associated with decreasing unemployment.

The scatterplots of the smoothed data for a time shift of ten years are given in Figure 8.7. Again, we can see the evidence for a positive long-run link between price inflation and unemployment for the United States and the United Kingdom, and somewhat weaker evidence for Germany and France. For the latter two countries the fitted loess smoother initially increases, suggesting a strong positive link, but for high unemployment rates, above 8 per cent in Germany and above 10 per cent in France, the positive link collapses. However, the suggested positive link becomes more apparent when we focus on a specified subperiod.

Interestingly, the observations of the upward sloping segments correspond in exact chronological order to the price inflation rates from the 1960s and 70s and accordingly to unemployment

⁹Spearman's rank correlation coefficient for the UK and the US increases up to values above 0.5 for shifts of thirteen and four years, respectively. This statistic has been considered for a very specific reason. Already Philipps argued that the relationship, on theoretical grounds, is "likely to be highly non-linear" (Phillips, 1958, p. 283). Investigating the relationship between the rate of change of money wages and unemployment, he argued that wages might be bid up rather quickly if very few workers are unemployed. Conversely, with a large number of workers being unemployed and the demand for labor being low, wages tend to fall slowly as workers are reluctant to offer their services at wages below the prevailing wage level. Under the assumption that the rate of change of money wages equals the increase of labor productivity minus inflation, the argumentation may well extend to the general price level within the economy. In this case, Spearman's coefficient would be a more appropriate measure as it looks for a monotonic, but not necessarily linear relationship. However, the differences between both statistics are not substantial and hence are not further discussed here.

Figure 8.5: Scatterplots of unemployment rates and first order difference of price inflation rates for Germany, France, the United Kingdom and the United States between 1960 and 2016; loess smoother (blue) with 95 per cent confidence band (grey)



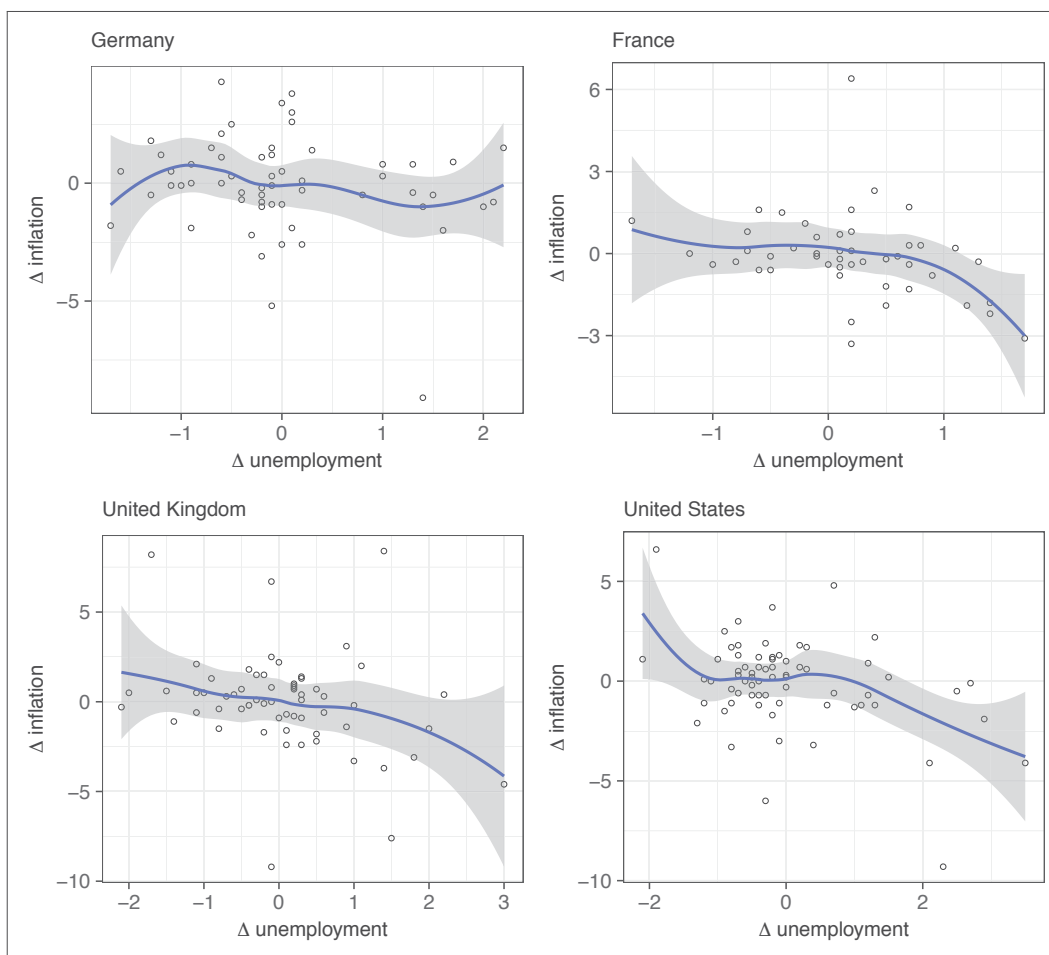
Sources of data: price inflation rates from Reinhart and Rogoff (2009); for unemployment rates of the UK and Germany see Mitchell (2007); for more recent data see Eurostat, for US and French data on unemployment see data bases of the Bureau of Labor Statistics (BLS) and the Insee

rates from the 1970s and 80s. In Germany, price inflation steadily increased from 1961 to 1974 just like unemployment ten years later, that is, from 1971 to 1984. After price inflation started to decrease in 1974, unemployment rates from 1984 onward continued to grow but at a lower rate. The latter peaks in 1995, leading to a time lag of 21 years between the two global maxima.

In France, price inflation rates increased throughout the 1970s and peaked in 1980. Respectively, unemployment rose throughout the 1980s. When price inflation started to fall after 1980, unemployment rates from 1990 onward kept increasing and peaked in 1996, leading to a time lag of 16 years between the global maxima.

The picture is slightly different for the UK and the US. In both cases the peak in unemploy-

Figure 8.6: Scatterplots of first order difference of unemployment rates and first order difference of price inflation rates for Germany, France, the United Kingdom and the United States between 1960 and 2016; loess smoother (blue) with 95 per cent confidence band (grey)

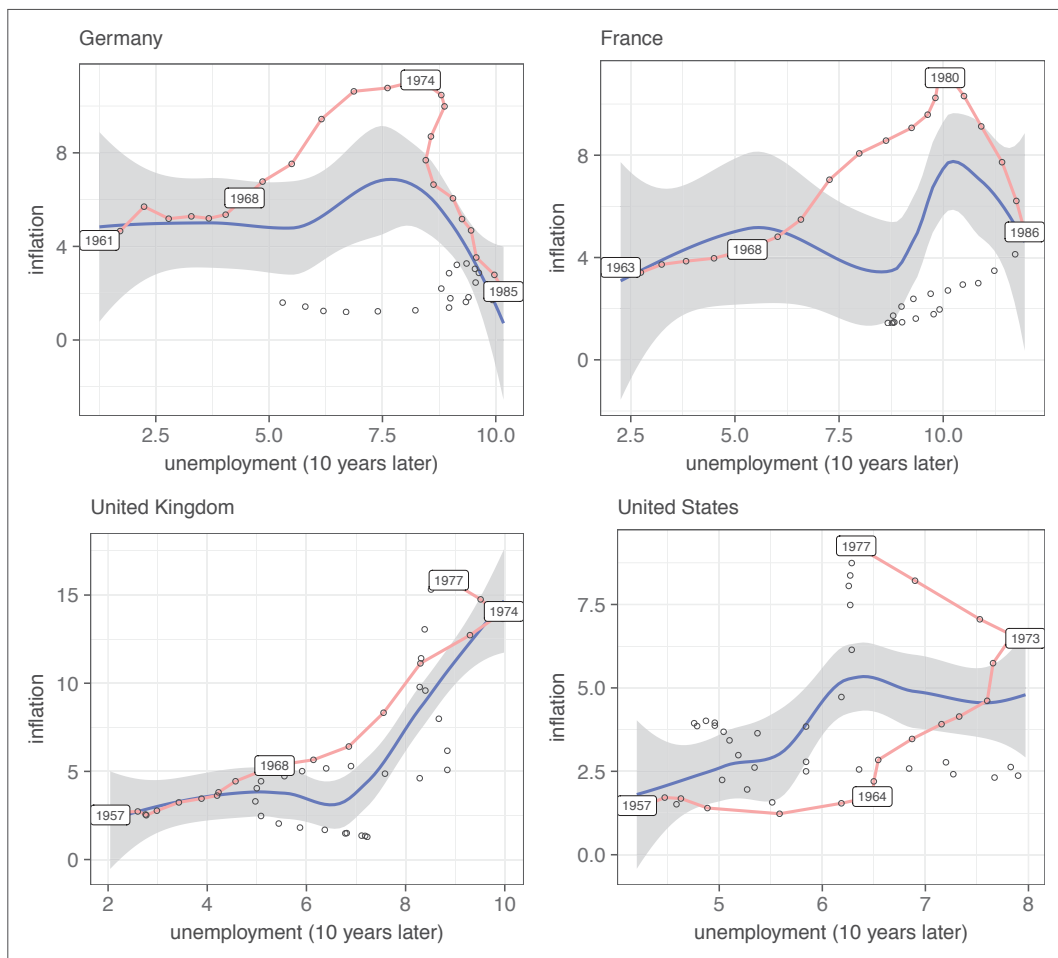


Sources of data: price inflation rates from Reinhart and Rogoff (2009); for unemployment rates of the UK and Germany see Mitchell (2007); for more recent data see Eurostat, for US and French data on unemployment see data bases of the Bureau of Labor Statistics (BLS) and the Insee

ment rates is reached in less than ten years after price inflation peaked. In the selected time path plotted in red, we therefore see unemployment diminishing before price inflation is at its maximum, in both cases in 1977. In the UK, unemployment peaked in 1984, with a time lag of seven years. For the US it was in 1983, giving a time lag of 6 years. In both countries the unemployment problem diminished earlier than in France or Germany.

It would be rather presumptuous to try to explain every particularity of the data. However, the overall positive link between present price inflation and future unemployment stands out. In section 8.3, we will provide an explanation for why this empirical finding should not come as a surprise, and why it might be worth rethinking long-run neutrality of inflation. Before, however,

Figure 8.7: Scatterplots of unemployment rates and price inflation rates as seven-year moving averages for Germany, France, the United Kingdom and the United States between 1960 and 2016; unemployment rates are shifted backwards in time by ten years; annotations of years refer to price inflation; loess smoother (blue) with 95 per cent confidence band (grey); selected time path (red)



Sources of data: price inflation rates from Reinhart and Rogoff (2009); for unemployment rates of the UK and Germany see Mitchell (2007); for more recent data see Eurostat, for US and French data on unemployment see data bases of the Bureau of Labor Statistics (BLS) and the Insee

we will have a look into the empirical evidence for some of the aspects discussed in the previous chapter.

8.2 Money Growth and some of Its Consequences

We have argued that higher indebtedness or leverage is a natural consequence of monetary expansion in the modern financial system. Moreover, as a result of ongoing monetary expansion, inequality in terms of the distribution of income and wealth will be higher than it would have been otherwise. We now turn to some observable magnitudes that may capture these tendencies. The following empirical analysis is again entirely descriptive and abstains from any inductive generalizations.

8.2.1 Wealth and Income Inequality

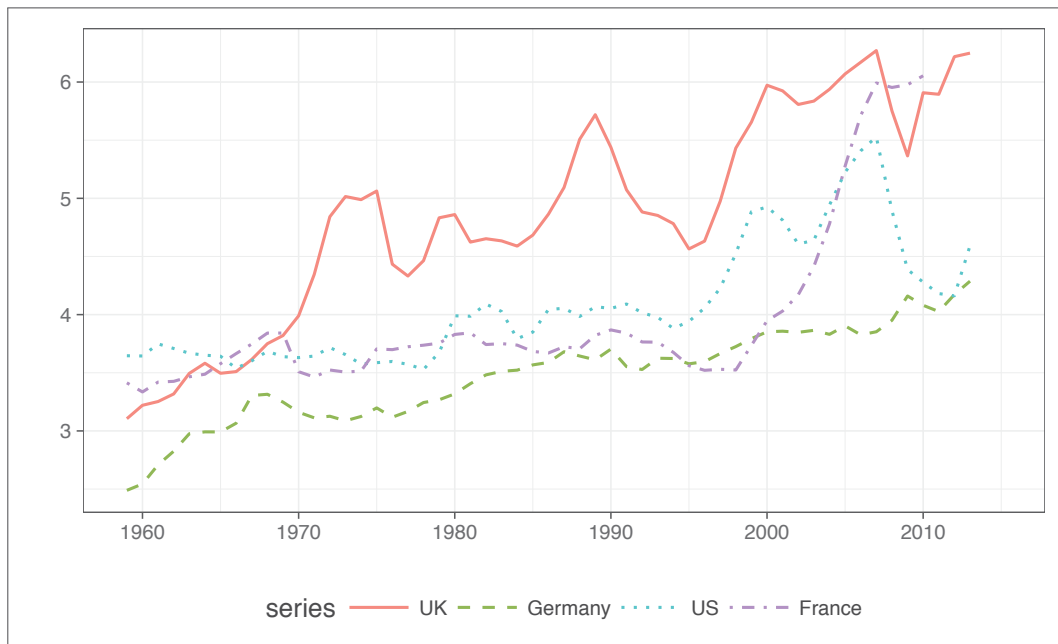
The wealth to income ratio in economies has become one of the most widely used measures in the growing research on inequality. An increased wealth to income ratio as a direct consequence of asset price inflation, for example, signifies that it has become harder to attain any given relative level of wealth for someone who starts from zero. It can thus be seen as a proxy measure of social mobility. Figure 8.8 shows the wealth to income ratio as one of the key indicators contained in the World Wealth and Income Database compiled by the extensive efforts of many researchers, most notably Thomas Piketty. This important magnitude has indeed shown an increasing trend over the second half of the 20th century up until today in all of the four countries selected for our empirical investigation.¹⁰

Figure 8.9 shows the shares of fiscal income of the top income brackets in the respective countries.¹¹ For the shares of total wealth, only data from France, the UK and the US are

¹⁰For the sake of clarity, we should emphasize that we have elucidated only one of potentially many causal chains affecting this magnitude. This is another practical reason why we cannot make any accurate quantitative statements about the impact. It is perfectly reasonable to assume that many other conditions of the political and economic system have an impact on the wealth to income ratio. Piketty (2014, pp. 169-172), for example, postulates what he calls “The Second Fundamental Law of Capitalism.” He argues that the long-term capital (or wealth) to income ratio is determined by the ratio of the savings rate over the rate of economic growth (s/g). Standard growth theory argues that it is determined by the savings rate over the sum of the growth rate and the rate at which capital depreciates ($s/(g + \delta)$). For a detailed discussion of the finer distinctions and implications see Krusell and Smith (2015). In any case, the rate of economic growth has a reverse impact on the wealth to income ratio. Any factor that increases or decreases the growth rate would also impact the wealth to income ratio, as long as it is not offset by a change in the savings rate. Autonomous changes in the savings rate would likewise alter the wealth to income ratio.

¹¹The concept of “fiscal income” as contained in the database refers to some measure of “total income that is or should be reported on income tax declarations (before any specific deduction allowed by fiscal legislation)” (Alvaredo et al., 2016, p. 16). It is a broader concept than “taxable income”, which is defined as “fiscal income”

Figure 8.8: Wealth to income ratio for Germany, France, the United Kingdom and the United States from 1959 to 2016



Sources of data: World Wealth and Income Database (*wid.world*). See also Piketty (2014).

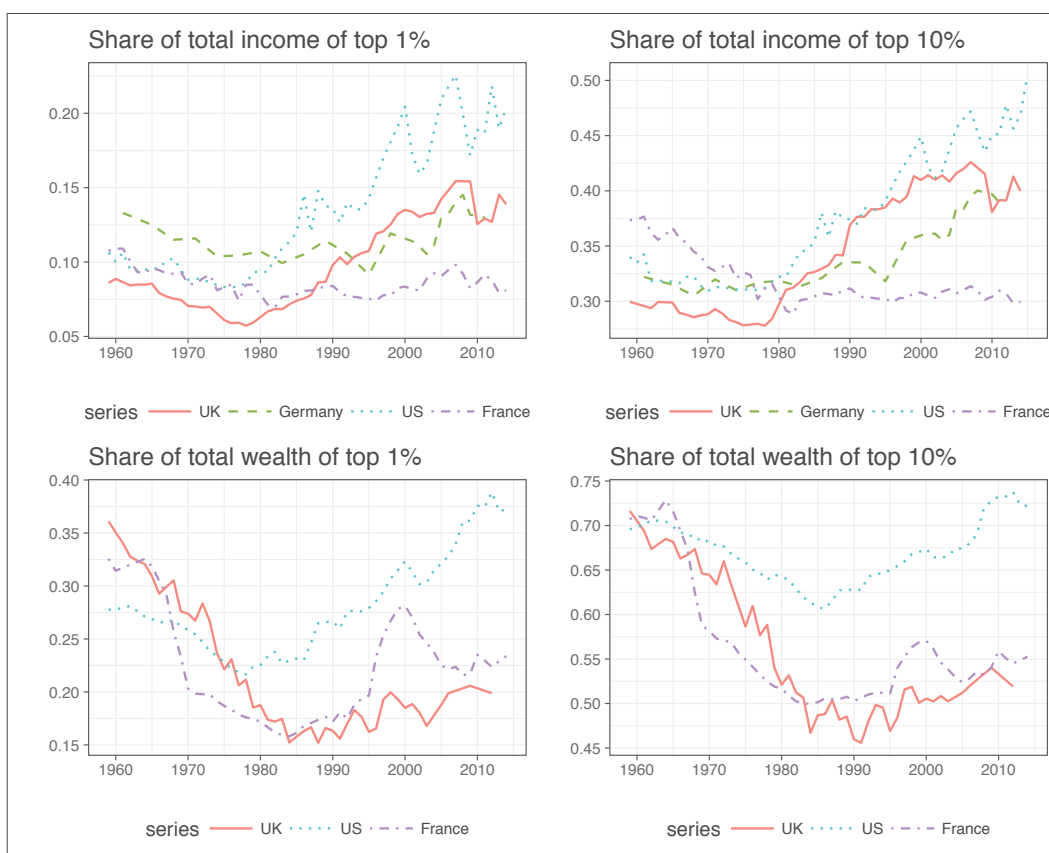
available.¹² The time series follow very similar paths in the top and bottom panels respectively. All income shares shown in the figure initially exhibit a decreasing trend through the 1970s and then turn upwards. We observe the strongest trend towards an increasing share of the top income brackets in the US and the UK. France, in particular, has a rather slow trend reversal if at all.¹³ The income share of the top 10% initially fell and then remained more or less constant. There is a very slight upward trend for the top 1%. The initial decreasing trend for shares in total wealth were generally stronger and lasted longer. By the early 1980s, however, the trends have also reversed. The strongest effect is again observable for the US. The statistical material thus shows that in the aftermath of the Nixon Shock of August 1971 there has been a trend reversal towards

minus income tax deductions, but it is still rather heterogeneous across countries because of varying tax legislation. This should be kept in mind when analyzing the data. The heterogeneity may lead to biases as the authors themselves point out. They are working on an even broader and more homogeneous concept, which they call “pre-tax income,” as well as a corresponding “post-tax income,” which will be very useful in assessing the extent to which income taxes in the various countries counterbalance inequalities. However, the data series have not yet been compiled for most countries in the database. We will make use of the US data below in section 8.3.

¹²The wealth concept we use here is “net personal wealth,” which is defined as the sum of non-financial and financial assets owned minus financial liabilities (Alvaredo et al., 2016, p. 38).

¹³The trend reversal is slightly stronger for the “pre-tax income” measure, which is available for France. Between 1983 and 2014, the pre-tax income share of the top 1% and 10% has increased by 3.47 and 3.21 percentage points, respectively. This, of course, implies that the share of the income group between the 90th and 99th percentiles, or the bottom 90% of the top 10%, if you will, has diminished by 0.26 percentage points. The relative gains have thus been entirely in the top 1% income group.

Figure 8.9: Wealth and fiscal income shares of the top 1% and 10% in Germany, France, the United Kingdom and the United States from 1959 to 2016



Sources of data: World Wealth and Income Database (*wid.world*). The shares of total wealth are not available for Germany. For the methods used for data collection and calculation see Alvarado et al. (2016).

a higher share in income and wealth for the top 10% (1%) and by implication a lower share for the remaining 90% (99%) of income earners and wealth owners.

