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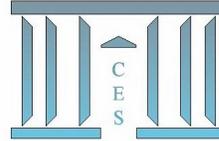
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**Econometrics as a Pluralistic Scientific Tool for Economic  
Planning: On Lawrence R. Klein's Econometrics**

Erich PINZÓN FUCHS

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# **Econometrics as a Pluralistic Scientific Tool for Economic Planning: On Lawrence R. Klein's Econometrics**

*Erich Pinzón Fuchs*<sup>†</sup>

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## **Abstract**

Lawrence R. Klein (1920-2013) played a major role in the construction and in the further dissemination of econometrics from the 1940s. Considered as one of the main developers and practitioners of macroeconometrics, Klein's influence is reflected in his application of econometric modelling "to the analysis of economic fluctuations and economic policies" for which he was awarded the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel* in 1980. The purpose of this paper is to give an account of Klein's image of econometrics focusing on his early period as an econometrician (1944-1950), and more specifically on his period as a Cowlesman (1944-1947). Independently of how short this period might appear, it contains a set of fundamental publications and events, which were decisive for Klein's conception of econometrics, and which formed Klein's unique way of doing econometrics. At least four features are worth mentioning, which characterise this uniqueness. First, Klein was the only Cowlesman who carried on the macroeconomic programme beyond the 1940s, even if the Cowles had already abandoned it. Second, his pluralistic approach in terms of economic theory allowed him not only to use the Walrasian framework appraised by the Cowles Commission and especially by T.C. Koopmans, but also the Marxian and Keynesian frameworks, enriching the process of model specification and motivating economists of different stripes to make use of the nascent econometrics. Third, Klein differentiated himself from the rigid methodology praised at Cowles; while the latter promoted the use of highly sophisticated methods of estimation, Klein was convinced that institutional reality and economic intuition would contribute more to econometrics than the sophistication of these statistical techniques. Last but not least, Klein never gave up what he thought was the political objective of econometrics: economic planning and social reform.

**Keywords:** Economic Epistemology, History of Econometrics, History of Macroeconomic Modelling, Pluralism in Econometrics, Lawrence R. Klein, Cowles Commission.

**JEL Classification:** B23, B31, B41

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## **Econometrics as a Pluralistic Scientific Tool for Economic Planning: On Lawrence R. Klein's Econometrics<sup>2</sup>**

“Contributions from economics, mathematics, and statistics...at the Cowles were obviously important, but I feel that my colleagues wanted results that were quite robust, in small confidence areas, and very discriminating among competing hypotheses. Quantitative economics is not like that. It is inelegant, very tedious, very repetitive, and capable of forward movement in small increment. I admired the elegant theorems that my associates produced, but [these] seemed to me...very strong and not very realistic. I felt that if one paid unusual attention to data – very much in the painstaking tradition of Simon Kuznets – replicated analyses regularly, looked at more detail for the economy, learned as much as possible about realistic economic reaction, and stayed in touch with the economic situation on a daily basis that it would be possible to use econometric models for guidance, both in the fields of policy application and in pure understanding of the economy”

*Lawrence R. Klein, 1991*

### **I. Introduction**

Lawrence R. Klein (1920-2013) played a major role in the construction and in the further dissemination of econometrics from the 1940s. Considered as one of the main developers and practitioners of macroeconometrics (Bjerkholt, 2014a; Mariano, 2008), Klein's influence is reflected in his application of econometric modelling “to the analysis of economic fluctuations and economic policies” for which he was awarded the *Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel* in 1980. At a first glance, Klein's affiliation to the Cowles Commission during the second half of the 1940s would suggest a close attachment from his part to the Commission's views about econometrics and to its methods. A more detailed scrutiny shows, however, that Klein did not partake of exactly the same *image* of econometrics of the other members of the Cowles<sup>3</sup>. Contrary to his fellow Cowlesmen, Klein did not envisage econometrics as an elegant and outrageously rigorous discipline, but as a sometimes tedious, repetitive and messy scientific practice, which involved painstaking analysis of data and economic reality, and which provided “guidance, both in the fields of policy application and in

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<sup>2</sup> This article was presented at the 46<sup>th</sup> UK HET Conference in London in September 2014. I am grateful to the participants there and especially to Geoffrey Harcourt and to Shobhana Madhavan for their comments and suggestions.

<sup>3</sup> To give an account of Klein's way of doing econometrics I will make use of Leo Corry's (1989; 2008) framework of image and body of knowledge. Rather than focusing on Klein's theoretical or technical contributions to econometrics, I will focus on the way Klein saw econometrics; how he conceived the practice of macroeconomic modelling; which elements should be emphasised, in order to improve the results; what should be the ultimate political purpose of econometrics; in short, what was Klein's image of knowledge about econometrics. For other applications of Corry's framework in economics see Weintraub (2002) and Giocoli (2003).

[the] pure understanding of the economy” (Klein, 1991). The purpose of this paper is to give an account of Klein’s particular image of econometrics focusing on his early period as an econometrician (1944-1950), and more specifically on his period as a Cowlesman (1944-1947). Independently of how short this period might appear, it contains a set of fundamental publications and events, which were decisive for Klein’s conception of econometrics, and which formed Klein’s unique way of doing econometrics<sup>4</sup>.