A closer look at the US seems expedient as it most strongly illustrates the point we wish to make. The upper left panel of Figure 8.10 shows the development of the pre-tax income shares of the top 1% and the bottom 50% beginning in 1971. The share of the lower half of the income ladder has diminished to almost the same extent as the share of the top 1% has increased. The latter grew by about 9 percentage points while the former fell by 8 percentage points. The pre-tax income of the remainder as an aggregated group thus remained more or less constant. There has effectively been a rising gap between the bottom 50% of income earners and the top 1%, at least when abstracting from corrections by taxation and redistribution.¹⁴

¹⁴We cannot perform the same analysis for the United Kingdom and Germany, since the pre-tax income data are not available. For France they are, and we find that pre-tax income shares of the top 1% and 10% are higher than the plotted fiscal income shares in Figure 8.9. The increasing trend since the late 1970s and early 1980s becomes

Figure 8.10: Pre-tax income shares of the top 1% and the bottom 50% in the United States from 1971 to 2014 (upper left panel) in comparison to the S&P 500 stock market index and the Consumer Price Index



Sources of data: World Wealth and Income Database (wid.world) for income shares. S&P asset price data as well as CPI data retrieved from <http://www.econ.yale.edu/~shiller/data.htm>. It has been used, for example, in Shiller (2015) and is revised on a constant basis. Note: The income share in the right panels has been multiplied by 1,500 (upper panel) and 15,000 (bottom panel). This does not affect the Pearson correlation coefficients, but only renders the relationship more visible without skewing it. We purposefully abstained from plotting two series in one graph on two different y-axes to make them overlap nicely, as has been done, for example, in several graphs in Shiller (2015). Some experts argue that graphs with more than one scale on an axis are fundamentally flawed and misleading (Few, 2008).

Since 1971, there is a very strong positive correlation between the pre-tax income share of the top 1% and the S&P 500 stock market index as well as the Consumer Price Index (CPI) ($r=0.97$ and $r=0.95$, respectively).¹⁵ This is shown in the two right panels of Figure 8.10. The S&P index can be seen as an indicator of asset price inflation, while the CPI is the most readily available measure of consumer price inflation. Asset price inflation, in particular, is a phenomenon that first of all benefits wealthy asset owners and puts others at a disadvantage, since it becomes generally more difficult to acquire assets, but those who already possess them make gains.¹⁶

The bottom left panel of Figure 8.10 suggests that estimated consumer price inflation has been much lower than asset price inflation over the whole period under consideration.¹⁷ However, also consumer price inflation has disproportionately hurt the bottom 50% income group, since their share of income has diminished as shown in the upper left panel. Consumer price inflation has posed less of a problem for top income earners since their share has increased simultaneously.

8.2.2 Indebtedness

The second consequence that we have discussed previously and that is intimately related to the notion of systemic risk is indebtedness or leverage. Expansionary monetary policies render credit cheaper than it would be otherwise and hence decrease the relative costs of credit finance as compared to equity finance. Figure 8.11 contains measures of the money stock as well as public and private sector debt in the US, the UK and the eurozone. Availability of data varies over the three regions. In particular, aggregated measures of private and public debt retrieved from the Eurostat database are available only from 1995 onward. This includes the private sector debt

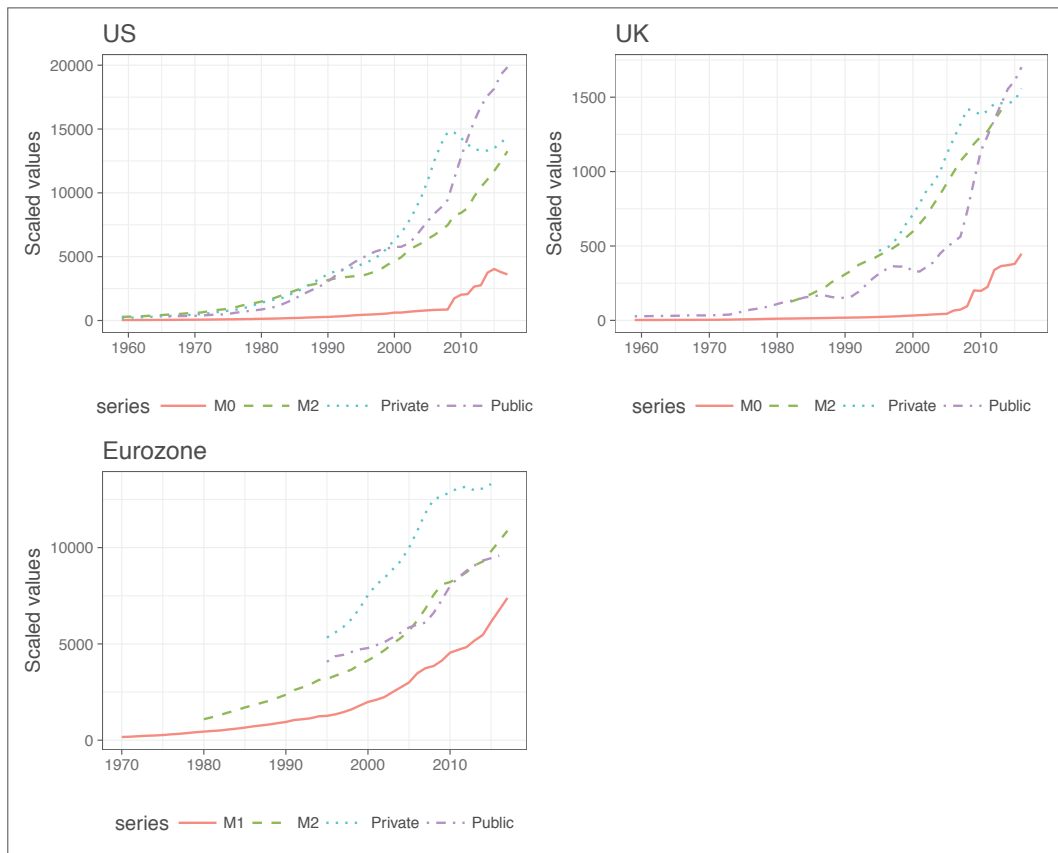
stronger when pre-tax income is considered. On this see footnote 13. Unfortunately, the post-tax income data has not yet been calculated, which renders a comparison between pre-tax and post-tax income impossible. Such a comparison would provide us with an indicator of how fiscal measures in France counterbalance the underlying trend towards more income inequality. This would be an interesting topic for further empirical research. We will come back to this issue in the case of the US, for which both pre-tax and post-tax income data are available, in section 8.3.

¹⁵Over a longer time span, the correlation is substantially weaker but still positive. From 1913 until 2015, the correlation between the CPI and the top 1% pre-tax income share is 0.38. Between the S&P 500 index and the income share over the same period it is 0.57. This suggests a closer connection between asset price inflation and the top income share, rather than consumer price inflation.

¹⁶Another potential impact of monetary policy on asset prices has been documented in Aubin et al. (2013). For the Federal Reserve System, it is shown that monetary policy may have led to an increase in the volatility of asset prices.

¹⁷Note that this kind of quantitative comparison is not adequate for the panels on the right-hand side of Figure 8.10, since the income share has been re-scaled. In the bottom left panel both indexes start at 100 in 1971. A similar indexation for the right-hand side panels would have been possible, but not very useful. The CPI and S&P 500 indexes have increased by factors of six and twenty, respectively, while the income share has roughly doubled. The path of the income share would not have been clearly visible under the same indexation. It would have been compressed at the bottom of the diagrams.

Figure 8.11: Monetary aggregates and measures of private and public indebtedness in the US, the UK and the euro area from 1959 to 2017



Sources of data: Monetary aggregates were retrieved from the FRED database of the Federal Reserve Bank of St. Louis. We retrieved the “Mortgage Debt Outstanding of all Holders” series as a measure of private sector debt and the “Federal Debt: Total Public Debt” series for the US, as well as the “Public Sector Debt Outstanding” series for the UK from the same database. The private debt measure for the UK is the “consolidated private sector debt” series from the Eurostat database. The eurozone debt measures have also been retrieved from Eurostat. The public debt measure for the eurozone is the “general government gross debt” series for all 19 euro members, defined on the Eurostat website as “consolidated general government gross debt at nominal (face) value, outstanding at the end of the year in the following categories of government liabilities (as defined in ESA 2010): currency and deposits, debt securities and loans. The general government sector comprises the subsectors: central government, state government, local government and social security funds.” The private debt measure has been calculated as the sum of the “consolidated private sector debt” series from France, Germany, Greece, Italy, Spain, Portugal, Belgium, Austria, Netherlands and Finland. Data for other eurozone countries is incomplete or nonexistent. Note: the time series have been re-scaled in order to render their empirical relationships more visible.

measure for the UK as well as both debt measures for the eurozone. The data are again most complete for the US.

In the US, the base money supply that is directly controlled by the Federal Reserve System has increased by a factor of 16.9 between 1966 and 2008. Over the same period, the broader monetary aggregate M_2 has synchronously increased by a factor of 16.2. Public debt has increased substantially more than that. It grew by a factor of 29.4, that is, almost twice as much.

However, especially private indebtedness is of importance when assessing systemic risk. Over the same period, the private debt volume has increased disproportionately by a factor of 41.4. If it had increased to the same extent as the money stock the relative private credit volume with respect to the overall money stock would not have changed. Since it has by far exceeded money growth, we can state that relative indebtedness has increased and private credit has disproportionately grown until the beginning of the Great Recession, as would be expected from an expansionary monetary policy that renders credit relatively cheap. The increased leverage has undoubtedly contributed to the severity of the recession (Mian and Sufi, 2014).

The empirical evidence in the UK is less clear-cut, but the fragmented series we have at our disposal reveal a very similar picture to that of the US. First of all, data on private debt is only available from 1995. We can see that it has since then increased quicker than the M_2 money stock. More precisely, from 1995 until 2008, private debt in the UK has increased by 200%, whereas M_2 has only increased by 160%. The growth of public debt has accelerated in the 2000s, especially during the Great Recession.

For the eurozone, we cannot find the same evidence of disproportionate credit growth as for the UK and the US. The monetary aggregate M_2 and the private debt measure have increased by 38% and 35%, respectively. The problem involved is twofold. There is no aggregated private debt measure readily available in the Eurostat database and private debt measures for individual member countries are fragmented and start at different points in time. We have simply calculated the sum of all the series that cover the whole period from 1995 onward. Other countries are left out, which might lead to biases.

In all three regions, we observe that public debt has increased sharply in the course of the Great Recession. In the US and the UK, the monetary base that is under direct control of the respective central banks has sharply increased since the crisis.¹⁸ These are reflections of strong fiscal and unconventional monetary policy reactions to the crisis. Deficit spending and quantitative easing or large scale asset purchases by central banks are relevant examples. Private debt has decreased since 2008, or at least its growth rate has decreased, which is a common feature of recessions, during which more debtors default on their loans than usual. This can be seen as a correction of excessive leverage accumulated over the pre-crisis period.

However, when interest rates are already close to zero and agents in the private sector are generally not willing to take out more loans for investments, conventional monetary policies

¹⁸For the eurozone, there is no readily available measure of the monetary base. Hence, we used M_1 in the plot instead of M_0 .

have only a limited effect and monetary authorities have to resort to unconventional measures to inflate the money stock. As the ratio of the money base to larger monetary aggregates decreases, a situation of excess reserves over and above the minimum reverse requirements for commercial banks emerges. Unless reserve requirements are adequately adjusted or other corrections are undertaken by central banks, such a situation leaves commercial banks in a position to rapidly inflate the money supply once firms and private agents are willing to take out loans again. This could potentially finance another rapid and unsustainable boom or inflate another asset price bubble. However, monetary policy measures are not the only aspect to consider. For a bigger and more complete picture, and especially for an explanation of the long-term developments of real macroeconomic aggregates an analysis of other policy interventions is indispensable.

8.3 A Positive Long-Run Link

We have seen in section 8.1 that there is a positive empirical connection between present price inflation and future unemployment over the second half of the 20th century. In the remainder of the chapter we explore one causal connection between the two variables and show how we can account for this observation. Of course, part of the empirical connection may be merely accidental or due to other causal chains unexplored here, but we show how at least one link between the variables can be established. This link turns out to be an indirect one that hinges on more than just monetary policy interventions. Yet, the logical starting point for our analysis remains an ongoing expansion of the money stock.

8.3.1 The Phillips Curve in the Short, Medium and Long Run

As seen in the previous sections, money growth has been persistent from the post-war years until today. One consequence of an expanding money stock, if it outweighs deflationary factors, such as economic growth, technological progress or a potentially increasing demand for money, is price inflation. Moreover, monetary expansion typically occurs in the form of credit expansion. One important consequence of the latter are business cycle fluctuations as explained in the previous chapter. Austrian business cycle theory provides the underlying rationale here.

However, remaining somewhat more agnostic about the ultimate cause of economic crises, one can argue that the politically induced reduction of interest rates contributes to systemic risk

via increased leverage and therefore magnifies the severity of crises once they occur. Some evidence of disproportionately increasing leverage has been presented in the previous section.

An important aspect of the business cycle are synchronous fluctuations in the rate of employment. We have thus established a link from the underlying monetary policy to both variables of interest, price inflation and the rate of unemployment. Let us explore the connection further by incorporating the timing of events.

Unemployment tends to fall during economic upswings. If the latter are caused by a sufficiently large credit expansion, the price level tends to increase. As pointed out previously, this provides a theoretical explanation for a negatively sloped short-run Phillips curve, that is, the coincidence of increasing price inflation and decreasing unemployment. When the inflationary boom, after a certain time period, turns into a bust, unemployment rates tend to increase. This, in and of itself, does not, however, establish a positive link between price inflation and unemployment in the *long run*. Eventually, after the boom period with lower unemployment and the bust period with higher unemployment, the rate returns to its natural level as determined by the institutional environment (Bellante and Garrison, 1988).

Hence, starting from an economy in which the actual and natural rates of unemployment are identical, we can argue that the expansion of money and credit, over the course of the business cycle, pushes the actual rate of unemployment below the natural rate in the short run, but eventually causes unemployment to increase above the natural rate in the medium run. After the business cycle, that is, in the long run when all the consequences of the initial expansion have played out, the actual rate returns to the natural rate of unemployment. The analysis is therefore compatible with the Friedman and Phelps natural rate hypothesis that includes a negatively sloped short-run Phillips curve and a vertical long-run Phillips curve. However, we add a third element: a positively sloped medium run Phillips curve.

Interestingly, Ravier (2013) argues that there might be a permanent positive impact on unemployment. However, he does not make sufficiently clear that his argument is contingent on politics outside of the monetary arena. He starts from a situation with minimum wage legislation, arguing that, due to capital consumption and destruction during the business cycle, labor productivity may have fallen so much that the existing minimum wages lead to increased unemployment. Yet again, in the long run, through a genuine process of capital accumulation based on real savings, labor productivity may reach and even exceed its prior level.

Moreover, nominal wages might have risen so much in the inflationary process that paying

minimum wages, which are fixed in nominal terms, does not actually pose any problems for employers. Abstracting from minimum wages and unemployment benefits, it is even conceivable that employment increases after the business cycle, namely, when capital destruction has impoverished society to such an extent that it precipitates lower wage elasticities of the supply of labor. To be fair, strictly speaking, in this scenario we would not be dealing with a reduction in unemployment of the kind that we are really concerned with, that is *forced* or *institutional unemployment* (von Mises, 1998, pp. 598ff.; Hutt, 2011, p. 73). What we would have here is a case in which *voluntary* or *preferred unemployment* (Hutt, 2011, pp. 38ff.) in a society that enjoys relatively high living standards has been transformed into *forced unemployment* in the impoverished society after the business cycle. On a free market for labor this would lead to increased employment (von Mises, 2000, p. 57).

These considerations show that it is a rather difficult endeavor to establish a necessary, time and place invariant long-run relationship between price inflation and unemployment that is independent of further political interventions. Ultimately, the height of unemployment is determined by restrictions and rigidities that are politically forced upon labor markets (Sennholz, 1987). As von Mises (1990a, p. 125) points out: “At the equilibrium wage rate unemployment is only a transitory phenomenon.” Long-term mass unemployment occurs when wage rates are not free to equalize supply and demand for labor, either directly through minimum wage legislation, or indirectly through pressure from labor and trade unions (Hutt, 1954). Rueff (1925a, 1931) emphasized the role of unemployment benefits, which can themselves be interpreted as quasi minimum wages, below which incentives to work are drastically diminished.

8.3.2 Adding the Extra Layer of Politics

One promising route to establish the connection to long-run unemployment is politics as Friedman (1977, p. 460) suggested, although he himself abstained from conducting the analysis. Politicians react to developments that are generally considered harmful for society. What are then the typical political reactions to economic crises and downturns?

Expansionary central bank policies, which we suggest are an important cause of economic downturns and higher unemployment in the medium run, can be instrumental in nursing public support for higher taxes and more rigid interventions into labor markets.¹⁹ These further

¹⁹For the specific case of the United States see Higgs (1987), who provides a detailed documentation of how crises triggered further political interventions. For France, see in particular Maurin (2009), who lists several interventions into the labor markets and links them directly to economic crises.

interventions, when implemented, have the objective of preventing economic mischief in the future. Yet, in general, they also tend to render labor markets less flexible. As a result, they might increase unemployment and decrease output in the long run.

We might say that inflation itself produces political incentives that tend to shift the Phillips curve, in the words of Milton Friedman, towards a higher *natural rate of unemployment*, or what von Mises much more appropriately termed *institutional unemployment*. In other words, the political decision for a movement along the short-run Phillips curve towards more price inflation and less unemployment may inherently trigger a rightwards shift of the entire Phillips curve schedule through the political process of interventionism.

At this stage we can broaden the analysis to the more general consequences of expansionary monetary policy. For example, Mayer (1978, p. 40) pointed out that “[price] inflation can easily lead to political pressures for the imposition of wage and price controls.” We would expect these measures to render markets more rigid and increase unemployment.

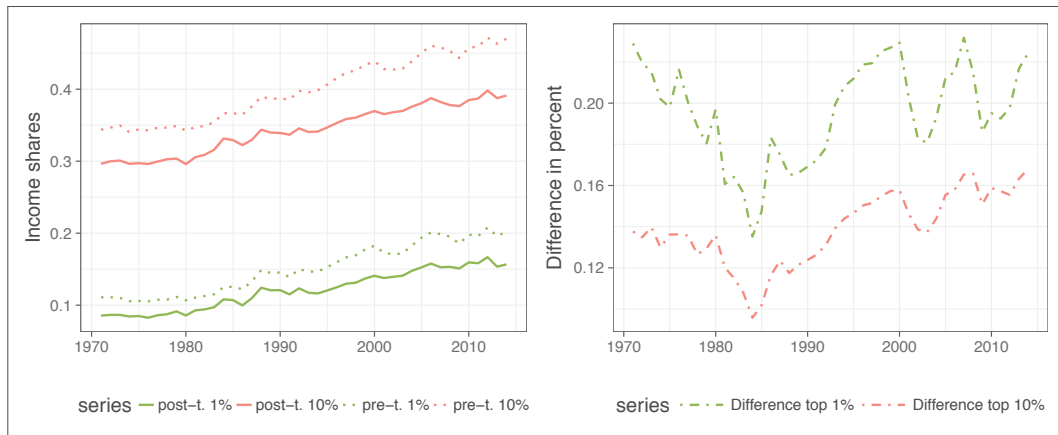
Moreover, increasing inequality, as theoretically and empirically analyzed above, motivates further political interventions, especially in countries governed by egalitarian politicians, who are more likely to be voted into office when redistributive effects of inflation foster egalitarian sentiments among the electorate (Brown, 1988). Insofar as increasing inequality is the result of Cantillon effects, we can again trace a link between price inflation as the direct consequence of the initial intervention of monetary expansion and increased unemployment as one of the direct consequences of further interventions that follow.

For example, more power might be given to trade and labor unions in collective bargaining. Labor laws might be adjusted in order to protect and support employees, and in particular low wage earners. Minimum wage and job protection laws are cases in point. These interventions generally render labor markets less flexible and are important causes of higher long-term unemployment rates (Hutt, 1954, 2011; Nickell, 1997).

Figure 8.12 contains the pre-tax and post-tax income shares of the top income earners in the United States from 1971 to 2014. We can see again that they were growing over the period, but interestingly, computing the differences between the two measures shows that from the mid 1980s onward there is a growing fraction of the income taxed away from these groups. This indicates that new fiscal measures have indeed been implemented.²⁰

²⁰As a matter of fact, Ronald Reagan signed the *Economic Recovery Tax Act* in 1981 and the *Tax Reform Act* in 1986, both of which are commonly referred to as the two Reagan tax cuts. For some income groups they certainly were, but they also broadened the tax base and eliminated loopholes. Moreover, they shifted part of the tax burden towards corporations.

Figure 8.12: Pre-tax and post-tax income shares of the top 1% and the top 10% in the United States from 1971 to 2014; difference in percent (right panel)



Sources of data: World Wealth and Income Database (*wid.world*).

On a much more fundamental level, growing inequalities are instrumental in generating public support for a stronger welfare state. In the short run, a growing welfare state might be financed by further monetary expansion and deficit spending. In the long run, however, it can only be sustained through tax increases. The corresponding political measures have of course numerous effects, but regardless of possible advantages, when it comes to employment, they can only have a negative impact.

Higher tax rates render businesses less profitable. There will be less investments and fewer workers will be employed (e.g. Rothbard, 1977, ch. 4; Reisman, 1998, chs. 9, 10 and 11; Hoppe, 2006, ch. 2; Salin, 2014). If labor laws increase the responsibilities and obligations of employers towards their employees, there are fewer incentives to hire people. Therefore, unemployment tends to increase, even more so, when the welfare state takes away incentives to work through unemployment insurance (Rueff, 1931).

Furthermore, large firms and corporations that are well established and connected on the market are benefited by credit expansion and inflation, since they can refinance their activities much more easily on the financial markets than smaller firms and newcomers (Hülsmann, 2008, pp. 180ff.). This provides big businesses with the opportunity to operate under higher leverage ratios. Hence, credit expansion serves as a means to deprive themselves of unpleasant competition.

Higher tax rates are effective in the same way. Successful and innovative newcomers usually satisfy the needs of consumers better than their competitors, because they improve an existing product or develop a completely new one that consumers prefer and demand. Therefore, they

obtain relatively high revenues, at least temporarily while the competitors adjust their own products. Yet, if a larger share of their revenues is taxed away, they partly lose their most important advantage that would help them to hold their ground and compete against well established firms and corporations for a longer time.²¹

To the extent that credit expansion benefits large firms and corporations at the expense of smaller firms and newcomers, it reduces competition among employers and businesses, destroys job opportunities and demand for labor, and hence tends to increase labor market rigidity and unemployment.²² ²³ This effect may even be reinforced under a corporatist government, as opposed to the egalitarian version mentioned above. In recent years, corporatist inclinations have manifested themselves among other things in the *too big to fail* argumentation (Stern and Feldman, 2004; Ennis and Malek, 2005).

At this point, it should be mentioned that corporatist and egalitarian governments are not mutually exclusive categories. One and the same government could, for example, pursue corporatist measures when it comes to economic policies, and egalitarian measures when it comes to welfare policies.

Finally, the effect of the bracket creep under a system of progressive taxation should not be neglected (Heer and Süßmuth, 2013). When incomes are pushed into higher tax brackets through inflation, the private sector is deprived of a larger proportion of its income. This effectively diminishes the capacity and the incentives to save and invest, and thereby tends to lower output and increase unemployment.

²¹This problem has implicitly been acknowledged by some governments who introduced tax exemption schemes for start-up companies, like the Singaporean government in 2005.

²²It should be mentioned that this latter chain of reasoning seems indeed to be independent of further political interventions. It is merely dependent on the specific kind of monetary expansion as credit expansion. It might thus qualify as an argument for a necessary, time and place invariant, connection between inflation and labor market rigidity. We intend to develop this line of argument in further research.

²³This view of course stands in sharp contrast to Schumpeter's take on credit expansion and inflation. He characterized it as a means to finance the ventures of bright entrepreneurs with innovative ideas who lack capital. In his view, it increases competition and innovation (Schumpeter, 1983, ch. 3). However, Hülsmann (2008, pp. 181-182) provides a suitable rebuttal:

Indeed, the economist Joseph Schumpeter has famously characterized fractional-reserve banks as being some sort of mainspring of economic development.[...] He argued that such banks may use their ability to create credit out of thin air (*ex nihilo*) to provide funding for innovative entrepreneurs. It is conceivable that in some cases they played this role, but the odds are overwhelmingly on the other side. As a general rule, any new product and any thoroughgoing innovation in business organization is a threat for banks, because they are already more or less heavily invested in established companies, which produce the old products and use the old forms of organization.

8.3.3 Recapitulation of the Argument for a Positive Link between Price Inflation and Future Unemployment

We have argued that the unintended consequences of monetary expansion on the distribution of income and wealth that trigger a rising gap between rich and poor might increase public support for more restrictive regulations on labor markets as well as higher taxes and increased welfare spending. These political measures render labor markets less flexible and destroy incentives to invest and hire people and thereby tend to increase unemployment. The bracket creep under a system of progressive taxation reinforces this tendency.

Moreover, monetary expansion may cause cyclical fluctuations that temporarily lower but ultimately increase unemployment as the boom turns into a bust. One may argue that increased unemployment during economic crises is also only a temporary phenomenon that will cease in the long run. Yet again, political measures to counter economic downturns and protect workers and firms are motivated in the course of the business cycle. Regardless of several other effects that these measures may have, they tend to increase the *natural* or *institutional* rate of unemployment in the long run, that is, as long as they remain in place.

In both cases, the unintended consequences of monetary expansion are considered to be inevitable on purely theoretical grounds. They form the incentive structure under which political decisions are made. Yet, at the point at which we rely on political reactions in order to establish ties between price inflation and unemployment, we are leaving the realm of economic theory and enter into historical interpretations of what the data show.

At the core of our argument is the idea of interventionist spirals as developed in von Mises (1977). The initial intervention is an expansion of money and credit through central bank policies. Whatever the purposes of the expansion, it leads to unintended consequences that demand further interventions. It is the initial intervention that causes price inflation, and further interventions that subsequently lead to increased long-run unemployment.

The danger of interventionist spirals has also been emphasized by one of von Mises's students. In an interview with Armen Alchian, Friedrich A. von Hayek has expressed serious fears about the potential consequences of inflation and the so-called fight against it. He said: "I think the great danger is that the so-called fight against inflation will lead to more and more control and ultimately to the complete destruction of the market" (von Hayek, 1978b).

Time will show how true or false Hayek's prediction was. But it is clear that if the process of interventionism truly ends up in a more or less completely planned economy, not only markets

but also unemployment will cease to exist. This might indeed be eagerly anticipated by some people. However, certain historically contingent combinations of interventionist programs that may lie on the way towards the planned economy result in increased unemployment as opposed to what we would expect to observe in a market-based order.

Chapter 9

Cost Accounting for a System of Central Banking under Fiat Money

Assessing the economy-wide consequences of central bank monetary policy is a difficult task indeed. The overall costs and benefits for society are never accounted for without a fair amount of speculation and uncertainty. There is, however, a much narrower notion of costs that can be evaluated more easily and, in principle, with perfect accuracy. These are what we might call the business-management costs of central banking. They are the subject-matter of this chapter.

The term business-management costs is in a way misleading, since it suggests that central banks are ordinary businesses, which they are not, as we have argued above (see ch. 7, p. 264). Yet, the term captures very well what we want to focus on in the following, namely, the items of expenditure that are included in ordinary business accounting. Hence, this chapter contains an exercise in econometrics in Pawel Ciompa's best sense of the word. It contains an exercise in cost-accounting for a modern system of central banks under fiat money.

As mentioned earlier in the thesis, an important argument for central bank controlled fiat money is related specifically to the costs of production of money. It can be found already in the writings of classical economists. Milton Friedman is one prominent modern economist, who has reinvigorated this argument, according to which fiat money would free up resources otherwise needed in the production of commodity money, or more specifically in gold mining and refinement (see ch. 3, pp. 109f.). In other words, according to this argument, fiat money is simply cheaper to produce than commodity money and the resources saved by implementing a fiat standard can be employed elsewhere to the benefit of society.¹

¹Interestingly, von Mises (1953, pp. 298-299) formulated a similar argument for fractional reserve banking

While, as a matter of principle, it is indisputable that fiat money could be much more cheaply produced than gold or any other commodity money, it is by no means a necessity. Sure enough, a Friedmanite k -percent rule with respect to some monetary aggregate could be implemented by a powerful computer network, a printing press, and some, relatively small, supervisory board. The costs of production would reduce to an annual electricity bill, expenses for ink, cotton and some other materials needed for banknote production, computer maintenance expenses, salaries for the board members, as well as some other minor expenses, such as maybe an annual board meeting in a pleasant and stimulating environment like, say, the Cook Islands, French Polynesia, or near the Great Blue Hole of Belize. The overall costs of production of money could be truly negligible.

Modern monetary policy is, however, conducted differently. It does not follow any simple and strict rule, but implements, especially in recent years, unconventional policy interventions and discretionary adjustments. Their effects are studied empirically and theoretically by numerous expert groups. Modern monetary policy is based on relatively costly data gathering and processing and requires very close and careful supervision of various macroeconomic developments both on the domestic and the international level. The governing council meets not only once a year, but around twice a month in the case of the European Central Bank, although of course in Frankfurt, Germany, and not in some far removed holiday resort.

The conduct of modern monetary policy is itself an institutional process that changes and evolves over time. As for any government monopoly that does not operate under the constraints of the profit and loss system (von Mises, 2008), we would expect there to be a tendency for central banks to become less efficient, more wasteful and costly in terms of business-management expenses. We would expect the overall costs of maintaining the institution and its operations to grow. In this chapter, we follow up this intuition. We thus focus specifically on this narrower notion of costs for the European Central Bank and the national central banks within the euro area over the past 18 years.

In section 9.1, we look at the ECB's annual financial statements and the development of selected items of expenditure over time. The operating expenses of the national central banks are studied as well. In the subsequent section an overview of current gold production costs is

under a gold standard. According to him, fractional reserve banking historically prevented a stronger increase in the exchange value of money in the course of technological progress and the extension of the monetary economy. As a result, fewer capital and labor was directed towards the mining of gold for monetary purposes and was instead available for other productive enterprises. As in footnote 47 on page 265, we emphasize again that von Mises did not repeat this argument in his *magnum opus* (von Mises, 1998), which indicates that his opinion on the topic has changed. Friedman, too, became more critical about fiat money later in his career (Friedman and Schwartz, 1986).

given in order to put the expenses of European monetary institutions into perspective. Section 9.3 concludes.

9.1 Operating Expenses of the Central Bank System of the Eurozone

The European project of a common currency provides an interesting case study for our purposes. The euro had been introduced as an accounting currency on the 1st of January 1999, three years before it was issued as a physical currency. The annual financial statements of the ECB for the end of 1999 are a suitable starting point for our analysis. They are covered in subsection 9.1.1. They provide an overview of what items of expenditure initially existed within the new institution.² Other items were added later on as the common currency area developed and expanded. We will follow some of them over time in subsection 9.1.2 in order to get an impression of how the overall costs have evolved. We then proceed with the national central banks to evaluate more precisely the operating expenses of the central bank system of the euro area as a whole. This will be done in subsection 9.1.3.

9.1.1 The 1999 Annual Financial Statements of the ECB

At the end of 1999, the ECB employed 732 staff, of which 55 held managerial positions. On average, over the whole year, 648 people were employed by the ECB as compared to only 478 in the previous year during which the institution was founded. This increase is not surprising given the early stage of the ECB's development. Moreover, the year 1999 marked a major step in the implementation of the common currency. In total, 242 new employees were hired that year and only 44 employees left the service (ECB, 2000, pp. 156-157).

The overall staff costs in the profit and loss account of the annual report amount to € 61.0 million. They include € 52.3 million in salaries and allowances, making an average of about € 80,700 annually per employee,³ as well as total pension costs of € 8.1 million. The latter

²The annual report 1999 is also the first one that covers an entire calendar year. The report for 1998, the year the ECB was founded, covers only seven months.

³Today, the lowest salary band, namely, that of Facility Management Operators, starts at a minimum monthly basic net salary of € 2,442.62 according to the ECB's website as of September 18, 2017. The highest band of Director General starts at a minimum of € 11,349.85 per month. The historical development of these salary bands are not readily available. Neither does the annual report of 1999 contain information about the salaries of the highest ranked servants at the ECB, such as president Willem F. Duisenberg, or other Executive Board members, like vice-president Christian Noyer, Otmar Issing and Sirkka Hämäläinen. The highest salary currently paid at the ECB

include a provision of pensions to members of the Executive Board of € 1.8 million. This decision-making body of the ECB consisted, as it does today, of six members, making an average pension of € 300,000 to each board member for that year.

Administrative expenses, which “cover all other current expenses relating to rental of premises, maintenance of premises, goods and equipment of a non-capital nature, professional fees and other services and supplies, together with staff-related expenses including recruitment, relocation, installation, training and resettlement” (ECB, 2000, p. 157) added up to € 60.7 million.

Table 9.1: Operating expenses of the European Central Bank in 1999 and for seven months in 1998 in €

	1999	1998
Staff costs	61,022,091	29,744,540
Administrative costs	60,748,855	30,229,686
Depreciation of tangible and intangible fixed assets	10,468,901	8,076,017
Total operating expenses	132,239,847	68,050,243

Sources of data: profit and loss account of the Annual Report 1999 (ECB, 2000, p. 148)

The third item shown in Table 9.1 is the depreciation of tangible and intangible fixed assets. It amounts to € 10,5 million. This item is included under operating expenses in our analysis, since it serves as a common means in business accounting to spread the costs of the relevant assets over the time span during which they are used by the institution. It is, in a sense, an indicator of capital consumption, and thus relevant in assessing the overall expenses.

Summing up the three items yields total expenditures of around € 132,2 million for the year 1999. During the seven months of the previous year, since its foundation on the 1st of June, 1998, the ECB’s operating expenses were € 68.1 million. Adjusted to a full calendar year, total operating expenses thus increased from 1998 to 1999 by about 13.4%. Staff and administrative expenses increased by 19.7% and 17.2%, respectively, while the depreciation of assets diminished by 24.4%.

is of course the one of president Mario Draghi. It amounts to € 389,760 per year. Vice-president Vitor Constâncio’s annual salary is € 334,080. The other four Executive Board members of the ECB currently earn salaries of € 277,896, leading to a combined annual salary payment of € 1,835,424 for the entire Executive Board. The total salary for the larger Supervisory Board, of which the Executive Board is a part, is € 2,466,678 (ECB, 2017, p. A56).

9.1.2 Selected Items of Expenditure over Time

It is not surprising that the ECB in its early years expanded. The introduction of the common currency, including its circulation in cash and its declaration as legal tender in the twelve initial member states,⁴ was completed only in 2002. From that year onward, a fourth item of expenditure was added to the annual accounts, namely, the costs for banknote and coin production services. In the first year the new notes and coins were issued, this item directly reached its maximum for obvious reasons.⁵ It amounted to around €118.4 million. In the next year, it dropped to € 2.1 million. These expenditures do not, however, cover the full costs involved in euro banknote and coin production and distribution. As we will see below, the remainder of the costs is accounted for in the financial statements of the national central banks of the member states.

Table 9.2: Operating expenses of the European Central Bank in 2003 and 2002 in €

	2003	2002
Staff costs	129,886,988	120,003,344
Administrative costs	153,549,282	133,966,576
Depreciation of tangible and intangible fixed assets	30,410,140	17,738,206
Banknote production services	2,096,766	118,358,022
Total operating expenses	315,943,176	390,066,148

Sources of data: profit and loss account of the Annual Report 2003 (ECB, 2004, p. 186)

Table 9.2 summarizes all four items of expenditure for the years 2002 and 2003. Not only was one item added, but the three original positions grew substantially. Staff costs, administrative costs and depreciation have grown in five years, from 1999 to 2004, by 112.9%, 152.8% and 190.5%, respectively. Total expenses have grown by 138.9%.

⁴These are Belgium, France, Germany, Ireland, Italy, Luxembourg, Netherlands, Greece, Spain, Portugal, Austria and Finland. Today the common currency area consists of 19 member states. Slovenia entered in 2007. Cyprus and Malta followed in 2008 and Slovakia in 2009. Estonia adopted the euro in 2011, and finally, Latvia and Lithuania followed in 2014 and 2015, respectively. The expansion of the eurozone is of course another aspect that, to a limited extent, justifies increasing operating expenses over time.

⁵The first two months of 2002 were the cash changeover period. The euro became the only legal tender in the eurozone on the 1st of March, 2002. In mid-January, the number of euro banknotes and coins in circulation peaked for that year at around 8.1 billion and 38.6 billion, respectively (ECB, 2003, pp. 130-131). While the overall number of banknotes and coins in circulation slightly decreased thereafter, the face value of the cash money in circulation steadily grew throughout the changeover period, as the number of banknotes with larger denominations increased. The ECB also launched a campaign to inform the public about the new currency. Most of these costs were accounted for already under administrative expenses in the year 2001, which led to a decrease in administrative expenses in 2002. The annual report of 2002 reads as follows: “The net decrease in administrative expenditure compared with 2001 is primarily due to the fact that in that year additional consultancy fees were incurred in connection with the Euro 2002 Information Campaign” (ECB, 2003, p. 211). This does not change the fact that total expenses have increased over the years.

The number of full time employees increased over the same period from 732 to 1,213, that is, by 65.7%. The number of employees in managerial positions increased by 52.7% from 55 to 84. Hence, staff costs have disproportionately increased compared to the number of employees. The average staff costs per employee have increased from about € 83,000 to slightly more than € 107,000. It is interesting to note that until 2003, all 1,213 employees of the ECB had been hired on permanent contracts (Luttmer, 2015). Since 2004, this employment policy has changed, and a fraction of the new employees has been hired on limited contracts.

Figure 9.1: Operating expenses of the ECB between 1999 and 2016



Source of data: annual reports of the ECB between 1999 and 2016

The upper right panel of Figure 9.1 shows all the above mentioned items of expenditure of the ECB from 1999 to 2016. We can clearly see the peak in expenses for note production services in 2002. After that year, this item showed an increasing trend but always remained below € 9 million. It mostly reflects the costs of cross-border banknote transportation to various national central banks to meet unexpected fluctuations in demand (ECB, 2004, p. 198). Beginning in 2013, the new Europe Series of banknotes with enhanced security features has been introduced,

which partly accounts for the increase in costs. After the new € 5, € 10, and € 20 notes, the new € 50 note was the latest denomination to enter into circulation in April 2017 (ECB, 2017, p. 80).

Depreciation of assets initially increased between 1999 and 2004, then decreased until 2014, before it sharply increased in 2015 from € 15.3 million to € 64.0 million. That year the new headquarters of the ECB in the East end of Frankfurt were inaugurated after more than four years of construction. Total costs for the new building were more than € 1.3 billion. The additional depreciation on that asset, which is owned by the ECB, explains the sharp increase in the annual accounts for this item of expenditure. Before, the ECB had rented office space in three different locations in the city center of Frankfurt, including the Eurotower and the Japan Center. The ECB continues to rent the Eurotower, which is now home of the Single Supervisory Mechanism (SSM), which supervises and monitors the stability of commercial banks within the eurozone since 2014.

Table 9.3: Operating expenses of the European Central Bank in 2016 and 2015 in €

	2016	2015
Staff costs	466,540,231	440,844,142
Administrative costs	414,207,622	351,014,617
Depreciation of tangible and intangible fixed assets	64,769,605	64,017,361
Banknote production services	8,315,671	8,130,019
Total operating expenses	953,653,129	864,006,139

Sources of data: profit and loss account of the Annual Report 20016 (ECB, 2016, p. 186)

Staff and administrative costs have, some minor off-trend fluctuations notwithstanding, continuously increased over the 18 years of the ECB's existence. In 2016, staff and administrative costs amounted to € 466.5 million and € 414.2 million, respectively. Hence, compared to 1999, they have increased by factors of 7.6 and 6.8, or by an average annual rate of 12.7% and 12.0%, respectively.

Total operating expenses as plotted in the upper right panel of Figure 9.1 and reported in Table 9.3 have risen to € 953.7 million in the last year. This corresponds to an average annual growth rate of 12.3%, or € 48.3 million per year.

The sharp increase that can be observed for staff, administrative, and hence overall costs in 2014 is mainly due to the implementation of the SSM (ECB, 2015, p. 153). In the following year, the increase in staff costs remained very high, namely 46.4%. The explanation provided in the annual report, however, is rather unsatisfactory. We only read: "Staff costs increased in 2015, mainly owing to the higher average number of staff employed by the ECB, as well as the higher

net expense in relation to post-employment benefits and other long-term benefits” (ECB, 2016, p. A49).