In what follows, I will discuss two features, which characterise Klein’s unique way of doing econometrics. First, his pluralistic approach in terms of economic theory, which allowed him not only to use the Walrasian framework appraised by the Cowles Commission and especially by T.C. Koopmans, but also the Marxian and Keynesian frameworks, enriching the process of model specification and possibly motivating economists of different stripes to get into the nascent econometrics. Klein did not restrict the construction of his econometric models only to one kind of economic theory. He demonstrated that it was possible to get the same econometric model departing from different theories (Klein, 1947b); but he also showed that several economic hypotheses stemming from different economic theories could prove useful when specifying an econometric model (Klein, 1950). Second, Klein differentiated himself from the rigid methodology praised at Cowles, which promoted the use of highly sophisticated statistical methods of estimation. Klein, on the contrary, was convinced that the use of a priori information, institutional reality and economic intuition would contribute more to econometrics than the further development of these sophisticated statistical techniques of estimation (Klein, 1960).

There are other aspects that characterized Klein’s particular image of econometrics that I will not be able to discuss here at length, but that should be kept in mind, for they are implicit in most of Klein’s work. First, Klein was the only Cowlesman who carried on the macroeconometric programme beyond the 1940s, even if the Cowles had already abandoned it (Epstein, 1987; Louçã, 2007; Bjerkholt, 2014b). This point illustrates Klein’s persistence and optimism towards econometrics, as a scientific tool that would provide sound guidance for policy makers. Second, Klein never gave up what he thought was the political objective of econometrics: economic planning and social reform. For Klein, econometrics was not only a tool for measuring economic variables, understanding and discovering the functioning of the economy, or rendering economics more scientific; for him, econometrics was a sound tool providing guidance for the objective of economic planning and social reform.

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<sup>4</sup> For other accounts of Klein’s life and work, see Bjerkholt (2014a), Klein and Mariano (1987), Bodkin, Klein and Marwah (1991), Mariano (2008), Klein (1980), Ball (1981), and Daza and D. Klein (2013a).

In short, Klein's image about the relation between econometrics and economic theory is quite peculiar when compared to the images of other members of the Cowles, notably when compared to the images of Marschak and Koopmans. On the one hand, while the Cowles Commission defended what Mirowski (2012) has called the "Cowles Creed", "that Walrasian general equilibrium theory is the only game in town" (p. 160), Klein offers a pluralistic approach, making use of different (and even contradictory) theories in order to build his econometric models. In the first section I will compare the Cowles's official image of econometrics, i.e. the Cowles Creed, with Klein's pluralistic image. I will show that Klein's peculiarity resides on the way he built his macroeconometric models. In the second section I will give a more detailed discussion about Klein's treatment of the problem of multiple hypotheses, of his criticism about the sophisticated methods of estimation, and finally, about his own methodological approach to econometrics, namely, his "methodological structuralism" (Nell and Errouaki, 2013).

## **I. Klein's Pluralism and the "Cowles Creed"**

Klein arrived at the Cowles Commission in November 1944. Jacob Marschak had used the occasion of the Econometric Society meeting in Cleveland to personally meet and recruit him (Bjerkholt, 2014a, p. 48). With only 24 years of age and with a recently acquired PhD degree at the MIT, he was one of the "prodigies" at Cowles (*ibid*)<sup>5</sup>. His thesis supervisor, Paul A. Samuelson, showed more than high esteem for his first PhD student in a letter of recommendation sent to Marschak:

"He is certainly the best student that I have had, and I have known very few better ones...[Klein is a] very promising, able, young economist with an excellent training. To a surprising degree he has been able to go ahead on his own steam in these disorganizing years" (Samuelson to Marschak, October 28, 1944, quoted by Bjerkholt, 2014a, p. 5).

Klein arrived at the Cowles Commission as one of the youngest researchers and with quite good credentials and recommendations. He was very enthusiastic and his humbleness and kindness, which characterised him throughout his whole life (Marquez, 1985; Mariano, 2007), rapidly allowed him to integrate to the Cowles's highly dialogical and demanding environment.

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<sup>5</sup> Klein was the second youngest researcher at Cowles, since there was also Hermann Rubin "the prodigy at Cowles" who was surprisingly only 17 years old in 1944 and already an active member of the Commission (Bjerkholt, 2014b, p. 47).

“[M]y collaborators and myself [Marschak] have found in him a person prepared to understand and appreciate the other point of view; equally agreeable in giving and in taking; and more interested in having the problem solved than in winning the argument or making career” (Marschak’s confidential statement for the Social Science Research Council, 1945, quoted by Bjerkholt, 2014a).

Klein was assigned a task that was quite different to the work he had been doing for his PhD thesis, *The Keynesian Revolution*. Although *The Keynesian Revolution* (Klein, 1947a) was a critical book of the theoretical and methodological grounds of Keynes’s works, but principally, of *The General Theory*, in which Klein barely made use of econometrics<sup>6</sup>, he was recruited at the Cowles to remake Tinbergen’s (1939) macroeconomic model of the United States. Mirowski (2012) suggests that Klein might not have known very well the work of Tinbergen, but he rapidly became an admirer of it<sup>7</sup>.

Politically, Klein was left wing oriented and he was perhaps more radical than many of his Cowles fellows (Mirowski, 2012; Bjerkholt, 2014b). During his first years at Cowles he was a member of the Communist party (Bjerkholt, 2014a; Daza et al., 2013; Mariano, 2008); a situation, which brought him several difficulties at the end of the 1940s inside the Commission<sup>8</sup> (Bjerkholt, 2014a; Mirowski, 2012; Louçã, 2007). Klein was, then, a particular fellow at Cowles. His particularity, however, does not only have to do with his academic and political backgrounds, but it also has to do with his image about economic theory and econometrics, and with his practices as an econometrician.

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<sup>6</sup> Klein’s critical position towards Keynes can be found throughout the book in quotations like the following:

“Keynes did not really understand what he had written, and chose the wrong thing to publicize as his innovation” (Klein, 1947b, p. 83).

“Keynesian economics gives us a set of tools with which to work on the unemployment problem, but it does not deal at all with many other important socio-economic questions that also deserve a large share of our attention and study” (ibid, p.186).

In terms of his barely use of econometrics, Klein mentions in some parts of his book that he had undertaken some regressions to see if the data fitted with the hypotheses, but he does not give more detailed information.

<sup>7</sup> A question that is left open here for further scrutiny is how did Klein conceive Keynes’s (1939) criticism of Tinbergen’s work and what was his position on this issue.

<sup>8</sup> His political views also proved troublesome in the mid-1950s, when Klein decided to leave the United States “for the peace and academic freedom of Oxford” (Klein in Breit and Hirsch 2004, quoted by Mariano, 2008, p. 5).

“The University of Michigan was to promote Klein to full professorship but then reneged when Klein testified in a Detroit hearing that he had been a member of the Communist Party for about six months in 1946” (Mariano, 2008, p. 5).

The first particularity that I would like to bring to the fore has to do with Klein's pluralistic approach to economic theory. Contrary to the Cowles Commission, Klein showed himself throughout his life and especially throughout the years between 1944-50, as an econometrician willing to confront all the plausible available theories with data. For Klein, as I understand, it is not a matter of being a Keynesian, a Marxian, or a Neoclassical, whatever this might mean. Abstract theories, of all kinds, provide fruitful and pedagogic ways of understanding some of the relationships between the variables studied. They provide an idea of the working of the economy, but only its confrontation with data can establish the usefulness of the particular theory (or hypothesis) in the model specification activity (I will come back to this issue in the second section of this paper). But, how does this position contrast with the Cowles's image of econometrics? In order to answer this question I will discuss two important events that happened at the Cowles Commission between 1946 and 1947: (1) the *Measurement without Theory* controversy, and (2) the rejection, by Koopmans and Marschak of a paper written by Klein, where he clearly defends his pluralistic approach.

*The methodological and political issues behind the Measurement without Theory Controversy*

The *Measurement without Theory Controversy* provides an example of the Cowles's official defence of Walrasian theory<sup>9</sup>. In his review of Burns and Mitchell (1946), Koopmans (1947) makes an analogy between the stages of development of economic theory and the stages of development of astronomy, i.e. between what he calls the *Kepler* and the *Newton* stage of development of a determinate science. The Kepler stage of development of any science would consist on a phase where researchers are (still) concerned with the discovery of the most fundamental relations between the variables, in order to understand the observed phenomena. Any discipline that would find itself in this stage would need of a great deal of purely observational and descriptive work. On the contrary, if a scientific field had reached the Newton stage of development, this would mean that there would exist a fundamental and general theory that would count as an explanation of the most "elementary and general" relations between the variables studied.

“The conviction that this ‘law’ [Newton’s law of Gravitation] is in some sense more fundamental, and thus constitutes progress over the Kepler stage, is due, I believe, to its being at once more elementary and more general. It is more elementary in that a simple property of mere matter is postulated. As a result, it is more general in that it

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<sup>9</sup> For detailed accounts of the *Measurement without Theory Controversy*, see Morgan (1990) and P. Epstein (1999). See also Koopmans (1947) and Vining (1949) for the original texts of the controversy.

applies to all matter, whether assembled in planets, comets, sun or stars, or in terrestrial objects – thus explaining a much wider range of phenomena” (Koopmans, 1947, p.161).

Burns and Mitchell – and the researchers at the National Bureau of Economic Research (NBER) – would be in the Kepler stage, while the Cowles Commission (which based its studies in the Walrasian framework) would be in the Newton stage of development.

“It appears to be the intention of Burns and Mitchell – in any case it is the opinion of the present reviewer – that their book represents an important contribution to the ‘Kepler stage’ of inquiry in the field of economics<sup>10</sup>” (Koopmans, 1947, pp. 161-162).