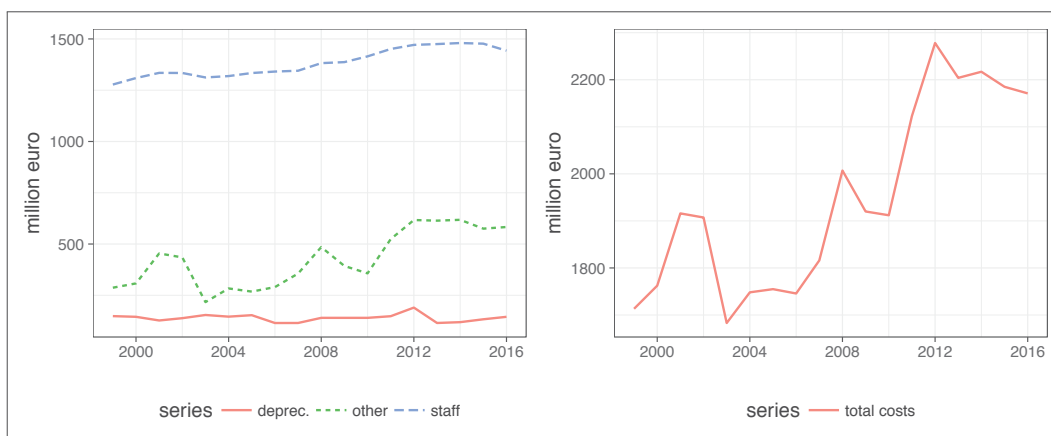
Overall staff as well as staff in managerial positions have indeed increased over the entire period under consideration as shown in the bottom panels of Figure 9.1. At the end of 2016, the ECB employed the equivalent of 3,171 full-time employees, among which 320 held managerial positions. In only three years, from 2013 to 2016, the ECB on net hired more than 1,381 new full-time employees. The number of employees in managerial positions increased by 151.

While the overall annual operating expenses of the ECB alone have shown an alarming trend over the past 18 years and amount by now to almost a billion euro, they are by no means the only expenses relevant for our purposes. National central banks of member states of the common currency area have not been abolished and replaced by the ECB. They exist side by side at substantial operating costs of their own.

9.1.3 Adding the National Central Banks

The three biggest national central banks within the euro area are those of France, Germany and Italy. Their operating expenses are plotted in Figures 9.2, 9.3 and 9.4. Each one of these institutions spends substantially more money to finance its activities than the ECB.

Figure 9.2: Operating expenses of the Banque de France between 1999 and 2016



Source of data: annual reports of the Banque de France between 1999 and 2016

The Banque de France reports operating expenses in three categories: depreciation on assets, staff costs and other expenses. The latter includes, for example, administrative costs and costs for banknote production services. The left panel of Figure 9.2 shows that depreciation remained more or less constant between 1999 and 2016 at around € 140 million. However, staff costs and

other expenses show positive trends. Although the ECB was created in 1998 and has taken over important monetary policy responsibilities within the common currency area, the national central bank of France did not shrink in terms of operating expenses. It has even expanded.

Staff costs have mildly increased by € 165.3 million, or by 12.9% over 17 years. That corresponds to a rather small average annual growth rate of 0.7%, but one has to take into account that staff costs had been quite substantial from the beginning. In 1999, they amounted to € 1,277.7 million. Hence, in 2016 staff costs of the Banque de France were more than three times as high as those of the ECB.

We observe a similar development for all other expenses. The annual accounts of the Banque de France for 2016 report other expenses of about € 583 million, which is more than twice as much as in 1999. They have grown at an average annual growth rate of 4.3% and are today 38% larger than administrative costs and banknote production services of the ECB.

Overall operating expenses, plotted in the right panel of Figure 9.2, amounted to about € 2,171 million in 2016. They grew at an average annual growth rate of 1.4%. While this is much slower, in relative terms, than the expansion of the ECB over the same period, overall expenses of the French central bank are still more than 2.3 times as high as those of the ECB.

Figure 9.3: Operating expenses of the German Bundesbank between 1999 and 2016



Source of data: annual reports of the German Bundesbank between 1999 and 2016

The German Bundesbank is another big player in the central bank system of the eurozone. The most obvious difference to the Banque de France is that its operating expenses have not grown since the foundation of the ECB. There is no clear trend observable. Expenses have fluctuated between the trough of € 1,307 million in 2011 and the peak of € 1,935 million in 2001, one year before the euro was introduced in cash. As for the Banque de France, staff costs

are the most important item of expenditure for the Bundesbank. They fluctuated around € 900 million for most of the period and reached their maximum level of € 1,123 million in 2016.

There is no clear trend for any of the other items of expenditure. Administrative costs slightly increased and amounted to € 396 million in 2016. The other items fluctuated around and below € 200 million. Total costs, as shown in the right panel of Figure 9.3, peaked the year before the euro became the exclusive legal tender of the currency union and reached a similar level of € 1,811 million last year.

Figure 9.4: Operating expenses of the Banca d'Italia between 1999 and 2016



Source of data: annual reports of the Bank of Italy between 1999 and 2016

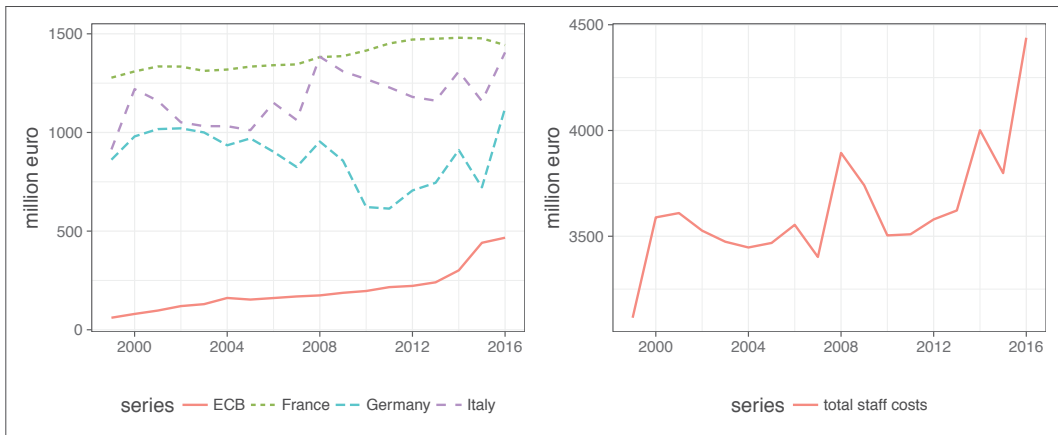
For the Italian central bank there is a decreasing trend in total costs as shown in Figure 9.4. This, however, is entirely due to an item of expenditure in their annual accounts labeled “other costs”. In 2001, it amounted to more than € 1,340 million.⁶ It decreased to around € 51 million in 2016. Administrative and staff costs, however, have continuously increased, together by € 619.5 million from 1999 to 2016.

Figures 9.5 and 9.6 summarize the development of staff costs and total costs of the big four central banks of the euro area from the foundation of the ECB until today. Overall staff costs have increased by € 1,324 million in 18 years and reached a level of € 4,458 million in 2016. The strongest expansion occurred in the newly founded ECB.

Total operating expenses of all four institutions increased from € 5,506 million in 1999 to € 6,978 million in 2016. They peaked at € 7,211 million in 2001, the year prior to the introduction

⁶According to the annual report for 2001, it subsumes the following expenses: losses on investments of reserves and provisions (144 million), other allocations to provisions (751 million), prior-year expense (1 million), appropriation of investment income to reserves (393 million), other taxes and duties (45 million), sundry (10 million) (BDI, 2002, p. 302).

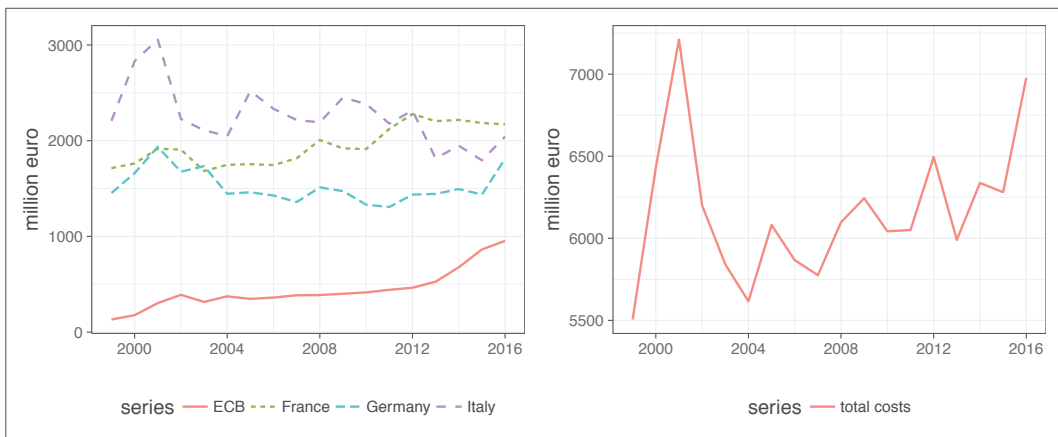
Figure 9.5: Staff costs of the ECB, Banque de France, German Bundesbank, and the Banca d'Italia between 1999 and 2016 (left panel); the sum of all four series (right panel)



Source of data: annual reports of the respective central banks between 1999 and 2016

of the euro as a physical currency. The strongest expansion occurred again, in both relative and absolute terms, in the youngest of the four institutions.

Figure 9.6: Total operating expenses of the ECB, Banque de France, German Bundesbank, and the Banca d'Italia between 1999 and 2016 (left panel) and the sum of all four series (right panel)



Source of data: annual reports of the respective central banks between 1999 and 2016

Hence, considering only the four big players within the central bank system of the euro area, we obtain annual operating expenses of almost € 7 billion. However, the euro area initially consisted of 12 member states. There are thus nine other national central banks that took part in the system from the beginning. They are considerably smaller. Their separate operating expenses are plotted in Figure 9.7 along with the sum over all smaller institutions. The upper panels include, and the bottom panels exclude, the central bank of Greece, which represents a special case within this group.

In 1999, the total operating expenses of all nine smaller central banks amounted to € 1,347 million, which is substantially lower than the operating expenses of any of the three larger national central banks for the same year. Over the past 18 years, operating expenses of the smaller banks have increased to € 2,961 million, which is substantially higher than the operating expenses of any of the big three today. Hence, the smaller central banks within the system have expanded much more than the larger ones after the introduction of the common currency.

Interestingly, there was not nearly as strong a peak in operating expenses for the smaller central banks in the year before the euro became the exclusive legal tender in 2002. This suggests that a disproportionate share of the costs of the introduction of the common currency was borne by the larger central banks of the union.

However, in 2012, there was a sharp increase for the smaller central banks. Overall operating expenses were € 4,568 million, that is, 54.2% higher than even in 2016. This, as we can see from the upper panels of Figure 9.7, is entirely due to a massive surge in operating expenses of the Greek central bank. Its expenses were € 1,498 million in 2011 and € 2,677 million in 2012. They fell again below € 800 million the next year. Hence, in 2012, the central bank of Greece had higher operating expenses than any of the central banks of France, Germany or Italy.

A look at the profit and loss accounts in the annual reports of the Greek central bank for the respective years reveals that the sharp increase reflects a major upward drift in “provisions”. This item of expenditure increased by more than a factor of 15, from € 148 million in 2007 to €2,342 million in 2012. In the annual report of 2012, we can find a generic explanation, according to which the provisions increased in order to “cover operational risks and other liabilities of the Bank” (BoG, 2013, p. A51). Digging a little bit deeper into the 249 pages of the document, we find that there are four different categories of provisions, of which “Provision covering the Bank’s obligation to provide social insurance to its staff”; “Provision against financial risks” and “Provision against general risks under Article 71 of the Statute” are the most important ones.⁷

The latter category is further explained as a provision intended to cover “any other risks and liabilities potentially arising from the Bank’s business as the country’s central bank and in the context of international agreements” as well as “any additional liabilities of the Bank arising from the provision of social insurance to its staff” (BoG, 2013, p. A34). So it is, on the one hand, again about the social insurance of the staff, just like the first category mentioned. On the other hand, it covers additional risks that would fit under the umbrella of the second and very generic

⁷The remaining category covers “Special provisions against operational risk, unexpected losses and doubtful claims” (BoG, 2013, p. A32).

Figure 9.7: Total operating expenses of the national central banks of Spain, Portugal, Greece, Belgium, Luxembourg, the Netherlands, Austria, Finland and Ireland between 1999 and 2016 (left panels) and the sum of all series including Greece (upper right) and without Greece (bottom right)



Source of data: annual reports of the respective central banks between 1999 and 2016

category of “financial risks”.⁸ What makes these provisions particularly interesting, however, is the fact that during the financial year 2012, a third paragraph was added to Article 71 of the Statute of the Bank of Greece. In this paragraph, we read:

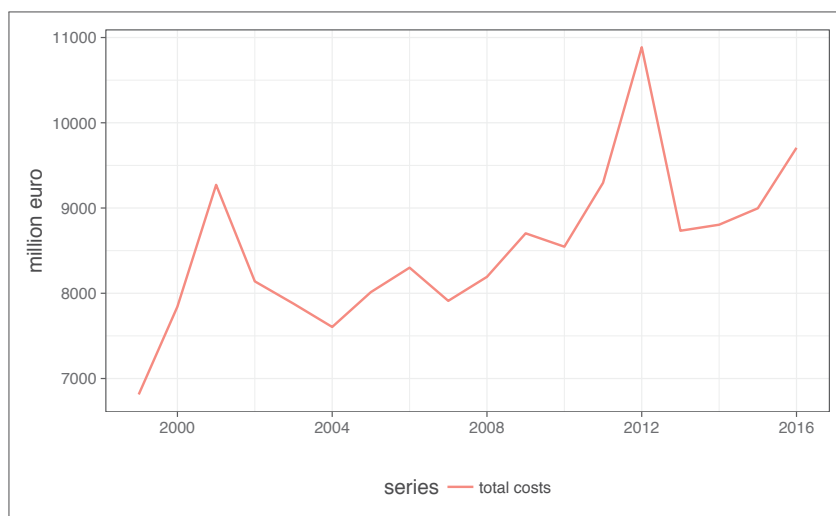
Exceptionally for financial years 2012-2020, and following a decision of the General Council taken in implementation of international agreements, the income from Greek government bonds held in the investment portfolio of the Bank of Greece as at 31 December 2011, as well as from Greek government bonds held for monetary policy purposes in the context of the Securities Markets Programme (SMP) of the Eurosystem, may be transferred to the Greek State. (BoG, 2016, pp. 56-57)

⁸The second category of “financial risks” is further described in the annual report as “including risks from the Bank of Greece’s investment activity and risks in the context of Eurosystem single monetary policy” (BoG, 2013, p. A32), the latter being an instance of international agreements.

In the final analysis, this addition to Article 71 allows for partial government debt cancellation through the Bank of Greece and the Eurosystem. It is obviously a measure undertaken to alleviate the debt burden of the Greek government in the course of the sovereign debt crisis. Whether this qualifies as direct government finance through the central bank system is an interesting legal question that goes far beyond the purpose of this chapter and cannot be discussed here. Yet, we see that at least part of the increased operating expenses of the Bank of Greece can be traced back to its government's indebtedness and increased systemic risk within the euro area.⁹ Another part seems to be disguised staff costs.

Recently, provisions of the Greek central bank have decreased again, but it remains the largest among the small central banks of the euro zone in terms of operating expenses. It is followed by the Spanish, Austrian and Belgian central banks, all of which had operating expenses above € 400 million in 2016. The smallest central bank is the one of Luxembourg with annual operating expenses of about € 81 million in the last year.¹⁰

Figure 9.8: Total operating expenses of the ECB and the national central banks of the 12 founding members of the Eurosystem from 1999 and 2016



Source of data: annual reports of the respective central banks between 1999 and 2016

We can now add the expenses of all 13 founding members of the central bank system of the euro zone, including the ECB. Figure 9.8 shows the result. From 1999 onward, overall annual operating expenses have increased by € 3,088 million and amounted to € 9,940 million in 2016.

⁹The situation, in which the Eurosystem finds itself, is a drastic illustration of the general relationship between debt and systemic risk as described in chapter 7 of this thesis. The moral hazard encapsulated in the system, as described in Bagus (2012), is an essential cause of the problem.

¹⁰The exact order and operating expenses in million € of the nine smaller national central banks in 2016 was as follows: Greece (636), Spain (492), Austria (425), Belgium (413), Netherlands (379), Ireland (233), Portugal (201), Finland (100) and Luxembourg (81). The sum over all nine institutions is € 2,326 million.

There are two peaks that stand out, the first in 2001 (€ 9,341 million) and the second in 2012 (€ 11,064 million). They reflect the increased expenses right before the introduction of the euro as a physical currency and the massive surge in provisions payed by the Greek central bank in recent years.

There are seven other national central banks that have entered the Eurosystem since its foundation. Some of them are almost negligible in size, such as the central banks of Estonia or Malta. Their annual operating expenses in 2016 were € 17.7 million and € 18.1 million, respectively. Others, however, such as the central bank of Cyprus, an insular state which has a bit less than three times as many inhabitants as Malta, had operating expenses of € 435 million.¹¹

All in all, there are 20 central banks, including the ECB, which form the Eurosystem today. Their total annual operating expenses amounted to € 10.6 billion in 2016. This number, in and of itself, does not tell us much, but it suggests that the savings obtained from a fiat money standard in the euro area are not nearly as high as they theoretically could be. We should therefore try to put this result into perspective.

9.2 A Note on the Gold Mining Industry

The costs of gold mining are the relevant benchmark in order to assess the above result. The historical predecessor of the modern fiat standard was the gold standard. The first thing to note is that global gold production has not actually diminished after the transition from gold to unbacked fiat money, a process that was completed with the end of the Bretton Woods system. Moreover, a substantial proportion of the demand for gold nowadays stems from central banks themselves.¹²

In fact, since 1971 world mine production of gold has more than doubled as shown in Figure 9.9. It initially fell, but started to increase again in the 1980s. In 2015, it reached an all-time high of 3,100 metric tons per year. This is all the more astonishing, given the general tendency for the production costs of gold to increase, irrespective of technological developments. The more easily accessible gold reserves tend to be mined first, before companies proceed to the more arduous

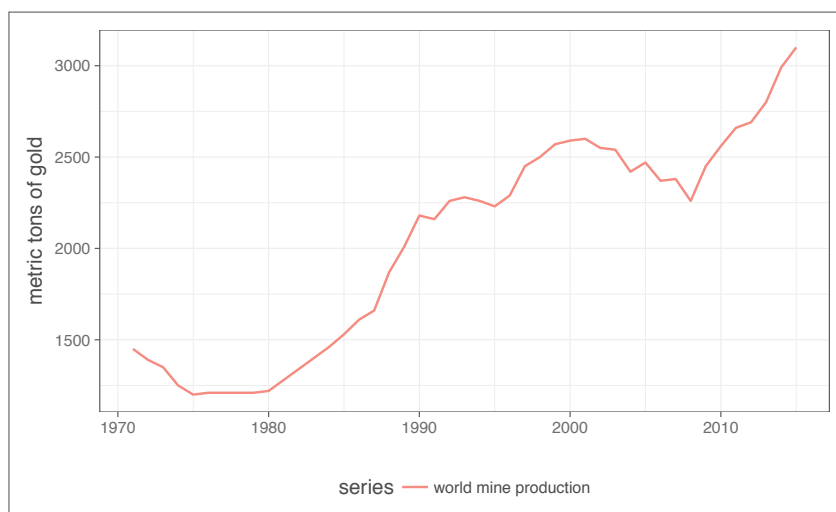
¹¹The exact order and operating expenses in million € of the remaining seven national central banks in 2016 was as follows: Cyprus (434.7), Slovakia (82.2), Latvia (39.4), Lithuania (36.3), Slovenia (33.1), Malta (18.1), Estonia (17.7). The sum over all seven institutions is € 661.6 million.

¹²Take, for example, the German Bundesbank. In the balance sheet for 2016, we find gold reserves of about €119 billion under the listed assets, which corresponds to roughly 3,380 metric tons of gold. In recent years, the Bundesbank has made an effort to bring part of its gold reserves, that were stored abroad for historical reasons, most notably in New York, Paris and London, back to Germany. The Bundesbank intends to transfer about 700 tons of gold until 2020 back to Frankfurt (FAZ, 2016). It thereby follows a citizens' initiative called *Holt unser Gold heim* [bring our gold home] (Boehringer, 2015).

methods of production. Part of the increase in global gold production is undoubtedly explained by technological progress. Another part may be explained by gold's perceived role as an anchor against inflation.

Even though gold is not backing the currency directly anymore, it is still demanded as a store of value. It is possible that the fear of potential inflation and actually observed price inflation in the aftermath of the Nixon Shock have led to an increase in private demand for precious metals. Given the ongoing demand for gold by central banks, this might have induced the increase in mine production. It is, however, entirely conceivable that in a counterfactual scenario without the abolition of the gold standard, there would have been an even larger increase in gold production. The relevant comparison is, as so often, a counterfactual one.