This means that the researchers of the NBER were still immersed in the task of searching for the establishment of the most empirical regularities between the economic variables. The Cowles’s researchers, in contrast, had already solved this problem, and they knew which were the most fundamental relations underlying economic variables and phenomena. The Walrasian general-equilibrium framework had provided them with that answer, and so, the Cowles did not have the necessity of dealing with the problem of *multiple hypotheses*. There was only one type of hypothesis that one could take for granted in the building of econometric modelling: this type of hypotheses stemmed from Walrasian economics<sup>11</sup>.

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<sup>10</sup> Two things are worth noticing here. First, Koopmans had received an academic formation as a physicist, not as an economist. He even developed a theorem during his very early years as a natural scientist, which rendered him well known in quantitative chemistry: *the Koopmans theorem*. Second, it is also of interest to notice that R. Vining (the young NBER standard bearer in the Controversy) did not refute Koopmans’s analogy between the stages of development in economics and astronomy. Even if he did not accept that Walrasian theory could be treated as the equivalent of Newton’s theory in economics, he did neither refute comparing economics to the natural sciences, nor did he refute the notion of progress underlying the Kepler and the Newton stage framework. This position is quite representative of the image of economics of the time, consisting on the consideration that economics should more and more converge to the natural sciences in order to “progress”. Nevertheless, the classification of Newton’s Law of Gravitation as “the most fundamental” and applying “to all matter” would not be very accurate from the perspective of the natural scientists, since Quantum Mechanics had, by 1947, already “revolutionised” physics and our understanding of matter.

<sup>11</sup> Qin (1993) provides a quite different story about the problem of multiple hypotheses and of the underlying reasons explaining why the Cowles disregarded this problem.

“Since the central task of the Cowles Commission was to formalize the statistical methods applicable for econometric analyses, given economic theory, they consciously left open the issue of how to put particular economic theory into a particular structural model” (Qin, 1993, p. 63).

I think that Qin misses the point here, because for the Cowles there was no doubt that the “given economic theory” could be no other than the Walrasian general-equilibrium framework. They did not

Klein's position towards the controversy is also worth mentioning, for, although he did not really play an active role in it, he felt some of the consequences of the controversy in his short stay at the NBER, in 1948. For him, for instance, the controversy was not exclusively methodological, but the political element also played an important role<sup>12</sup>.

“As a visiting staff member of the National Bureau during 1948-49, I could sense the tension in the dispute over methodology. It was not purely methodological, however. A central issue was that we members of the Cowles Commission were seeking an objective that would permit state intervention and guidance for economic policy, and this approach was eschewed by...the National Bureau” (Klein, 1991, p. 112).

Although closer to the Cowles than to the Bureau in political terms, in terms of methodology, Klein very much admired the work of the NBER and appreciated the fact that researchers such as Kuznets (and presumably Burns and Mitchell) attributed so much importance to the role of data in the analysis of the business cycle.

“I felt that if one paid unusual attention to data – very much in the painstaking tradition of Simon Kuznets – ... that it would be possible to use econometric models for guidance, both in the fields of policy application and in pure understanding of the economy” (Klein, 1991).

Klein felt that the main dispute between the NBER and the Cowles Commission had to do with their distinct political positions rather than with methodological issues. While the NBER was closer to the Department of Economics of the University of Chicago, and hence, was more reticent in the promotion of governmental intervention, the members of the Cowles Commission were more left wing oriented and so, they would promote government intervention to steer the economy. This, however, does not mean that the Cowlesmen were Keynesians (Mirowski, 2012), but just that they favoured intervention.

But intervention alone (be it inspired by Keynes or Tinbergen) would not satisfy Klein. For Klein thought that the social and economic problems were so profound that they had to be resolved at its roots. In his 1947 book – *The Keynesian Revolution* – Klein described, in general terms, “a practical program[me] of economic policy ... necessary in order to *reform capitalism* to a system of full employment” (Klein, 1947b, p. 168, my emphasis). This programme had a Marxian (and not a Keynesian) flavour, since it was Marx's ultimate aim (and not Keynes's) which would satisfy Klein's image about

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leave the issue open, but they closed it before one could even think about it (see the letter that I quote in page 9 from Koopmans to Marschak).

<sup>12</sup> A third aspect worth of analysis in this controversy is the battle for funding between both institutions, treated in more detail by Mirowski (2002; 2012).

intervention. For Klein, Marx's aim was to "analy[s]e the reasons why the capitalist system could not function properly, while Keynes analy[s]ed the reasons why the capitalist system did not but could function properly. Keynes wanted to apologi[s]e and preserve, while Marx wanted to criticize and destroy" (ibid, p. 131). For Klein the positions of Marx and Keynes were opposed; the former would be a revolutionary and the latter a reformer. Although Klein favoured income distribution policies, he demonstrated, following Marxian arguments, that this policy would not be sufficient "to insure that capitalism will always provide uninterrupted full production and employment" (Klein, 1947a, p. 131).

"Full-employment planning (functional finance or compensatory fiscal policy) is not enough" (Klein, 1948, "The case of Planning", quoted by Mirowski, 2012, p. 149).