Figure 9.9: World mine production of gold per year from 1971 to 2015



Source of data: United States Geological Survey

In the following we will first look at the expenses of leading gold companies in order to get an impression of what the range of costs is in that industry. Next, we will look at a suitable fraction of the global gold production and give an estimate of its costs of production.

9.2.1 Production Costs of Leading Gold Mining Companies

The three biggest publicly traded and non state-owned gold mining companies in the world are Barrick Gold in Canada, Newmont Mining in the US and AngloGold Ashanti in South Africa.¹³

¹³A top ten list was compiled by Basov (2017). The next companies in descending order with their respective market shares are Goldcorp (2.87%), Kinross Gold (2.79%), Newcrest Mining (2.46%), Gold Fields (2.15%), Polyus Gold (1.97%), Agnico Eagle (1.66%), Sibanye Gold (1.51%). Together the top ten publicly traded, non state owned gold mining firms had a market share of 29.46%.

Together these firms covered 14.05% of the annual gold production in 2016. According to the United States Geological Survey database, annual gold production for 2016 was estimated to be the same as in the previous year, that is, 3,100 metric tons. This corresponds to about 100 million troy ounces, which is the value we assume for world output in order to calculate market shares of the three firms summarized in Table 9.4.¹⁴

Table 9.4: Gold production and all-in sustaining costs (AISC) of Barrick Gold, Newmont Mining and AngloGold Ashanti in 2016

	Barrick Gold	Newmont Mining	AngloGold Ashanti
Production in oz.	5,517,000	4,898,000	3,628,000
AISC per oz. in US \$	730	912	986
Market share	5.52%	4.90%	3.63%

Sources of data: annual reports of the respective firms

As a measure of production costs, the table includes all-in sustaining costs (AISC) per troy ounce of gold, which is a metric developed by the World Gold Council. It is usually higher than alternative measures such as the cash costs.¹⁵ If we weight the reported AISCs of the three firms by their respective market share, we obtain an average AISC of \$ 859.61 per ounce.

The three firms produced a total of 14,043,000 ounces, or 436.8 metric tons. The total all-in sustaining costs for this volume were thus \$ 12,071,562,211 or about \$ 12.1 billion. If we take the average exchange rate between US dollar and euro for the year 2016, this sum corresponds to € 10.97 billion.¹⁶

This measure is of a very similar magnitude as the overall operating expenses calculated for the central banks of the eurozone. However, why should we take the costs of the three leading gold mining companies as the relevant benchmark? Why not the leading two, four, six or maybe

¹⁴To be precise, 3,100 metric tons correspond to 96,420,778 troy ounces. We round this value to 100 million ounces, which is the same value used in Basov (2017). This rounding does not make operating expenses of central banks appear relatively more important in our later comparison. If anything, the opposite would be the case.

¹⁵AISC is a non-GAAP metric. According to a press release of the World Gold Council (Murray, 2013) it incorporates the following elements from the income statement of the firm: On-Site Mining Costs (on a sales basis); On-Site General and Administrative costs; Royalties and Production Taxes; Realised Gains/Losses on Hedges due to operating costs; Community Costs related to current operations; Permitting Costs related to current operations; 3rd party smelting, refining and transport costs; Non-Cash Remuneration (Site-Based); Stock-piles / product inventory write down; Operational Stripping Costs; By-Product Credits; Corporate General and Administrative costs (including share-based remuneration); Reclamation and remediation - accretion and amortisation (operating sites); and Exploration and study costs (sustaining). Moreover, it incorporates three elements from the cash flow: Capital exploration (sustaining); Capitalised stripping and underground mine development (sustaining); and Capital expenditure (sustaining).

¹⁶The average exchange rate for 2016 was at 0.9089 US dollar per euro. This calculation is based on monthly averages provided in the database of www.x-rates.com. We have taken the monthly averages weighted by the number of days in the month to calculate the annual average.

all mining companies? Indeed, taking the average AISC of the leading companies as a proxy for the average AISC of world mine production of gold might lead to a bias. It is possible that the leading companies operate at lower costs than the average company, which might be the reason why they hold the largest market shares. In fact, among the three companies in Table 9.4, Barrick Gold does not only have the largest market share, but also the lowest AISC per ounce, and AngloGold Ashanti has the highest AISC per ounce and the smallest market share. Let us therefore try to look at the overall costs of the world production of gold, before we think about what fraction of it could be a suitable benchmark.

9.2.2 The Total Costs of Gold Mining

As far as we can tell, there is no data on the overall average AISC of gold mining readily available. As a start we can look at some more of the leading gold mining companies in order to see whether the trend of increasing AISC observed for the first three remains stable.

Table 9.5: All-in sustaining costs (AISC) of the top ten non state-owned, publicly traded gold mining firms

	AISC in US \$ per oz.
Barrick Gold	730
Newmont Mining	912
AngloGold Ashanti	986
Goldcorp	856
Kinross Gold	984
Newcrest Mining	763
Gold Fields	980
Polyus Gold	572
Agnico Eagle	824
Sibanye Gold	954

Sources of data: annual reports of the respective firms

If we weight the individual AISCs of the top ten firms by their respective market share, we obtain an average AISC of \$ 855.36 per ounce. The average for the top ten is thus even slightly lower than the average for the top three. The trend does not continue within the top ten mining firms, although it might, of course, play out again in an even larger data set. Output of the top ten covered 29.46% of global gold production in 2016. Hence, these firms produced some 913 metric tons of gold with a total AISC of about \$ 25.2 billion.

Assuming that the average AISC per ounce of gold for other mining companies was substantially higher, say \$ 1000, which is far above any value reported in the annual reports we have consulted, we can calculate a weighted average AISC for world production. It is \$ 957.39 per ounce. This would lead us to an estimate of total AISC for world production of gold in 2016 of \$ 95.7 billion.

If we assume, even more conservatively, that average AISC per ounce for world production in 2016 have been \$ 1000, then our estimation of overall AISC amounts exactly to \$ 100 billion. This leaves us with two more or less cautious estimates of the overall AISC of gold mining, which form an interval of \$ 95.7 billion to \$100 billion.¹⁷

9.3 In Comparison

The relevant question is what fraction of the overall costs of gold mining should be taken as a suitable benchmark for comparison with the calculated operating expenses of the central bank system of the eurozone in the financial year 2016? It seems intuitively plausible to take the fraction that corresponds to the eurozone's contribution to world GDP. According to data provided by the ECB, this contribution is 11.8%.¹⁸

Depending on which of the above estimates we take, 11.8% of the overall costs of gold mining amount to \$ 11.3 billion or \$ 11.8 billion. Taking the average exchange rate between euros and US dollars for 2016 as calculated above, these values translate to € 10.3 billion and € 10.7 billion, respectively. We like to consider it, an astonishing coincidence that the interval formed by these two values contains the calculated operating expenses of the central bank system of the euro area for that same year. It is € 10.6 billion.

While this finding substantiates to some extent our claim that costs savings from fiat money are not nearly as high as they could theoretically be, an unfavorable reader might accuse us of tweaking the numbers to fit the results that were intended in the first place, or of comparing apples to oranges. But we would hold that we did neither.

As far as an accusation of cherry picking statistics is concerned, we can only repeat that the above statistics come from official and publicly available sources. We rely in that regard on the

¹⁷Notice again, that for all the above estimates the annual production of gold in 2016 is set equal to 100 million ounces. The estimate in billion US dollars is thus simply given by the assumed overall AISC per ounce of gold divided by 10.

¹⁸This is the share of world GDP in purchasing power parity of the eurozone in 2016. For more information, see <https://www.ecb.europa.eu/mopo/eaec/html/index.en.html>.

prudence of the relevant institutions and firms in communicating key figures of their business affairs to the public. There might be errors and inaccuracies in the data, even systematic ones, but that is an unavoidable problem.

Now, it is true that the most important items of expenditure are very different in the two sectors considered. Staff costs are astonishingly high in modern central banking, while mining companies invest more in capital and heavy machinery. One could thus point out that modern central banking after all creates jobs. And this can arguably be seen as a benefit, as Yaeger (1990, p. xxi) has wittily put it: “Nowadays, furthermore, it seems reasonable to suppose that central banks are valued for providing prestigious and comfortable jobs.” But it nevertheless qualifies as an expense that has to be financed, at least in real terms, by the rest of society. And it is by no means clear that it creates a net surplus of jobs. We can only hold with certainty that it creates a redistribution of real income from other sectors towards employees of the central bank system.

Moreover, if we are willing to leave the realm of the pure business accounting costs of central banking for a brief moment, there is much more to consider here. The ECB alone employed 3,171 full time staff in 2016. Most of these people are very well educated and trained. They are essentially diverted from other productive activities. There is thus also an unaccounted loss in *human capital* in other sectors of the economy.

There is another potential extension that we could make to the above analysis. In fact, central banks are not the sole producers of legal tender in a fractional reserve system. Commercial banks play an important role as well. It would probably be an exaggeration to factor in all the operating expenses of commercial banks, but a certain fraction of it would seem adequate. In particular, given the existence of entities like the Single Supervisory Mechanism by which central banks effectively gain partial control over the business affairs of commercial banks, one could argue that the latter are an extended arm of the former. Indeed, in practice, bank supervision means co-management. It grants intervention privileges to central bankers, such as on-site inspections, dividend suspensions, or attendance at credit committees. This indicates a potential route for further research in this area.

Chapter 10

Conclusion

A long journey through the economic analysis of the costs and benefits of central bank monetary policy lies behind us, and yet we have only seen a fraction of the tremendous amount of work that has been done on the topic. Nonetheless, we believe to have traced very important strands of thought that emphasize different aspects and that come to different conclusions.

In the following, we will first summarize our results in section 10.1, before we push our analysis to a synthesis. In section 10.2, we will tackle the problem of whether, and under what conditions, one can come to a definitive answer to the question that naturally arises from a cost-benefit analysis: Are the benefits worth the costs? We will end with some final remarks.

10.1 A Summary of Costs and Benefits of Central Banking

One important source of inspiration for this thesis has been *The Rationale of Central Banking* by Vera Smith, published in 1936. We have started our own analysis at the historical point in time when Smith ended hers, namely, the years that modern macroeconomics was born.

Some particularities of the developments in modern economic thought have brought us to adopt another organizing principle than Smith. While she classified the various contributions according to the conclusions drawn, and discussed the relevant literature in the context of the historical development of central banking in various countries, we have decided to make a distinction along a methodological dividing line. There are three reasons for that.

First of all, a country-by-country discussion would have been almost obsolete given that economic research has become increasingly an international affair. Secondly, modern macroe-

economics is, in our opinion, characterized not only by the advent of Keynesian economics, but also by a very influential methodological movement, namely, the steadily increasing influence of econometrics in the sense of Ragnar Frisch and his followers at the Econometric Society founded in 1930. Thirdly, when arguments clash, it seems natural to look for their bases. This ultimately leads to methodology.

We hold that the acceptance of the core postulates of econometrics is an important combining element in the most dominant stream of modern economic thought in general, and in monetary economics in particular. Frisch's dictum of transforming economics into a science, "*dans le sens restreint de ce mot*", has set firm roots. Advocates of what we call *instrumentalist-positivist* approaches to economics emphasize, admittedly to varying degrees and with specific nuances, the importance of the combination of two elements. Their goal is to bring, in Frisch's terms, a *theoretical-quantitative* analysis to some form of *empirical-quantitative* test. These approaches emphasize the importance of empirical predictions in evaluating economic models and policy conclusions drawn from them. This has important implications for the evaluation of the costs and benefits of central banking.

We have covered the contributions of four important schools of modern macroeconomic thought that fit under the umbrella of instrumentalist-positivist approaches in the first part of the thesis. We have devoted one chapter to each of them in chronological order: large-scale Keynesian macroeconometrics, Monetarism, New Classical and New Keynesian economics.

Each chapter starts with a review of some historical background and a discussion of the respective methodological orientation in order to justify our classification. We then turn to the specific analytical frameworks and models used, and finally their implications for monetary policy. We argue that there is, as a result of the underlying methodological outlook, an emphasis on those consequences of monetary policy that have immediate and readily measurable empirical counterparts, such as changes in unemployment, output, and price inflation. The short-run consequences on aggregated measures of these concepts are typically studied more thoroughly, while a rather agnostic stand on the long-run implications is taken. Alternatively, assumptions of long-run neutrality enter explicitly into the analysis.

We hold that one important reason for this tendency is the focus on predictive power. A model is regarded as reliable when its predictive power is high. Yet, any model's predictive power shrinks when the time horizon for predictions grows. We can thus naturally be more confident about the short-term predictions of a model and its short-term implications for monetary policy.

The first important instance of what might be called a bias towards short-run analysis, is the much discussed early Phillips curve. The early Phillips curve was, as we show in chapter 2, an integral part of large-scale Keynesian models. On the basis of the empirical material available at the time, these models suggested a trade-off between price inflation and unemployment. The perceived benefits of central banking, especially of central banking in combination with flexible fiat money, were the expected stimulating effects from expansionary monetary policies on employment and output. In particular, during times of general economic distress, central bank induced expansions of the money supply were seen as a potentially vital tool of countercyclical policy.

These large-scale models were the first major applications of Frischian econometrics. They provided, first of all, an argument for active monetary policy and a flexible money supply that should be produced outside of the market, and secondly, by implication, an argument for central banks as the very institutions responsible for the provision of money and the conduct of monetary policy.

The perceived benefits of expansionary central bank policies were subsequently characterized as a short-term phenomena, most notably, by members of the Monetarist school, as described in chapter 3. The Friedman and Phelps *natural-rate hypothesis* incorporated *adaptive expectations* into the analysis, and suggested that the beneficial impact of accelerated money growth would cease as soon as price inflation expectations have adapted to the actual rate of price inflation. In the long run, unemployment and real economic activity will return to their natural level as determined by real economic factors, such as labor laws and capital endowment. Monetary changes were then assumed to be neutral in the long run.

In our exposition, we have a strong emphasis on the writings of Milton Friedman. Not only did he provide a clear description of the methodological stance of instrumentalist-positivist economics that is congenial to Frisch's conception of econometrics, but he also published the most important contributions to our topic. While some of his work has been purely formal and theoretical, the bulk of his writings in monetary economics followed a strictly empirical approach. The latter was also ultimately what accounts for his policy conclusions.

He recommended the famous constant growth rule, based on extensive empirical investigations into the relationship between the money stock, price inflation and aggregate economic output, although in purely theoretical works, he identified a deflationary rule as optimal. He argued that an annual growth rate of 3 to 5% for high-powered money (i.e. M_1) would have been

roughly compatible with stable prices over the course of 94 years until 1960.

Friedman also made a corollary argument for 100%-reserve banking in the spirit of the Chicago Plan, which would significantly improve central bank control over the money stock. The benefit of the latter would be that a constant growth rule could be implemented more easily. It would further be facilitated by a pure fiat standard. Moreover, Friedman reinvigorated the classical cost-saving argument for fiat money. An additional benefit of a central bank controlled fiat money would be that the real resources, otherwise employed for gold mining, could be used productively elsewhere in the economy.

Now, while Friedman assigned a much more passive role to central banks than Keynesians, he did not deny their importance in promoting general economic stability. Monetary policy authorities would just have to abstain from too expansionary policies and from frequent policy changes. In his perspective, the costs of central banks materialize whenever they fail to stabilize the growth rate of the money stock, but instead cause even stronger fluctuations, or implement excessive rates of monetary expansion. In his later career, Friedman became increasingly skeptical about the benefits of paper money, and hinted at additional costs of fluctuating price inflation rates, such as resources employed for hedging against future price uncertainty.

In chapter 4, we argue that New Classical economists followed the alternative path for economic analysis that was implicitly suggested in Friedman's *Methodology of Positive Economics*. Instead of directly searching for regularities in the available empirical data, they engaged in mathematical model building, based on formal assumptions, in order to derive quantitative relationships that would then have to be empirically estimated and tested.

In response to the extremely influential *Lucas critique* of traditional large-scale modeling, New Classical economics emphasized the importance of microfoundations for macroeconomic models. The latter would be needed in order to find structural relationships that are invariant to policy changes. Only by means of such relationships could we engage in reliable counterfactual policy simulation and evaluation. An economic model must thus incorporate all relevant feedback effects of policy interventions on the behavior of economic agents.

New Classical economists developed the *dynamic stochastic general equilibrium* (DSGE) framework as an alternative to large-scale econometric models and as a solution to the Lucas critique. We show that the policy conclusions from DSGE models do not depend on their central building blocks, such as rational expectations or intertemporal utility and profit maximization of representative agents, but rather on their auxiliary assumptions. In fact, the baseline New

Classical model with perfect competition and flexible prices suggests neutrality of money in the strongest sense. There is no optimal monetary policy, or put differently, any monetary policy would be optimal.

This pure version has always been seen as merely a benchmark model for a world without monetary policy, or where monetary policy does not add any disturbances itself, but works perfectly and smoothly. It was seen as a starting point for more complex and practically relevant analyses. From the perspective of New Classical and New Keynesian economics, it schematically describes something like an ideal state that is to be approached in reality.

The transition from adaptive to rational expectations strengthened the conclusions drawn from the natural-rate hypothesis. It was argued that when expectations formation is rational, an expansionary monetary policy might be ineffective even in the short run, because agents would change their expectations immediately when they find out about the expansionary policy measures, and not only when price inflation rates actually start to change. Monetary policy can then have stimulating real effects only in so far as it is unexpected. On the basis of the *policy ineffectiveness proposition* and the postulated *inflation bias* of central banks, New Classical economics has made an even stronger case for monetary policy rules and against discretion.

In particular, the simple constant growth rule was again suggested as the tentatively optimal policy path as long as no other rule could persuasively be shown to be superior. Like Friedman, New Classical economists argued for a simple rule partly from acknowledged ignorance about the complex dynamics of the real economy. Moreover, a simple rule would be more easily understood by the public and hence would foster economic stability. The precise rate of expansion was subject to debate as the optimal rate of price inflation would have to be identified.

It was admitted that a more complex rule that incorporates feedback effects and allowed for well-defined exceptions might be superior, as unexpected events can happen all the time. New Classical economics has thus initiated an extensive research on optimal policy rules. The obvious benefit of central banks, from that point of view, is that they are the very institutions to implement the optimal rule. The argument for a fiat standard over a commodity standard is then also very simple. Any rule feasible under a commodity standard is *a fortiori* feasible under a fiat standard, but not the other way around.

The costs of central banking are the welfare losses caused by deviations from the optimal rule. They are typically specified in terms of losses in real economic activity and growth, or excessive price inflation or deflation. In this context, the discussion of *central bank independence*

is relevant, because it was argued that an independent central bank would be most effective against the inflation bias and generally more reliable in implementing a policy rule deemed optimal.

As pointed out above, the DSGE framework within which optimal policy rules are analyzed has been developed in response to the Lucas critique. A number of economists in the Keynesian tradition took the critique very seriously and adopted the framework for their own analyses as the alleged solution. This culminated into the most recent New Keynesian DSGE approach presented in chapter 5.

While both Monetarists and New Classical economists have criticized the naive Keynesian interpretation of the Phillips curve as a stable trade-off, the sutler expectations-augmented Phillips curve still became instrumental in rationalizing more active monetary interventions and justifying more complex feedback rules for monetary policy. The *New Keynesian Phillips curve* (NKPC), which is essentially still based on the natural-rates hypothesis, was developed within the DSGE framework, albeit under alternative auxiliary assumptions.

New Keynesian economists postulate monopolistic competition and price rigidity instead of perfect competition and fully flexible prices. There are thus, even under rational and perfect expectations, real economic effects of monetary policy in the short run. In the baseline New Keynesian model, optimal monetary policy sets the nominal interest rate, as a policy tool, in such a way that the real interest rate equals the *natural rate of interest*, which is the real interest rate that would occur under perfect price and wage flexibility, that is, under the New Classical baseline model. Moreover, optimal policy would have to correct for deviations of the actual price inflation rate from a specified target rate of price inflation, as well as deviations of output from its natural level, in order to ensure a single solution along the optimal equilibrium path within the DSGE model. Rules of this kind have become known as *Taylor rules*.

In practice, there are obvious problems with these rules, since they incorporate unobservable concepts like the natural rate of interest and the natural level of output. These concepts must be operationalized by means of proxy variables. We have shown that there are fundamental drawbacks to that approach. One can never test the suitability of a proxy and the adequacy of the model at the same time. One can in principle only test one source of error, and this only if the other sources can reliably be assumed away. In practice, monetary policy evaluation within that framework remains a matter of more or less arbitrary proxy selection.