"Complete planning leads generally to a higher level of welfare than perfect competition even in the case where wealth redistribution is permitted in the latter system" (ibid, p. 149).

*The way to sell econometrics to anybody is first to sell modern economic theory, then to sell econometric methods*

In 1946 Klein had written a paper where he attempted at a comparison of Walrasian, Keynesian and Marxian theories of effective demand, in order to show that one could arrive to the same conclusions and build the same model, starting from any of the three theories<sup>13</sup>. This procedure, however, did not please Koopmans who advised Marschak not to publish that paper under the name of the Cowles Discussion Paper series. Klein, Koopmans argued, was not exclusively using "modern theories of economic behavio[u]r", i.e. Walrasian general equilibrium theory, and this argument was enough for rejecting Klein's paper:

"This paper [*Theories of effective Demand and Employment*] is an attempt to sell the idea of econometric model building to adherents of Marxian economic doctrine. I shall explain in these comments why I believe that *such attempts*, including the present one, *are harmful to the objectives of econometric model building*. The main reason is that econometric research of the type in which we are engaged in is essentially based on modern theories of economic behavio[u]r. *The way to sell it to anybody, including Marxian economists, is first to sell modern economic theory, then to sell econometric methods*. There are no short cuts" (Koopmans to Marschak, memo December 10, 1946, box 148, folder Klein, Jacob Marschak's Papers,

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<sup>13</sup> In fact the original title of the paper was "Marxian Theory of Effective Demand", and in its first version the paper did not include the Keynesian framework (Bjerkholt, 2014b).

quoted in Mirowski, 2002, p. 247, my emphasis).

Koopmans's position shows that the Cowles's aim was first to promote Walrasian theory, and then econometrics. It would make no sense to promote econometrics without the Walrasian framework, since only that particular framework would act as warrantor of an accurate use of econometrics. This means, that econometrics would be only then dealing with the most advanced "modern economic theory" and not with any other kind of forgotten pseudo-theory. In short, the Cowles Commission did not only seek to provide a powerful scientific tool for economists, but to promote the Walrasian framework and then to reinforce this framework by means of econometrics. Klein's image and method was, under these lines, not always in accordance with the aims and methods of the Cowles.

The refusal of Klein's paper and Koopmans's position in the Controversy not only show that the Commission was advocating for the promotion of Walrasian theory, but it also points to the divergences between Klein and the Cowles. On the one hand, the refuted paper is an indication of Klein's characteristic pluralism, which does not always seem to fit very well with the rigid position of the Cowles towards economic theory. On the other hand, Koopman's definition of the Walrasian framework as the most advanced and complete economic theory, clearly contrasts with the more flexible and pragmatic approach appraised by Klein, characterised by his use of economic hypotheses, stemming from different economic theories to complete and estimate his models. The second section of this paper will attempt to give a more detailed account of Klein's image of econometrics and of his methodology.

## **II. Klein and the Building of Macroeconometric Models**

### *Two ways of dealing with multiple hypotheses at the Cowles Commission*

I have mentioned that, for the Cowles, the problem of multiple hypotheses was already solved, since economics counted with the most general and fundamental theory explaining economic phenomena: Walrasian general equilibrium theory. The real problem to which the Cowles was confronted in its econometric venture was, as Mirowski (1989) puts it, to verify "the validity of *neoclassical* theory, and not all theory *tout court*". Their task as econometricians would primarily consist on the testing of the neoclassical hypotheses, very much in the spirit of some early econometricians of the 1910s, such as Lenoir or Lehfeltdt who were occupied in testing whether "the assumptions in the theoretical demand model were actually fulfilled in the statistical data" (Morgan, 1990, p. 141), but several years after them and armed with more sophisticated statistical and mathematical techniques.

Inside the Cowles, however, there was another way of dealing with the problem of multiple hypotheses. Klein, who regarded the question of multiple hypotheses as anything but a problem, led this other alternative way of dealing with multiple hypotheses. In fact, he seemed to be willing to make his readers aware of the existence of multiple hypotheses, and then to make the most of it in order to build his econometric models.

“It is desired to impress upon the reader that the models of this volume are put forth in full knowledge of the existence of the problem of multiplicity of hypotheses” (Klein, 1950, p. 122).

I come back to Klein’s 1946 paper (rejected by Marschak and Koopmans), *Theories of Effective Demand and Employment*. This paper, published one year later, in 1947, in the *Journal of Political Economy*, is a good example of the way Klein made use of different economic theories. The paper was about the possibility of building the same econometric model on the bases of any of the following theories: Neoclassical, Marxian or Keynesian<sup>14</sup>. Three years later, in 1950, when his first celebrated econometric book appeared, Klein insisted on his pluralistic way of building macroeconomic models:

“Many economists will recognize the resemblance between the three-equation model [Klein’s celebrated Model I], Kalecki’s models of the business cycle, and some of the doctrines of Marxist economics. The model could actually be called a Marxian theory of effective demand. It is possible to develop this model, as we have shown, from the un-Marxian principles of utility and profit maximization [Klein, 1950], but it is also possible to develop this model from purely Marxian principles [Klein, 1947a]. The same model can be consistent with a multiplicity of hypotheses. The problem of developing models from Marxian principles is of great interest from the point of view of the history of economic thought, but is not an essential problem of this book, which is concerned mainly with quantifying a true description of the structure of [the] United States economy. We mention this relation only in passing, as a point of general interest<sup>15</sup>” (Klein, 1950, pp. 63-64).