The natural rate of interest has been estimated in different recent studies. The consensus

view is that it has significantly fallen. Under central bank controlled fiat money, nominal interest rates can be adjusted accordingly, so that the real rate of interest is close to the estimated natural rate. However, these estimates are themselves contingent on the models used. When a model requires a low natural rate to provide a reasonable fit to the observed data, the estimated natural rate will indeed be low when the model itself or similar ones underlie the estimation. We have thus argued that the approach is, in a sense, self-fulfilling.

Moreover, we have discussed the use of external shocks in the estimation and interpretation of the NKPC. In regression analyses, external shocks, or more precisely, well chosen proxies for external shocks, can account for an arbitrary proportion of the actually observed changes in target variables, such as the price inflation rate. We have shown how monetary policy then effectively ends up as the explanation for the residuals, that is, the part that is not statistically accounted for by the shocks or other explanatory variables deemed unrelated to monetary policy within the regression analysis.

From the perspective of New Keynesian economics, the role of central banking is then to accommodate real economic changes adequately. If the natural rate decreases due to external forces, central banks would have to lower nominal interest rates accordingly. If adverse supply shocks cause price inflation to accelerate, central banks should not try too hard to prevent the inflation rate from rising, since this will cause losses in real economic activity along the NKPC. Instead, monetary policy should accommodate the real shocks by adequate monetary expansion to free up additional spending power that would be lost otherwise, since an increased expenditure share is devoted to the goods for which the adverse supply shock occurred.

The benefits of central bank policy are thus seen as a reduction of the adjustment costs to real economic changes. The costs of central banking is the possible failure of monetary policy to adequately accommodate real shocks, that is, they are interpreted as benefits foregone. They are again welfare losses due to deviations from an optimal policy path.

The evaluation of the costs and benefits is, however, entirely dependent on the proxy variables chosen and the empirical methods applied. From a purely theoretical DSGE model as such, there can hardly be drawn any relevant conclusions, since it is based on arguably unrealistic and formal assumptions. They are empty shells that carry no weight if they are not filled with empirical information. In their application to practical policy problems, they are then as vulnerable to the Lucas critique as any other model in the instrumentalist-positivist tradition.

The methodological alternative lies, in our opinion, in what we call the *causal-realist*

approach. It is covered in the second part of the thesis. More specifically, we focus on the contributions of Austrian economics to the evaluation of the costs and benefits of central banking.

In chapter 6, we draw the methodological dividing line to the approaches discussed in the first part of the thesis. We provide a critique of modern econometrics as applied within the instrumentalist-positivist tradition, and substantiate our claim that the Lucas critique hints at a fundamental problem in empirical-quantitative research that is not solved by DSGE modeling. In fact, we argue that the Lucas critique can be reformulated as an *a priori* proposition about the limitations of empirical methods in the social sciences. We show that, from the perspective of a human being, exact empirical and scientific predictions of variables that are themselves the direct outcome of human choice and action are in principle impossible. This is because human knowledge changes in scientifically unpredictable ways, but influences the outcomes that are to be predicted.

The radical conclusion from this argument is that the inductive element of modern econometrics has to be rejected. It is, of course, not to argue that all research conducted within the instrumentalist-positivist tradition is without merit. Their descriptive elements are of great value. However, when generalized conclusions in terms of policy recommendations for the future are based on historical empirical relationships of macroeconomic aggregates, particular caution is required.

We argue that causal-realist economics assigns a narrower role to empirical-quantitative methods. They are not used inductively, but only descriptively as analytical tools in economic history and accounting. It is then also shown how the hitherto neglected original conception of econometrics by Polish economist Pawel Ciompa, presented 16 years prior to Ragnar Frisch's interpretation of the term, is compatible with the causal-realist position.

Causal-realist economics in general, and Austrian economics in particular, do not declare empirical prediction to be ultimately the highest quality standard for scientific inquiry. Rather, they try to gain insight from thorough reflection on the nature of their subject matter and proceed deductively from there. This again has very important implications for the conclusions drawn in the field of monetary economics.

In chapter 7, we present the most important elements in the Austrian theory of money and monetary policy for our topic. The logical, time and place invariant, implications of monetary policy, or more precisely, the implications of monetary expansion from outside of the market, are traced. We argue that monetary expansion, understood as an increase in the money stock, has

inevitable implications. Yet, our propositions invoke a comparison between the factual scenario that actually occurs and the counterfactual scenario that would have happened if it were not for monetary expansion. Hence, they ultimately elude empirical testing.

As a first and most immediate consequence of monetary expansion, the purchasing power of money will be lower than it would have been otherwise. Moreover, a redistribution of wealth as explained by Cantillon effects occurs from the late receivers to the early receivers of newly created money.

The specific mechanism by which new money enters the economy further determines its consequences. There are structural particularities of the redistributive effect when the increase of the money stock occurs via credit expansion. We argue that credit expansion leads to a perverse redistribution of income and wealth from bottom to top, due to the inherently discriminatory nature of credit. Lenders tend to discriminate in favor of those borrowers that represent a lower expected default risk, that is, they tend to prefer the haves over the have-nots. The haves then receive the new money first and benefit, not least from a lower interest rate than would have prevailed without monetary expansion.

A central bank induced decrease of interest rates can have more far-reaching effects in the form of business cycle fluctuations. Hence, an outline of the *Austrian Business Cycle Theory* (ABCT) is provided. According to that theory, an unsustainable boom or upswing is initiated whenever the market rate of interest is pushed below the *natural rate of interest* in the Misesian sense, that is, the equilibrium rate of the counterfactual economy without credit expansion.

In such a case, entrepreneurs will engage in more investment projects than can be sustained given the amount of real savings available. In the course of the respective price adjustment process, certain investments will have to be liquidated as they turn out to be unprofitable. This constitutes the subsequent downturn or crisis. The potentially contagious effect of bankruptcies can aggravate the latter.

Here, another cost dimension of credit expansion emerges. We argue that credit expansion leads to a substitution of equity finance by credit finance, and hence causes higher *systemic risk*. The overall leverage in the economy is increased and the capacity to absorb losses once they occur is diminished.

We also argue that the existence of central banks and fiat money lead to a genuine *moral hazard* that ultimately hinges on competitive privileges established by law, in particular, *legal tender laws*. The moral hazard is, however, not limited to monetary policy authorities themselves.

It propagates to commercial banks as intermediaries as well as money users in general. To the extent that central banks act, or are expected to act, as a *lender of last resort* that stops a systemic breakdown once it is looming, and recapitalizes struggling banks and businesses, it incentivizes riskier investment projects and business strategies. In particular, the leverage ratio on which a business operates is subject to entrepreneurial decision making and is thus influenced by the expected costs of an investment or business failure. If the latter is systematically lowered by expected central bank support in times of liquidity shortages and insolvency, the leverage ratio tends to be higher. This is the case, because part of the costs of business failure can be imputed on to third parties, not exclusively, but also due to central bank interventions.

From this perspective, it is clear that modern central banking leads to unintended consequences as far as stated public policy goals are concerned. If these side-effects are to be countered, further policy interventions have to be implemented that may go beyond the area of monetary policy proper. However, if the initial monetary policy interventions cause further interventions in other areas, then we have to take account of the consequences of the latter in order to obtain a more comprehensive cost-benefit analysis of the former.

This idea underlies and motivates chapter 8. We apply the Misesian theory of interventionist spirals to provide an alternative explanation for stagflation as experienced in the second half of the 20th century. In this chapter, we leave the narrow realm economic theory and enter into economic history. We engage in descriptive data analysis and show that there has been a positive link between present price inflation and future unemployment since the 1960s. While we do not try to establish a necessary theoretical link between the two magnitudes, we argue that the observed phenomena can be explained in part by incorporating the political process of interventionism.

ABCT can be reconciled with the short-run Phillips curve, that is, a negative correlation between current price inflation and unemployment, both over the inflationary boom and the deflationary bust. Moreover, it suggests that we might observe something like a positively-sloped medium-run Phillips curve. The initial inflation might correlate with unemployment in the medium-run, that is, when the corrective economic downturn sets in. However, in the long run, when the business cycle fluctuations have played out, it seems that a positive link can be established only by incorporating historically contingent policy decisions in reaction to the unintended consequences of monetary expansion.

Both increased unemployment during economic downturns and redistributive effects from

bottom to top are commonly regarded as problems to be solved by public policy. Hence, there are political incentives to implement reverse redistributive measures. We can regard the creation and expansion of welfare state programs as part of this tendency. Moreover, there have been numerous interventions into labor markets, for example, in the form of job protection or minimum wage legislation. These measures have themselves numerous effects. One of them is that labor markets will become less flexible and the *structural rate of unemployment* will increase.

It is of course not argued that any political decision made in the second half of the 20th century is solely motivated by the unintended consequences of monetary policy, but we suggest that the latter have contributed to the problems that present-day politics tries to solve.

The last chapter of the second part of the thesis contains an application of econometrics in Pawel Ciompa's sense. We engage in a pure business-accounting exercise for the system of central banks in the eurozone. We pick up the classical cost-saving argument for fiat money and try to analyze what can be said about it from an empirical angle. While in theory fiat money could be produced at negligible costs, we investigate whether this is actually the case in practice.

We find that the annual operating expenses of the central banks in the euro area are likely to be higher than the fraction of the overall annual costs for world-wide gold mining that corresponds to the euro area's contribution to world GDP. This is an even more interesting result, given the fact that annual gold production since the Nixon shock has not actually decreased. It has reached an all-time high in 2015 and 2016.

Capital and labor are thus still devoted to gold mining, and gold is demanded for various purposes, not least by central banks themselves. It is only fair to assume that even more resources would be devoted to gold mining today, if the gold standard had prevailed, but quantifying this counterfactual comparison lies outside of what empirical research can teach us. We can see, however, that operating expenses of central banks as public policy institutions are not nearly as low as one might expect. It is indeed a mundane and narrow conception of costs that we have in mind here, but it is one that cannot be ignored, if it is our goal to contribute to a more comprehensive cost-benefit analysis of central banking.

10.2 Towards a Synthesis

The dividing line drawn in this thesis is based on methodological considerations, that is, on attempts to answer questions about the proper conduct of economic analysis, and, more impor-

tantly, the justification of economic propositions. We have described the fundamental differences between the instrumentalist-positivist approach on the one hand, and the causal realist approach on the other hand, at some length. We have shown how the methodological position affects the selection of specific aspects of central banking that are analyzed more closely. But there is, the many divergences notwithstanding, at least some common ground with respect to the analytical frameworks used, if only a rudimentary one. We will first consider the latter, before we come to the challenging problem of whether from any of the two methodological positions a definitive answer can be given on the *to be or not to be* of central banks.

The debatable analytical commonality becomes, in our opinion, most visible in the concepts of the *natural rate of interest*. It is an essential part of modern DSGE modeling as well as the Austrian theory of business cycles. And in both instances, the natural rates that are postulated are conceptually not altogether different. The necessary and costly real adjustments that occur in the observed economic system are partly explained in terms of deviations of market rates from the natural rate. The natural rate refers in both cases to an equilibrium state. The central difference lies in what is made of the deviations.

Most of the contemporary discussions of optimal monetary policy rules, within the research program of instrumentalist-positivist economics, essentially ask how to push market rates actively towards the natural rate, and at the same time, keep price inflation on target as well as real economic activity at its long-term potential. The presumption is that market interest rates are deviating from some natural rate, because of external demand and supply shocks, and that optimal central bank policy should monitor these shocks and adjust interest rates accordingly. It ultimately hinges on the possibility of inferring the state of the natural rate by means of empirical proxies.

Within the Austrian framework, market interest rates are also assumed to fluctuate due to what might be called “external shocks,” or simply changes in supply and demand, but these are regarded as an essential part of a constantly evolving and changing economic system. Furthermore, it is held that they cannot be monitored in order to inform public policy in any meaningful sense. Again, the causal factor of the recurring business cycle is a systematic deviation of market interest rates from some natural level, but there is no reason to believe that a systematic deviation would prevail on the financial markets if it were not for central bank intervention. In fact, the very institution that is most capable in bringing such a systematic deviation about is the monopolist in legal tender production itself.

In a sense, these are two diametrically opposed views on two conceptually similar analytical frameworks. That one is typically presented in mathematical terms and the other verbally, and that the natural rates are defined differently, can be ignored for a moment. The first view holds that there is a role for active monetary policy in improving market outcomes. The solution to real economic disturbances, caused externally, is adequate monetary policy. In the other view, the market process is self-adjusting and monetary policy is incapable of systematically improving it, which is not to say that it works perfectly. However, if changes in supply and demand on credit markets occur, price and quantity adjustment processes should be left to their own devices, since there is no way of knowing where either quantities or prices should be.

Now, it is not merely a cliché that there is also a stronger tendency for Monetarists and New Classicalists to emphasize the self-adjusting powers of the market, relative to more Keynesian-inspired economists of various sorts.¹ This begs the question whether it is possible at all, within the instrumentalist-positivist program, to come to a definitive evaluation of the role and necessity of central bank monetary policy. Likewise, our exposition of the causal-realist approach that focuses on Austrian economics highlights aspects of central banking that would generally be considered as costs. However, could we also think of a causal-realist argument in favor of central banking? Under what conditions and to what extent can a general conclusion be reached?

Let us start with the instrumentalist-positivist position. The analytical starting point is an abstract economic system subject to some central bank policy. As we have shown previously, the aim is to find the optimal monetary policy. Now, it is clear that for any given system, there can be an optimum only with respect to a well-defined criterion. Hence, the problem is twofold. First, we need a specification of the system, that is, an operationalized version of it. We have shown how predictive power is the guiding methodological principle in that regard. Next, once the specification of the system is given, we need a criterion to determine the optimum.

In order to remain within the boundaries of instrumentalist-positivist economics, this criterion must be stated in terms of observable empirical outcomes. For example, the optimal monetary policy could be defined as one that ensures that price inflation, as measured by deviations of the

¹At the Lindau Nobel Laureate Meeting in Germany, in August 2017, this tendency became very obvious, in a most comical way, during a panel discussion between Peter A. Diamond, Edward C. Prescott, and Christopher A. Sims, on “New Conditions for Monetary and Fiscal Policy?”, moderated by Martin F. Hellwig. In order to solve current economic problems, specifically but not exclusively within the eurozone, Prescott wholeheartedly insisted that fiscal discipline was important and ought to be respected. Sims urged the listeners to take into consideration how essential it was to actively bring price inflation rates back on target, that is, 2%, and Diamond emphasized that aggregate demand needs to be stimulated and that this could only be done, if monetary and fiscal policy work hand in hand. Gentlemen that they are, it did not come to serious criticism between them.

consumer price index, is close to 2% per year. However, such a criterion cannot be established objectively from within the framework. Whatever might be proposed, can be disputed on various grounds. One might ask: “What is the criterion for the criterion?” Ultimately, it seems, it must be established by convention.

Moreover, any optimal rule that is suggested within that general framework has merely the status of being *tentatively* optimal, that is, optimal as long as some structural change in the economic system occurs and a new optimal rule has to be identified, possibly based on other criteria. This qualifier extends to the specification of the system, or the underlying economic model, as well. There is then, from the perspective of instrumentalist-positivist economics, no way to come to a definitive answer on what constitutes optimal monetary policy. Any given rule might suggest more or less active policy interventions. It might even suggest that a fiat money standard with a fixed money stock and 100% reserve banking is optimal, given a properly chosen criterion, such as minimization of administrative and supervisory expenses. Such a proposal could effectively be taken as advocating the abolition of modern central banking. Yet, besides not having been uttered, as far as we know, by any noteworthy economist in that tradition, it can be disputed internally from various angles. The choice of the criterion ultimately must be arbitrary or value-laden.

In contrast, the causal-realist tradition does not ask what the tentatively optimal monetary policy rule might be and does not have to establish a formal criterion for its analysis. It merely attempts to trace the consequences of monetary policy deductively. How these consequences are to be evaluated on the part of economic analysts or policy makers is not subject of the economic inquiry itself. And yet, in chapter 7, we have at least vaguely hinted at a possible route to come to a clear and definite conclusion.

Let us rephrase the question at hand as follows: Is monetary policy as conducted by central banks today overall beneficial? Any given individual might answer differently. An employee of a central or commercial bank might be biased towards answering in the affirmative. An ardent reader of Austrian economics might be of a very different opinion. But is there a way to come to a somewhat more objective assessment? We think there is, at least under specific conditions.

As argued at the end of section 7.1 (pp. 243ff.), by the mere fact that the legal tender is in various countries indeed the most commonly used medium of exchange, we *cannot* simply deduce that it actually satisfies the subjective preferences of money users better than any available alternative. This is because there is an element of political force in making it acceptable in

transactions. In particular, legal tender laws make central bank produced fiat money a compulsory means of payment for certain obligations. It is therefore granted a privileged legal status and has a competitive advantage over potential market alternatives. However, what if it were not for these legal privileges?

If central bank produced money, and the monetary policy measures that its users are subjected to, were competing on equal footing with potential market alternatives, such as cryptocurrencies, precious metals, or indeed money produced by other central banks or private institutions, our assessment could go further. By virtue of revealed preferences (Rothbard, 2011, ch. 17), one could argue that the good that becomes, under these conditions, in any given area, the most commonly accepted medium of exchange actually satisfies the subjective preferences of the members of the community best. Its use and hence the existence of its producers are overall beneficial, at least from the perspective of those people who do in fact use it. For them, the selected medium of exchange is subjectively optimal among all the available alternatives, all relevant properties taken into account.²

Any expected changes in the stock of that medium of exchange, what we might misleadingly call expected monetary policy interventions, are at least compensated sufficiently by its other desirable properties. If a central bank produced paper money would remain the most commonly used medium of exchange under these conditions for an extended period of time, it would seem to us legitimate to call that central bank's monetary policies optimal in that limited sense. And it would be completely irrelevant under what economic framework and what methodological orientation these monetary policies are derived and justified. Whether inspired by instrumentalist-positivist, causal-realist, or any other economics, they would have to stand the test of subjective evaluation by the money users under equal competition.

The above scenario would allow us to assess whether certain measures with respect to the quantity of a medium of exchange can be considered optimal, under full respect of the subjective theory of value. It would thus hypothetically allow us to investigate whether the inflationary policies of modern central banks are overall beneficial. However, it is plainly obvious that the scenario itself would amount to an abolition of central banks as we know them. The defining characteristic of the latter is that they hold a government granted monopoly in legal tender production. If the legal privileges were abolished, there would be no central bank to speak of and hence no monetary policy.

²Whether a property is relevant or not is also a matter of subjective assessment.

This leads us to the somewhat paradoxical conclusion that one could ultimately assess the policy interventions of modern central banks only if the latter were abolished. Our intuition is that a producer of a medium of exchange under equal competition would never engage in anything that remotely resembles modern central bank policies, precisely because they would fail the test of subjective evaluation by the money users.

10.3 Final Remarks

We have seen that the most pressing question that naturally arises from a cost-benefit analysis of central banking is whether the costs ultimately outweigh the benefits, and hence, whether central banks should be abolished or not. The easiest way out of the problem, from the perspective of positive economic science, is to say that answers to questions like this are a matter of subjective value judgments. It lies outside of economics proper. While the claim is true, one has to notice, that this is ultimately the case whenever questions of public policy are concerned (Rothbard, 2011, ch. 5). An economist's recommendation for public policy can never be *value-free*. There must be either agreement on the goals to be achieved, that is, harmony of value judgments, or disregard for opposing value judgments.

The problem seems even more pronounced for our specific question. Whether or not to have a central bank is not merely a question of what public policy ought to be implemented. It ultimately boils down to something more existential: Should there be any public policy at all with respect to the supply of money?

It was Joseph Schumpeter (2006, [1954], part I, ch. 4), in his *History of Economic Analysis*, who stressed the role and inevitability of ideology in economic analysis. According to him ideology enters economic analysis at the very start, in the “preanalytic cognitive act that supplies the raw material for the analytic efforts”, which he would also refer to as the “vision” (p. 39). Schumpeter explains:

Analytic work begins with material provided by our vision of things, and this vision is ideological almost by definition. It embodies the picture of things as we see them, and wherever there is any possible motive for wishing to see them in a given rather than another light, the way in which we see things can hardly be distinguished from the way in which we wish to see them. (p. 40)

The vision is then what pushes an economist in a certain direction from the outset. It might subtly affect the methodological inclinations as it comprises the very fundamental questions of how we look at the world, and more importantly, how we look at man. Are we willing to assign more to human beings than to the inanimate objects of physics or astronomy? Or do we ultimately abstract from action, choice, preferences, means and ends, if only as a matter of scientific pragmatism?