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<sup>14</sup> De Vroey and Malgrange (2009) present serious doubts about the Keynesian spirit of Klein’s models. In particular, they argue that Klein’s “claim that he empirically demonstrated the validity of Keynes [sic] involuntary unemployment hypothesis is ill-grounded” (De Vroey *et al.*, 2009, p. 1).

<sup>15</sup> Note that Klein adopts a quite defensive tone in his claims. He asserts that this problem would only be interesting from the point of view of the history of economic thought, and he dismisses it. It is worth remembering that, by 1950, Klein was not a member of the Cowles anymore and that the econometric programme had already lost much of its earlier personal enthusiasm, as well as its financial and institutional support. In fact, the Cowles Monograph 11 was the result of Klein’s work at the Cowles in

In his 1947 paper and in his 1950 book, Klein defends a fundamental idea that reflects his image of econometrics. He wants to convince the reader that irrespective of the economic beliefs that econometricians might have, econometrics should be a tool capable of leading the researchers to the same conclusions:

“It is desirable to provide tools of analysis suited for public economic policy that are, as much as possible, independent of the personal judgements of a particular investigator. Econometric models are put forward in this scientific spirit, because these models should lead all investigators to the same conclusions, independent of their personal whims” (Klein, 1947a, p. 111).

Klein’s (1950) celebrated Model I is then a good example of this model-building procedure. This simple model consisted of “a completely determined system containing three statistical equations (i.e. three equations involving random terms and unknown parameters) plus some definitions or identities” (Klein, 1950, p. 58). The model had the following form:

$$\begin{aligned}
 (1) \quad & C = \alpha_0 + \alpha_1 W + \alpha_2 \Pi + u_1 \\
 (2) \quad & I = \beta_0 + \beta_1 \Pi + \beta_2 \Pi_{-1} + \beta_3 K_{-1} + u_2 \\
 (3) \quad & W = \gamma_0 + \gamma_1 Y + \gamma_2 Y_{-1} + \gamma_3 t + u_3 \\
 (4) \quad & Y = C + I + G \\
 (5) \quad & Y = \Pi + W \equiv \\
 (6) \quad & \Delta K = I
 \end{aligned}$$

Where  $C$  is consumption,  $W$  is the wage bill,  $\Pi$  is non-wage income (profit),  $I$  is net investment,  $Y$  is output,  $t$  is time and  $G$  are the goods demanded by the government and foreigners. All variables are expressed in constant dollars.

Let us just take one equation – the investment function (2) – as an illustration of Klein’s way of constructing his models. Since, according to Klein, in both the Classical and the Keynesian cases, the investment function is derived from the principles of profit-maximization, I will first describe the construction of equation (2) from these points of view, and then, I will come back to describe the Marxian path leading to that equation.

“... the methodologies of classical and Keynesian economics do not differ... Both theories are based on... business-firm profit- (or utility-)

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1947, not in 1950 (Bjerkholt, 2014b). Koopmans had already occupied the research directorship of the Cowles at that moment.

maximization to get the demand for producer goods, labor and business cash-holdings” (Klein, 1947b, p. 117).

The principle of utility-maximization gives Klein the possibility of developing equation (2) by going along two different ways. On the one hand, he can derive it from “the heuristic principle...that profits are the mainspring of economic action in a capitalist society. Entrepreneurs expand when profits are anticipated to be high and contract when profits are anticipated to be low” (Klein, 1947b, p. 60), and so, Klein establishes a positive relation between investment  $I$  and (expected) profits, which depends upon profits today  $\Pi$ , and precedent profits  $\Pi_{-1}$ . “However, not only the absolute size of profits but also their relation to the existing stock of capital is important; hence the variable  $K_{-1}$  is introduced” (Klein, 1950, p. 60).

The other way he goes along to derive equation (2) is by dividing the economy into two social groups: “workers and capitalists” or, in this case, rather consumers and producers, and assuming that they attempt to maximise their respective satisfactions. In short, going along this way, Klein first looks into the behaviour of the individuals within the social classes, and then aggregates their individual equations in order to get the total demand for investment goods. The consumers’ satisfactions depend upon the current and future consumption of household goods and services, while the producers’ “satisfactions depend upon the use of current and future consumer goods and services and also upon the consumption of producer goods in their possession...The income of [producers] can be used for two purposes, to spend on consumer goods and to spend on producer goods. They derive ‘pleasure’ from both types of spending” (Klein, 1950, pp. 60-61). Producers’ utility-maximization can lead, then, to equation (7), which represents the demand for producer goods:

$$(7) \quad d_j = \beta_{0j} + \beta_{1j}\pi_j + u_j$$

Taking into account that in the classical world the use of capital is represented by

$$d = d(i, k_{-1})$$

Klein arrives at the individual’s equation of investment (8).

$$(8) \quad i_j = \beta_{0j}^* + \beta_{1j}^*\pi_j + \beta_{2j}^*(k_j)_{-1} + u_j$$

If the latter equation is aggregated over all firms, it becomes equivalent to equation (2). Note, however, that Klein’s way of aggregating individuals’ behaviour was also of a special kind. Because of its importance I will not be able to give a thorough discussion of his method of aggregation, but I cannot just leave this point without any comment.

Paul Samuelson (1983) denoted Klein's approach as Envelope Aggregation<sup>16</sup>. Rather than deriving a macroeconomic theory from the mere aggregation of microeconomic theory, Klein took "the existing macro and micro theories as given and then measur[ed] the economic variables in a way that aim[ed] to insure consistency between both sets of theories...The distinguishing feature of Klein's approach is that measurement is what is endogenous rather than theory" (Marquez, 1985, p. 3).

In the case of deriving the investment equation from Marxian principles, Klein recognises that Marx's equations do not represent complete systems of equations but mainly definitions, and so, he "search[es] through Marx's literary explanations and numerical examples for the strategic hypotheses that will produce a determinate system of equations" (Klein, 1947b, p. 120).

"Our model is intended as an extension of the Marxian analysis to a logical conclusion in terms of a theory of effective demand. Actually, Marx laid the groundwork for a complete equation system to determine the level of income (effective demand) but did not build the complete system"

Furthermore, Klein recognises the methodological differences between the Marxian and the classical and Keynesian cases.

"The methodology of the Marxian approach is quite different from that of Keynes and the Classics: Instead of studying the behavio[u]r of individuals, Marx studies the behavio[u]r of classes directly...the Marxian system of macroeconomics differs essentially from the Keynesian and classical systems. The macrounits in the latter systems are producers and consumers, and this overlapping fails to bring out some essentials. The macrounits of the Marxian system are not only producers and consumers but also workers and capitalists. The latter two groups are, practically speaking, exclusive, and their basic conflict of interests can more easily be singled out as one of the moving forces in the system" (Klein, 1947b, p. 118).

In the case of the Marxian determination of the demand for investment goods he follows two steps: first, he "deriv[es] the demand relation for constant capital (capital used up) according to Marx and then transform[s] the demand for constant capital into investment" (*ibid.*, p. 120). Since workers only demand consumer goods in the Marxian system, the demand for constant capital is based entirely on the behaviour of capitalists. Klein, then, searches for Marx's numerical examples in *Volume II* of *Das Kapital* to establish the form of the Marxian demand for investment goods. First, the demand for investment goods will only depend upon the behaviour of capitalists, since

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<sup>16</sup> For a comprehensive discussion of Klein's aggregation methods see Klein (1946a; 1946b; 1950) and Hoover (2012).

workers buy only consumer goods. Since, as Marx explains, capitalists in Department I (the producer-goods industry) spend from surplus value  $S$  on constant capital  $C$ , then, Klein obtains the following equation<sup>17</sup>:

$$(9) \quad C = \beta_0 + \beta_1 S$$

Klein, then, attempts at a transformation of this equation in order to work with the variable  $I$  instead of  $C$ . He denotes the capital acquired during the  $p$ th preceding time period by  $x_{-p}$  and he becomes:

$$(10) \quad C = C(x, x_{-1}, x_{-2}, x_{-3} \dots)$$

Making a linear transformation of (10) he gets:

$$(11) \quad C = \delta_0 + \delta_1 x + \delta_2 x_{-1} + \delta_3 x_{-2} + \dots$$

And because of the impossibility of statistically measuring in a separate way the capital purchased during every preceding period, Klein approximates all these variables by means of a proxy variable, representing all the capital accumulated until the period under consideration (Klein, 1950):

$$(12) \quad C = \delta_0 + \delta_1 x + \delta'_2 Z_{-1}$$

Furthermore, he writes the stock of existing fixed capital in terms of the net investment of all preceding periods as:

$$(13) \quad Z_{-1} = \sum_{t=-1}^{-\infty} I_t$$

And because it is net investment, rather than gross investment, which interests him, Klein writes:

$$(14) \quad x = I + C$$

The next two steps before arriving at the final result are simple algebra. He substitutes (14) in (12), and gets:

$$(15) \quad C = \delta_0 + \delta_1(I + C) + \delta'_2 Z_{-1}$$

And finally, he replaces (9) in (15), getting:

$$(16) \quad I = \beta_2 + \beta_3 S + \beta_4 Z_{-1}$$

Note that equation (16) is equivalent to our original investment equation (2).

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<sup>17</sup> I keep Klein's (and Marx's) original notation, where  $C$  is constant capital, not consumption.

Beyond the algebra and the simple and elegant mathematical final form of the Marxian, Keynesian and (Neo)classical model of effective demand, the important point is that Klein gets at the same equations and at the same model departing from three different theories. Klein recognises, however, that he could have gotten many different model specifications from each of the theories and that he had to do a careful selection of the parts of the theory that would be useful for his particular task.