Whatever the answer might be, it seems to us that there is another prominent and arguably more important place for ideology, not at the start, but at the very end of economic analysis. Ideology frequently becomes a placeholder for ignorance or a substitute for blanks that economic analysis as such cannot fill. It is added, usually without explicit admission, to answer questions that lie outside of economics as a value-free discipline.

It thus seems as if the question that this thesis ultimately leads to is precisely one of ideology and vision. Whether or not the benefits of modern central banking outweigh the costs is a matter of subjective value judgments. And the answer to the fundamental question of whether the institution ought to be abolished must be found in ethics. Our proposed principle of revealed preferences as a criterion for the assessment of monetary policy must itself ultimately be justified on ethical grounds, if it can be justified at all. Economics can only help elucidate certain effects of monetary policies and the underlying institutional arrangements. How these effects are evaluated is a different matter entirely. Economics, “like the other sciences, is the value-free handmaiden of values and ethics” (Rothbard, 2011, p. 83). What is made of the insights that economics provides is up to those who understand them, or only believe to do so.

Appendix A

Mathematical Compendium to the Klein-Goldberger Model

This appendix provides a list of variables and estimated equations from the model presented in Klein and Goldberger (1955). The estimation method applied by the authors is explained briefly.

A.1 List of Variables

The Klein Goldberger-Model contains the following 20 *endogenous* variables:

- **Income** (5): National income (Y); Private wage income (W_1); Farm income (A)¹; Non-farm non-wage income (P); Corporate profits (P_c)
- **Liquid assets** (2): Held by persons (L_1); Held by enterprises (L_2)
- **Prices** (3): Price index of GNP (p); Index of agricultural prices (p_A); Index of hourly wages (w)
- **Interest rates** (2): Yield on short-term commercial paper (i_S); Yield on corporate bonds (i_L)
- **Consumption** (C); **Gross private investment** (I); **Depreciation** (D); **Imports of goods and services** (F_I); **Private capital stock** (K); **Corporate savings** (S_p); **Corporate surplus** (B); **Number of wage earners** (N_w)

¹This variable was later (for a revised version of the model) split into farm income excluding subsidies (A_1) and farm subsidies (A_2). Hence, the identity $A = A_1 + A_2$ holds. In the revised version of the model only A_1 is considered to be endogenous. A_2 , as a policy instrument, is exogenous.

The model contains 19 *exogenous* variables:

- **Policy variables** (9): Government expenditures (G); Government wage bill (W_2); Number of government employees (N_G); Net tax on wage income (T_W); Net tax on farm income (T_A); Net tax on non-farm non-wage income (T_p); Corporate income tax (T_C); Indirect taxes less subsidies (T); Excess reserves of banks as a percentage of total reserves (R)
- **Variables determined by conditions abroad** (3): Exports of goods and services (F_E); Index of agricultural exports (F_A); Import prices (P_I)
- **Demographic variables** (5): Index of hours (h); Number of persons in the labor force (N); Number of farm operators (N_F); Number of non-farm entrepreneurs (N_E); Number of persons in the US (N_p)
- Time trend (t); Constant term

Moreover, the model incorporates 24 additional variables that are simply lagged values of some of the above variables, such as Y_{t-1} ; $(W_1)_{t-1}$ from the set of endogenous variables and $(W_2)_{t-1}$; T_{t-1} from the set of exogenous variables.

A.2 Estimated Equations

Applying the limited information maximum likelihood method to annual data for the periods of 1929-1941 and 1946-1952, the following 15 equations were estimated:²

$$C_t = -22.26 + 0.55(W_1 + W_2 - T_W)_t + 0.41(P - T_p - S_p)_t + 0.34(A_1 + A_2 - T_A)_t + 0.26C_{t-1} + 0.072(L_1)_{t-1} + 0.26(N_P)_t \quad (\text{A.1})$$

$$I_t = -16.71 + 0.78(P - T_p + A_1 + A_2 - T_A + D)_{t-1} - 0.073K_{t-1} + 0.14(L_2)_{t-1} \quad (\text{A.2})$$

$$(S_p)_t = -3.53 + 0.72(P_c - T_c)_t + 0.076(P_c - T_c - S_p)_{t-1} \quad (\text{A.3})$$

$$(P_c)_t = -7.60 + 0.69P_t \quad (\text{A.4})$$

²The following estimates correspond to the revised model in Klein and Goldberger (1955, pp. 90-92) incorporating data from 1951 and 1952. The original model was estimated based on data from 1929-1941 and 1946-1950 (Klein and Goldberger, 1955, pp. 51-53).

$$D_t = 7.25 + 0.10 \frac{K_t + K_{t-1}}{2} + 0.044(Y + T + D - W_2)_t \quad (\text{A.5})$$

$$(W_1)_t = -1.40 + 0.24(Y + T + D - W_2)_t + 0.24(Y + T + D - W_2)_{t-1} + 0.29t \quad (\text{A.6})$$

$$(Y + T + D - W_2)_t = -26.08 + 2.17[h(N_W - N_G) + N_E + N_F]_t + 0.16 \frac{K_t K_{t-1}}{2} + 2.05t \quad (\text{A.7})$$

$$w_t - w_{t-1} = 4.11 - 0.74(N - N_W - N_E - N_F)_t + 0.52(p_{t-1} - p_{t-2}) + 0.54t \quad (\text{A.8})$$

$$(F_I)_t = 0.32 + 0.0060(W_1 + W_2 - T_W + P - T_p + A_1 + A_2 - T_A)_t \frac{p_t}{(P_I)_t} + 9.81(F_I)_{t-1} \quad (\text{A.9})$$

$$(A_1)_t \frac{p_t}{(p_A)_t} = -0.36 + 0.054(W_1 + W_2 - T_W + P - T_P - S_p)_t \frac{p_t}{(p_A)_t} - 0.007(W_1 + W_2 - T_W + P - T_P - S_p)_{t-1} \frac{p_{t-1}}{(p_A)_{t-1}} + 0.012(F_A)_t \quad (\text{A.10})$$

$$(p_A)_t = -131.17 + 2.32p_t \quad (\text{A.11})$$

$$(L_1)_t = 0.14(W_1 + W_2 - T_W + P - T_P - S_p + A_1 + A_2 - T_A)_t + 76.03(i_l - 2.0)_t^{-0.84} \quad (\text{A.12})$$

$$(L_2)_t = -0.34 + 0.26(W_1)_t - 1.02(i_S)_t - 0.26(p_t - p_{t-1}) + 0.61(L_2)_{t-1} \quad (\text{A.13})$$

$$(i_L)_t = 2.58 + 0.44(i_S)_{t-3} + 0.26(i_S)_{t-5} \quad (\text{A.14})$$

$$100 \frac{(i_S)_t - (i_S)_{t-1}}{(i_S)_{t-1}} = 11.17 - 0.67R_t \quad (\text{A.15})$$

The following 5 auxiliary equations were also used. They do not require econometric estimation of parameters but share the character of definitional statements:

$$C_t + I_t + G_t + (F_E)_t - (F_I)_t = Y_t + T_t + D_t \quad (\text{A.16})$$

$$(W_1)_t + (W_2)_t + P_t + (A_1)_t + (A_2)_t = Y_t \quad (\text{A.17})$$

$$h \frac{w_t}{p_t} (N_W)_t = (W_1)_t + (W_2)_t \quad (\text{A.18})$$

$$K_t - K_{t-1} = I_t - D_t \quad (\text{A.19})$$

$$B_t - B_{t-1} = (S_p)_t \quad (\text{A.20})$$

A.3 The Limited Information Maximum Likelihood Method

The general idea is the following. In order to make any probability statements concerning the econometric model certain assumptions about the random error terms are needed. The 15 structural equations do not claim to explain the dynamics of the endogenous variables completely, but only partly. As Klein and Goldberger (1955, p. 42) put it: “Another part is unaccounted for by our theory of the economic structure. We have no preconceptions about the size of the unexplained errors; *we simply believe that they are accounted for by the laws of chance.*” More precisely, assumptions about the random distributions of the error terms are needed.

Generally, we can assume that there exists a joint probability distribution for all error terms, $u_{i,j}$, for $i = 1, 2, \dots, 15$ (for each of the 15 structural equations) in all $j = 1, 2, \dots, 20$ years in the sample, defined by its density function p :

$$p(u_{1,1}, \dots, u_{1,20}, \dots, u_{15,1}, \dots, u_{15,20}).$$

Under the assumption of timewise mutual independence³ the joint probability distribution reduces to:

³That is for each $j, k \in [1 : 20], j \neq k$, the joint random variables $(u_{1,j}, \dots, u_{15,j})$ and $(u_{1,k}, \dots, u_{15,k})$ are independent and follow the same random distribution, i.e. share a common density function, say p .

$$p(u_1, \dots, u_{15})^{20}$$

In the *full information maximum likelihood* approach, given p , usually taken to be a normal distribution with zero mean, the coefficients in the structural equations would be chosen in such a way that the resulting residuals maximize the joint density function. As this procedure would have required more computing power than was available at the time, the limited information maximum likelihood method was applied. It allows to focus on each structural equation, or any selected subset of structural equations that can be handled at once, and the respective error terms separately. It is very closely connected to the nowadays commonly used and well-known two-stage least squares estimator (Anderson, 2005).

Appendix B

Mathematical Compendium to the New Classical DSGE Model

This appendix contains a canonical version of the New Classical dynamic stochastic general equilibrium (DSGE) model. It is closely related to the standard model framework of Real Business Cycle (RBC) theory based on the works of Kydland and Prescott (1982) and Prescott (1986). It was for some time the reference framework for the analysis of macroeconomic fluctuations and its New Keynesian adaptation, presented in appendix C, has become the core model of modern monetary analysis, although RBC theory abstracts completely from monetary factors. Among others, it were Cooley and Hansen (1989), who added a monetary sector to the RBC model framework, which under typical new classical assumptions led to predictions of (near) monetary neutrality, attaching a rather limited role to money and monetary policy. The following exposition is almost identical to the one found in Galí (2008, ch. 2) and uses the name notation.

B.1 The Model

We present the model in three parts, the representative household, the representative firm, and the general equilibrium conditions.

B.1.1 The Representative Household

The representative household at time $t = 0$ is maximizing expected utility over an infinite time horizon, given by the optimization problem:

$$\max_{C_t, N_t} E_0 \sum_{t=0}^{\infty} \beta^t U(C_t, N_t), \quad (\text{B.1})$$

subject to a series of budget constraints:

$$P_t C_t + Q_t B_t \leq B_{t-1} + W_t N_t - T_t, \quad (\text{B.2})$$

where

- E_0 denotes the expectation at time $t = 0$,
- β is a discount factor such that $0 < \beta < 1$,
- C_t is a consumption index for period t ,
- N_t is hours worked in t ,
- U is the utility function, which is twice differentiable, such that

$$\frac{\partial U}{\partial C_t} = U_{c,t} > 0, \quad \frac{\partial^2 U}{\partial^2 C_t} = U_{cc,t} \leq 0, \quad \frac{\partial U}{\partial N_t} = U_{n,t} \leq 0, \quad \frac{\partial^2 U}{\partial^2 C_t} = U_{nn,t} \leq 0,$$

- P_t is the price index corresponding to the consumption index,
- W_t is the nominal wage of the household,
- B_t represents the quantity of one-period, nominally riskless discount bonds purchased in period t and maturing in period $t + 1$ and paying off one unit of money,
- Q_t is the price of the bond in t ,
- T_t represents lump-sum additions (dividends) or subtractions (taxes) from the household's income in t .

We also have the additional no-Ponzi-scheme assumption, $\lim_{T \rightarrow \infty} E_t \{B_T\} \geq 0, \forall t$. One can derive the following optimality conditions from C.1 subject to B.2 for $t = 0, 1, 2, \dots$:

$$-\frac{U_{n,t}}{U_{c,t}} = \frac{W_t}{P_t},$$

$$Q_t = \beta E_t \left\{ \frac{U_{c,t+1}}{U_{c,t}} \frac{P_t}{P_{t+1}} \right\}.$$

Assuming a specific form of the utility function, such as the one used in Galí (2008, p. 17):

$$U(C_t, N_t) = \frac{U_t^{1-\sigma}}{1-\sigma} - \frac{N_t^{1+\varphi}}{1+\varphi},$$

the optimality conditions are

$$C_t^\sigma N_t^\varphi = \frac{W_t}{P_t}, \quad (\text{B.3})$$

$$Q_t = \beta E_t \left\{ \left(\frac{C_{t+1}}{C_t} \right)^{-\sigma} \frac{P_t}{P_{t+1}} \right\}. \quad (\text{B.4})$$

Log-linearization of the conditions B.3 and B.4, where lower case letters represent the natural logarithm of the respective upper case letters, gives:

$$\sigma c_t + \varphi n_t = w_t - p_t, \quad (\text{B.5})$$

$$c_t = E_t\{c_{t+1}\} - \frac{1}{\sigma}(i_t - E_t\{\pi_{t+1}\} - \rho). \quad (\text{B.6})$$

The variable π_{t+1} corresponds to the rate of price inflation, $p_{t+1} - p_t$, and i_t is defined as $-\ln(Q_t)$ and is thus interpreted as the nominal interest rate. Note that Q_t is the price of the bond B_t that pays off one unit of money in the next period. Hence, $1 = Q_t(1 + i_t)$, where i_t is the interest rate on the bond. It follows that $-\ln(Q_t) = -\ln(1 + i_t)^{-1} = \ln(1 + i_t) \approx i_t$, when i_t is small.¹ The coefficient ρ can be interpreted as the household's rate of time preference, i.e. its discount rate.

Money enters the model in the form of an add-hoc money demand function. An alternative approach is presented in much detail for example in Walsh (2010, ch. 2 and ch. 8), where money holdings enter the utility function and allow the derivation of a money demand function, which leads to a very similar result. The money demand function that Galí (2008, p. 18) uses has the following form:

$$m_t - p_t = y_t - \eta i_t, \quad (\text{B.7})$$

where m_t and y_t are the natural logarithms of output and nominal money demand, respectively. $\eta > 0$ denotes the interest rate elasticity of the money demand.

¹For details on deriving equation B.6 from equation B.4 see Galí (2008, appendix 2.1, pp. 35-36). It is obtained by approximating around a steady state of constant rates of output growth and price inflation.

B.1.2 The Representative Firm

The behavior of the representative firm, more precisely its demand for labor, is simply derived from profit maximization subject to a production function, that is,

$$\max_{N_t} P_t Y_t - W_t N_t, \quad (\text{B.8})$$

subject to

$$Y_t = A_t N_t^{1-\alpha},$$

where A_t is an exogenous stochastic process that represents the evolution of technology. Hence, the optimality condition for problem B.8 is satisfied when the real wage equals the marginal product of labor:

$$\frac{W_t}{P_t} = (1 - \alpha) A_t N_t^{-\alpha}.$$

Log-linearization of the condition gives

$$w_t - p_t = \ln(1 - \alpha) + a_t - \alpha n_t. \quad (\text{B.9})$$

B.1.3 General Equilibrium Conditions

The general equilibrium condition is that in every period t the output produced by the representative firm is equal to the consumption of the representative household, that is, $y_t = c_t$. Using the optimality conditions B.5, B.9, the log-linearized production function of the firm, $y_t = a_t + (1 - \alpha)n_t$, as well as the general equilibrium condition, one can derive equilibrium paths of output and employment:

$$n_t = \psi_{na} a_t + \vartheta_n \quad (\text{B.10})$$

$$y_t = \psi_{ya} a_t + \vartheta_y, \quad (\text{B.11})$$

where $\psi_{na} = \frac{1-\sigma}{\sigma(1-\alpha)+\varphi+\alpha}$, $\vartheta_n = \frac{\ln(1-\alpha)}{\sigma(1-\alpha)+\varphi+\alpha}$, $\psi_{ya} = \frac{1+\varphi}{\sigma(1-\alpha)+\varphi+\alpha}$, and $\vartheta_y = \frac{(1-\alpha)\ln(1-\alpha)}{\sigma(1-\alpha)+\varphi+\alpha} = (1 - \alpha)\vartheta_n$.

Moreover, using the household's optimality condition B.6, the real interest rate as defined by the Fischer equation, $r_t = i_t - E_t\{\pi_{t+1}\}$, and the output path B.11, one can derive the equilibrium path of real interest rates:

$$\begin{aligned}
c_t &= E_t\{c_{t+1}\} - \frac{1}{\sigma} (i_t - E_t(\pi_{t+1}) - \rho) \\
\Leftrightarrow y_t &= E_t\{y_{t+1}\} - \frac{1}{\sigma} (r_t - \rho) \\
\Leftrightarrow r_t &= \sigma E_t\{\Delta y_{t+1}\} + \rho \\
\Leftrightarrow r_t &= \sigma \psi_{ya} E_t\{\Delta a_{t+1}\} + \rho
\end{aligned} \tag{B.12}$$

This is where the assumption of rational expectations is implicitly used. The “subjective” expectations of the household are taken to be equivalent to the “objective” expectations or predictions from the model itself, so as to allow the combinations and rearrangements made in B.12. The representative household does not systematically err.

Finally, combining optimality condition B.9 and the employment path B.10, the real wage, $\omega_t = w_t - p_t$, can be written as:

$$\omega_t = \psi_{\omega a} a_t + \vartheta_{\omega}, \tag{B.13}$$

where $\psi_{\omega a} = \frac{\sigma + \varphi}{\sigma(1-\alpha) + \varphi + \alpha}$ and $\vartheta_{\omega} = \frac{(\sigma(1-\alpha) + \varphi) \ln(1-\alpha)}{\sigma(1-\alpha) + \varphi + \alpha}$

B.2 Monetary Policy

We see that in the above model the real macroeconomic variables employment (B.10), output (B.11), real interest rates (B.12) as well as real wages (B.13) can all be written as functions of the exogenous technology process a_t . They are all independent of any nominal variables. There is thus monetary neutrality and as a consequence there is no such thing as an optimal monetary policy. All fluctuations of real variables are reduced to technology shocks.

Appendix C

Mathematical Compendium to the New Keynesian DSGE Model

In the following appendix, we present a formal canonical version of the standard New Keynesian dynamic stochastic general equilibrium (DSGE) model. The representation follows again Galí (2008, ch. 3). The characteristic assumptions of the New Classical model presented in appendix B are relaxed. Instead of perfect competition and price flexibility, we introduce imperfect or monopolistic competition with differentiated goods and staggered price setting in the sense of Calvo (1983).

C.1 The Model

Just like the baseline New Classical DSGE model, the New Keynesian version will be presented in three parts, the representative household, the representative firm, or rather many representative firms, and the general equilibrium conditions that are invoked in order to close and solve the model by combining the optimality conditions for firms and households.

C.1.1 The Representative Household

The optimization problem of the representative household is once again defined in terms of some utility function U

$$\max_{C_t, N_t} E_0 \sum_{t=0}^{\infty} \beta^t U(C_t, N_t), \quad (\text{C.1})$$

where the notation corresponds exactly to the one used in the previous appendix. C_t corresponds again to the consumption index of the differentiated goods produced by the firms.¹ It is formally defined as

$$C_t = \left(\int_0^1 C_t(i)^{1-\frac{1}{\varepsilon}} di \right)^{\frac{\varepsilon}{\varepsilon-1}}, \quad (\text{C.2})$$

where $C_t(i)$ is the quantity of good i consumed by the household in period t . It is produced by firm i , with i being an element of the continuum $[0, 1]$. There is thus an infinite number of differentiated goods produced by an infinite number of firms.

The household's budget constraint is thus slightly adjusted to the following form with $P_t(i)$ denoting the respective price of good i :

$$\int_0^1 P_t(i)C_t(i)di + Q_tB_t \leq B_{t-1} + W_tN_t - T_t. \quad (\text{C.3})$$

The household has to allocate consumption expenditures between the different goods. It has to maximize C_t for any given level of expenditure, denoted $Z_t = \int_0^1 P_t(i)C_t(i)di$. The corresponding Lagrangean to the optimization problem is thus

$$L = \left[\int_0^1 C_t(i)^{1-\frac{1}{\varepsilon}} di \right]^{\frac{\varepsilon}{\varepsilon-1}} - \lambda \left(\int_0^1 P_t(i)C_t(i)di - Z_t \right),$$

which leads to the following series of first order conditions:

$$C_t(i)^{-\frac{1}{\varepsilon}} C_t^{\frac{1}{\varepsilon}} = \lambda P_t(i), \quad \forall i \in [0, 1].$$

Combining the first order conditions for any two goods $i, j \in [0, 1]$ yields

$$C_t(i) = C_t(j) \left(\frac{P_t(i)}{P_t(j)} \right)^{-\varepsilon},$$

which can be inserted into the expenditure equation above:

$$\int_0^1 P_t(i)C_t(j) \left(\frac{P_t(i)}{P_t(j)} \right)^{-\varepsilon} di = Z_t.$$

Defining a general price index as $P_t = \left[\int_0^1 P_t(i)^{1-\varepsilon} di \right]^{\frac{1}{1-\varepsilon}}$ we can rewrite this last expression as follows:

¹We have already in the previous appendix called C_t an index. However, without loss of generality in the former setting, it could have simply been regarded as the quantity of the one good produced by the representative firm.

$$\begin{aligned}
C_t(j)P_t(j)^\varepsilon P_t^{1-\varepsilon} &= Z_t \\
\Leftrightarrow C_t(j) &= \left(\frac{P_t(j)}{P_t}\right)^{-\varepsilon} \frac{Z_t}{P_t}, \quad \forall j \in [0, 1].
\end{aligned} \tag{C.4}$$

For $i \in [0, 1]$ we can insert this last equation into the expression of the consumption index to obtain

$$\begin{aligned}
C_t &= \left[\int_0^1 \left(\left(\frac{P_t(i)}{P_t} \right)^{-\varepsilon} \frac{Z_t}{P_t} \right)^{1-\frac{1}{\varepsilon}} di \right]^{\frac{\varepsilon}{\varepsilon-1}} \\
\Leftrightarrow C_t &= Z_t P_t^{\varepsilon-1} \left[\int_0^1 P_t(i)^{1-\varepsilon} \right]^{\frac{\varepsilon}{\varepsilon-1}} \\
\Leftrightarrow C_t &= Z_t P_t^{\varepsilon-1} P_t^{-\varepsilon},
\end{aligned}$$

and hence,

$$C_t P_t = Z_t = \int_0^1 P_t(i) C_t(i) di, \tag{C.5}$$

the consumption expenditure can just be written as the product of the consumption and price indexes. Combining equations C.4 and C.5 we can derive the respective demand schedules, $\forall i \in [0, 1]$:

$$C_t(j) = \left(\frac{P_t(j)}{P_t}\right)^{-\varepsilon} C_t. \tag{C.6}$$

Plugging equation C.5 into the budget constraint C.3 we obtain a constraint that is formally identical to the one in the previous appendix

$$P_t C_t + Q_t B_t \leq B_{t-1} + W_t N_t - T_t,$$

and hence the expressions for labor supply and savings decisions are also identical. Under the assumption that the utility function has the form $U(C_t, N_t) = \frac{U_t^{1-\sigma}}{1-\sigma} - \frac{N_t^{1+\varphi}}{1+\varphi}$ we have once again the same log-linear versions of the optimality conditions for the household:

$$\sigma c_t + \varphi n_t = w_t - p_t, \tag{C.7}$$

$$c_t = E_t\{c_{t+1}\} - \frac{1}{\sigma}(i_t - E_t\{\pi_{t+1}\} - \rho). \tag{C.8}$$

If needed, these equations are supplemented with an *ad-hoc* money demand function as described in the previous appendix:

$$m_t - p_t = y_t - \eta i_t. \quad (\text{C.9})$$

C.1.2 The Representative Firms

The infinite number of consumer goods is produced by an infinite number of firms, $i \in [0, 1]$, all of which face an identical production function:

$$Y_t(i) = A_t N_t(i)^{1-\alpha},$$

with a common exogenous technology parameter A_t . Moreover, each firm faces an identical demand schedule C.6 and takes the aggregated values C_t and P_t as given. At this point we introduce one of the characteristic features of the New Keynesian DSGE model, namely staggered pricing in the sense of Calvo (1983). In any given period t a firm can only adjust its selling price with a probability of $1 - \theta$, where $\theta \in [0, 1]$ is a measure of price stickiness. The case $\theta = 0$ corresponds to the scenario of perfectly flexible prices outlined in the previous appendix.

Hence, in every period there will be a fraction $1 - \theta$ of firms adjusting their selling prices and a fraction θ that does not change selling prices. Since all the firms changing their price level in any given period face the same demand schedule and production function, their chosen price will be identical P_t^* . Given its definition we can thus write the price index as

$$P_t = \left[\int_{S(t)} P_{t-1}(i)^{1-\varepsilon} di + (1 - \theta) (P_t^*)^{1-\varepsilon} \right]^{\frac{1}{1-\varepsilon}},$$

where $S(t) \subset [0, 1]$ is the set of firms that does not change selling prices. The distribution of prices in this subset of firms corresponds to the distribution of effective prices in period $t - 1$, though with a smaller mass of θ . Hence,

$$P_t = [\theta (P_{t-1})^{1-\varepsilon} + (1 - \theta) (P_t^*)^{1-\varepsilon}]^{\frac{1}{1-\varepsilon}},$$

and dividing by P_{t-1} ,

$$\begin{aligned} \Pi_t &= \frac{1}{P_{t-1}} [\theta (P_{t-1})^{1-\varepsilon} + (1 - \theta) (P_t^*)^{1-\varepsilon}]^{\frac{1}{1-\varepsilon}} \\ \Leftrightarrow \Pi_t^{1-\varepsilon} &= \theta + (1 - \theta) \left(\frac{P_t^*}{P_{t-1}} \right)^{1-\varepsilon}. \end{aligned} \quad (\text{C.10})$$

Log-linearization around the steady state of zero price inflation, that is, $P_{t-1} = P_t = P_t^*$ and $\Pi_t = 1, \forall t$ yields

$$\pi_t = (1 - \theta)(p_t^* - p_{t-1}). \quad (\text{C.11})$$

We see that the inflation process in this scenario is determined by the optimization decision of the fraction of firms that can adjust their selling prices in any given period. In order to understand inflation the optimization problem of the price setting firms must be analyzed. They choose the price P_t^* such that the current market value of expected profits over the periods for which the chosen price remains effective is maximized.² For any period t , this fraction of firms solves the following optimization problem:

$$\max_{P_t^*} \sum_{k=0}^{\infty} \theta^k E_t \{ Q_{t,t+k} (P_t^* Y_{t+k|t} - \Psi_{t+k}(Y_{t+k|t})) \},$$

subject to³

$$Y_{t+k|t} = \left(\frac{P_t^*}{P_{t+k}} \right)^{-\varepsilon} C_{t+k}, \quad \forall k = 0, 1, 2, \dots,$$

where $Q_{t,t+k} \equiv \beta^k (C_{t+k}/C_t)^{-\sigma} (P_t/P_{t+k})$ is a discount factor of future nominal payoffs, $Y_{t+k|t}$ is the output in period $t+k$ of a firm that has last optimized its price in period t , and $\Psi_{t+k}(\cdot)$ describes the cost of production in period $t+k$.

The first order condition of this optimization problem are given by

$$\sum_{k=0}^{\infty} \theta^k E_t \{ Q_{t,t+k} Y_{t+k|t} (P_t^* - \mathcal{M} \psi_{t+k|t}) \} = 0,$$

where $\psi_{t+k|t} = \Psi_{t+k}(Y_{t+k|t})$ corresponds to the nominal marginal costs of production in period $t+k$ for a firm that has last reset its selling price in period t , and $\mathcal{M} = \frac{\varepsilon}{\varepsilon-1}$ is interpreted as the desired frictionless markup, since as θ approaches 0, that is, price flexibility increases, the above optimality condition collapses to $P_t^* = \mathcal{M} \psi_{t|t}$. This last equation can be divided by P_{t-1} in order to reformulate it in terms of variables that possess a steady state:

$$\sum_{k=0}^{\infty} \theta^k E_t \left\{ Q_{t,t+k} Y_{t+k|t} \left(\frac{P_t^*}{P_{t-1}} - \mathcal{M} M C_{t+k|t} \Pi_{t-1,t+k} \right) \right\} = 0.$$

²Since the probability of being able to change selling prices for any firm in any given period is $1 - \theta$, independently of past pricing decisions, the average duration for which a chosen price remains effective is $(1 - \theta)^{-1}$ periods.

³The constraints of the optimization problem invoke market clearing in every period. This means in other words that produced quantities are fully flexible to ensure equilibrium.

Here, $MC_{t+k|t} = \psi_{t+k|t}/P_{t+k}$ denotes the real marginal costs in period $t+k$, and $\Pi_{t-1,t+k} = P_{t+k}/P_{t-1}$.

In the steady state of zero inflation $P_t^* = P_{t+k} = P_{t-1}$, and hence, $\Pi_{t-1,t+k} = 1 = P_t^*/P_{t-1}$. Moreover, $Q_{t,t+k} = \beta^k$, $MC_{t+k|t} = MC = 1/\mathcal{M}$, and $Y_{t+k|t} = Y$, because all firms face the same demand schedule and production function and will thus produce the same quantities. A first order Taylor expansion around that steady state yields

$$p_t^* - p_{t-1} = (1 - \beta\theta) \sum_{k=0}^{\infty} (\beta\theta)^k E_t \{ \hat{m}c_{t+k|t} + (p_{t+k} - p_{t-1}) \}, \quad (\text{C.12})$$

$\hat{m}c_{t+k|t} = mc_{t+k|t} - mc$ is the logarithmic deviation of marginal costs from its steady state, and $mc = -\mu \equiv \log(\mathcal{M})$. We can thus rearrange the last equation as follow:

$$p_t^* = \mu + (1 - \beta\theta) \sum_{k=0}^{\infty} (\beta\theta)^k E_t \{ mc_{t+k|t} + p_{t+k} \}.$$

C.1.3 General Equilibrium Conditions

We can now combine the results derived above and the respective equilibrium conditions to obtain the key equations of the model. Equilibrium on the goods markets is given by

$$Y_t(i) = C_t(i), \quad \forall i \in [0, 1],$$

and on the aggregated level, if we define Y_t analogously to C_t by $Y_t = [\int_0^1 Y_t(i)^{1-\frac{1}{\epsilon}} di]^{\frac{\epsilon}{\epsilon-1}}$, we have $Y_t = C_t$. Combining equation C.8 with the condition for the goods market equilibrium yields

$$y_t = E_t\{y_{t+1}\} - \frac{1}{\sigma}(i_t - E_t\{\pi_{t+1}\} - \rho). \quad (\text{C.13})$$

Equilibrium on the labor market is given if labor supply N_t by the representative household equals the total labor demand by the representative firms, that is, if

$$N_t = \int_0^1 N_t(i) di.$$

Inserting the firm's production function yields

$$N_t = \int_0^1 \left(\frac{Y_t(i)}{A_t} \right)^{\frac{1}{1-\alpha}} di.$$

Applying the goods market clearing condition and the household's demand schedule we can rewrite this relationship as

$$N_t = \left(\frac{Y_t}{A_t}\right)^{\frac{1}{1-\alpha}} \int_0^1 \left(\frac{P_t(i)}{P_t}\right)^{\frac{-\varepsilon}{1-\alpha}} di,$$

which can be approximated in logarithmic terms as⁴

$$y_t = a_t + (1 - \alpha)n_t. \quad (\text{C.14})$$

The next step is the derivation of an equation expressing the relationship between an individual firm's marginal costs and the average real marginal costs. The average real marginal costs in the economy are defined as

$$mc_t = (w_t - p_t) - mpn_t,$$

where mpn_t is the average marginal product of labor, $mpn_t = \log(1 - \alpha) + a_t - \alpha n_t$, derived from the production function of the firms. Hence,

$$mc_t = (w_t - p_t) - (a_t - \alpha n_t) - \log(1 - \alpha),$$

and using equation C.14

$$mc_t = (w_t - p_t) - \frac{1}{1 - \alpha}(a_t - \alpha y_t) - \log(1 - \alpha).$$

Accordingly,

$$\begin{aligned} mc_{t+k|t} &= (w_{t+k} - p_{t+k}) - mpn_{t+k|t} \\ &= (w_{t+k} - p_{t+k}) - \frac{1}{1 - \alpha}(a_{t+k} - \alpha y_{t+k|t}) - \log(1 - \alpha), \end{aligned}$$

and hence,

$$mc_{t+k} = mc_{t+k|t} + \frac{\alpha}{1 - \alpha}(y_{t+k|t} - y_{t+k}).$$

From the household's demand schedule and goods market clearing it follows that

$$mc_{t+k} = mc_{t+k|t} - \frac{\alpha\varepsilon}{1 - \alpha}(p_t^* - p_{t+k}). \quad (\text{C.15})$$

⁴The term $(1 - \alpha)\log\left(\int_0^1 P_t(i)/P_t^{\frac{-\varepsilon}{1-\alpha}} di\right)$ is approximately 0 in the neighborhood of the zero inflation steady state.

As a next step, we can combine equations C.15 and C.12. Defining $\Theta \equiv \frac{1-\alpha}{1-\alpha+\alpha\varepsilon} \leq 1$ and rearranging terms we obtain

$$p_t^* - p_{t-1} = (1 - \beta\theta)\Theta \sum_{k=0}^{\infty} (\beta\theta)^k E_t \{\hat{m}c_{t+k}\} + \sum_{k=0}^{\infty} (\beta\theta)^k E_t \{\pi_{t+k}\}.$$

The discounted sum can be rewritten as a difference equation

$$p_t^* - p_{t-1} = \beta\theta E_t \{p_{t+1}^* - p_t\} + (1 - \beta\theta)\Theta \hat{m}c_t + \pi_t. \quad (\text{C.16})$$

We then combine the equation for aggregate price dynamics C.11 and equation C.16 to obtain

$$\pi_t = \beta E_t \{\pi_{t+1}\} + \lambda \hat{m}c_t, \quad (\text{C.17})$$

with $\lambda \equiv \frac{(1-\theta)(1-\beta\theta)}{\theta}\Theta$.

As a next step, we derive an equation relating marginal costs and economic activity in the model economy. Using the household's optimality condition, we can write

$$\begin{aligned} mc_t &= (w_t - p_t) - mpn_t \\ &= (\sigma y_t + \varphi n_t) - (y_t - n_t) - \log(1 - \alpha), \end{aligned}$$

and using equation C.14

$$mc_t = \left(\sigma + \frac{\varphi + \alpha}{1 - \alpha} \right) y_t - \frac{1 + \varphi}{1 - \alpha} a_t - \log(1 - \alpha). \quad (\text{C.18})$$

As shown above with flexible prices ($\theta = 0$) the marginal costs are constant $mc = -\mu$. We define the *natural rate of output*, y_t^n , as the level of output obtained under fully flexible prices. It satisfies the following equation

$$mc = \left(\sigma + \frac{\varphi + \alpha}{1 - \alpha} \right) y_t^n - \frac{1 + \varphi}{1 - \alpha} a_t - \log(1 - \alpha), \quad (\text{C.19})$$

which can be rewritten as

$$y_t^n = \psi_{ya}^n a_t + \vartheta_y^n, \quad (\text{C.20})$$

where $\psi_{y\alpha}^n \equiv \frac{1+\phi}{\sigma(1-\alpha)+\phi+\alpha}$ and $\vartheta_y^n \equiv -\frac{(1-\alpha)(\mu-\log(1-\alpha))}{\sigma(1-\alpha)+\phi+\alpha} > 0$.⁵ The higher μ , which we can interpret as a measure for market power, the lower the path of natural output. The impact of technological developments on the natural output is not affected by market power of firms.

Dividing equation C.19 from equation C.18 yields

$$\hat{m}c_t = \left(\sigma + \frac{\varphi + \alpha}{1 - \alpha} \right) (y_t - y_t^n). \quad (\text{C.21})$$

The deviation of the output level from the natural rate of output is called the *output gap* and will be denoted \tilde{y}_t . It is thus proportional to the gap between real marginal costs and its steady state level.

The last equation can be combined with equation C.17 to derive the *New Keynesian Phillips curve* (NKPC):

$$\pi_t = \beta E_t\{\pi_{t+1}\} + \kappa \tilde{y}_t, \quad (\text{C.22})$$

with $\kappa \equiv \lambda \left(\sigma + \frac{\varphi + \alpha}{1 - \alpha} \right)$.

The next important equation, the so-called *dynamic IS equation* (DIS) is obtained by rewriting C.13 in terms of the output gap:

$$\tilde{y}_t = E_t\{\tilde{y}_{t+1}\} - \frac{1}{\sigma} (i_t - E_t\{\pi_{t+1}\} - r_t^n). \quad (\text{C.23})$$

In this last equation we have the *natural rate of interest*, r_t^n , defined as $r_t^n \equiv \rho + \sigma E_t\{\Delta y_{t+1}^n\} = \rho + \sigma \varphi_{y\alpha}^n E_t\{\Delta a_{t+1}\}$.

The DIS and NKPC equations are central building blocks of the New Keynesian DSGE model.

C.2 Monetary Policy

The final model has a simple structure. Price inflation rates, π_t , are determined by the New Keynesian Phillips curve, i.e. equation C.22, as a function of the output gap, \tilde{y}_t , and expected price inflation for the next period, $E\{\pi_{t+1}\}$. The dynamic IS equation, i.e. equation C.23, determines the output gap as a function of the actual real rate of interest, $i_t - E_t\{\pi_{t+1}\}$, and the natural rate of interest, r_t^n . More precisely, the difference between the actual rate and the natural

⁵Notice that in the case of perfect competition, that is, the markup equals zero ($\mu = 0$), the equation C.20 for the natural level of output corresponds exactly to equation B.11 for the equilibrium level of output under fully flexible prices derived in the previous appendix.

rate determines the output gap. We can thus see that the equilibrium paths of real magnitudes in the model are partly determined by nominal magnitudes. Monetary policy has an impact, most notably, through the determination of the nominal interest rates. The dynamics of monetary policy rules within the model are discussed in the main text in section 5.2.1 in chapter 5, starting on page 180.

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Thèse de Doctorat

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The Costs and Benefits of Central Banking

Des coûts et des avantages de l'existence des banques centrales

Résumé

L'évaluation des coûts et des avantages du système de banque centrale dépend fondamentalement de l'approche analytique adoptée. Les approches instrumentalistes et positivistes, discutées dans la première partie de cette thèse, se concentrent principalement sur l'importance des prédictions empiriques quantitatives. Ces approches tendent à mettre l'accent sur les conséquences immédiates et facilement mesurables. Les effets bénéfiques de court-terme des politiques monétaires expansionnistes sont ainsi étudiés en détail par les partisans de ces approches. Les critiques internes de ces dernières sont basées sur la reconnue ignorance des potentiels effets secondaires déstabilisateurs. Les coûts engendrés par les banques centrales sont vues principalement comme une perte de bien être due à une activité économique diminuée où une inflation des prix trop élevée résultant d'une déviation vis-à-vis de la politique monétaire optimale.

L'approche causale-réaliste, étudiée dans la deuxième partie, donne un rôle plus restreint à la méthode empirique quantitative. Cette ligne de recherche permet une extension fructueuse de l'analyse coûts bénéfiques à des notions plus abstraites qui peuvent être étudiées qualitativement tels le risque systémique et l'aléa-moral. Cette étude défend que le régime moderne de banque centrale a alimenté le développement de ces deux phénomènes. Nous analysons comment la récurrence du cycle économique et la redistribution de la richesse au profit des plus aisés sont des effets importants de l'expansion du crédit. Une explication alternative de la stagflation basée sur le processus de l'intervention politique en réponse à ces conséquences inattendues est proposée.

Mots clés

banque centrale ; théorie monétaire ; politique monétaire ; cycle économique ; politique conjoncturelle ; risque systémique ; aléa moral ; redistribution des richesses

Abstract

The evaluation of the costs and benefits of central banking is to a considerable degree dependent on the analytical approach pursued. Instrumentalist-positivist approaches, as discussed in the first part of the thesis, emphasize the importance of empirical-quantitative predictions for the development of economic models. Within this line of research, there exists a strong emphasis on those consequences of central banking that have an immediate and readily measurable empirical counterpart in the observed economic environment. Hence, the beneficial short-term effects of expansionary monetary policy in terms of increased economic activity are studied in detail and are well understood by proponents of these approaches. Internal counterarguments are based on acknowledged ignorance about potentially destabilizing side-effects. The costs of central banking are essentially seen as welfare losses from decreased economic activity or excessive price inflation as results of deviations from an optimal monetary policy path.

The causal- realist approach, as studied in the second part, assigns a narrower role to quantitative-empirical tools of analysis. This line of research allows for a fruitful extension of the cost-benefit analysis to more abstract notions such as systemic risk and moral hazard that can be analyzed qualitatively. It is argued that modern central banking is a contributing factor to both. Moreover, business cycles and wealth redistribution from bottom to top as important effects of credit expansion are analyzed. An alternative explanation of stagflation based on the process of political interventionism in response to unintended consequences of monetary policy is proposed.

Key Words

central bank ; monetary theory ; monetary policy ; business cycle ; countercyclical policy ; systemic risk ; moral hazard ; redistribution of wealth