“...we have not utilized Marx’s methods to their fullest extent. Only those aspects of Marx’s theories are used that are necessary to build a complete system of equations. Many Marxian theories are unrelated to the principle of effective demand, but even some of those parts of his theory that are related to effective demand have been left out” (Klein, 1947a, pp. 125-126).

He carried on the transformations of the equations and he expressed the hypotheses in a peculiar way, so that he could get the same equations in the end. But he did not just pick up any hypotheses carelessly. He actually confronted each hypothesis with data, and he used its good fit as a criterion to reject or accept it.

“Many of the parts of the Keynesian system have withstood the test of being consistent with observed data” (Klein, 1947a, p. 116)

“...the author has applied various methods of statistical estimation to the Marxian model and has found the estimated parameters to be very reasonable in size. Moreover, the model fits the observed data very closely” (ibid, p. 127).

In short, Klein based the construction of his models on the multiplicity of hypotheses. In his process, which appears quite close to that proposed by Boumans (1999; 2005) Klein “backed” his models using different bits and pieces of distinct theories as “ingredients” for his model construction. Klein did not follow any well-defined recipe. He developed mathematical models of the economy as a whole based on hypotheses of different (and sometimes opposed) economic theories; he then, confronted these hypotheses to data, retaining those, which seemed to fit the observations. Although I did not describe Klein’s confrontation of the hypotheses with data, one can imagine that the whole process of model specification was inevitably accompanied by an activity characterised by arduous work, consisting on a tedious, repetitive and sometimes even disappointing task. Tinkering might quite well describe Klein’s econometric modelling activity.

*Painstaking effort and disappointing results of the Cowles’s methods of estimation*

Not only the original workers of Marschak’s econometric programme, like Koopmans,

Hurwicz or Marschak himself, eventually abandoned econometrics. Also the Cowles Commission ceased to pay attention to econometrics and, especially, to macroeconomic model building (Klein, 1991; Epstein, 1987; Mirowski, 2002; 2012). The effort of the Cowles Commission to regain attractiveness for funding might be partly responsible for this abandonment (Mirowski, 2002; 2012). Yet, there is another part of the story that has to do with more technical reasons, which accounts for the loss of enthusiasm and faith in the programme.

Econometrics *à la* Cowles was much of a painstaking activity with “three joint lines of interest: 1. economics, 2. statistics, [and] 3. mathematics” (Klein, 1991, p. 109). These lines of interest, combined with the high ambitions of the Cowles, were not necessarily comfortable subjects to work at, for they constituted the source of a painstaking activity, supposing heavy mathematical and statistical work, long and tedious calculations, repetitions, many disappointments, drawbacks and the remaking of the econometric exercise all anew again and again (Klein, 1991). The emphasis that each author placed on the different lines of research tells us a lot about the images that every econometrician had about econometrics. As we have seen from the Measurement without Theory controversy, Koopmans accorded more importance to the lines of interest “statistics” and “mathematics”. Klein, however, accorded more importance to the line of interest of economics as I will show further on.

One has also to imagine that the technical possibilities of doing calculations at the time were completely different from the ones we know today. Without the aid of advanced calculation machines such as the digital computer, “elaborate calculations were slow, complex and awkward to carry out” (Mirowski, 2012, p. 147). The research field of econometrics was then seen as an activity that needed of a teamwork effort to be successfully fulfilled, partly because of the burdensome calculations. The team assembled by Marschak at the Cowles Commission was divided in a specific way, where each worker was in charge of an explicit task, so that the team could work out the complex solutions in the most efficient way<sup>18</sup>.

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<sup>18</sup> Klein gives a very clear account of the division of labour within the Cowles Commission:

“When [Marschak] recruited [Klein], it was explicitly to prepare model specifications according to received theory...In addition [he] was assigned the task of data preparation and model estimation/testing...[Tygvre] Haavelmo was recruited to work on econometric theory, Theodore Anderson to work on the underlying theory of mathematical statistics, Koopmans to work on overseeing all the pieces but especially on implementation of the work, through computation that was very complicated and tedious, given available facilities of the day. Herman Rubin worked on econometric theory and mathematical statistics; Leonid Hurwicz...contributed to all aspects of the work; Roy Leipnik was a mathematical statistician for the project. Don Patinkin was assigned work on

When the programme was taking off by 1944, Marschak claimed that the Cowles would deliver powerful new results for economic analysis in just three years (Klein, and Mariano 1987; Klein, 1991). Nevertheless, the results of the first macroeconometric models did not prove very convincing, not only within the Cowles, but also in the other institutions based at Chicago and in Washington<sup>19</sup>.

Inside the Cowles, Marschak's reactions in respect to the results of Klein's models were quite clear. "As early as 1946...Marschak...did not wish to claim much for Klein's early efforts" (Epstein, 1987, p. 105):

"The present admittedly *very crude and preliminary results* were tentatively applied to measuring the effects of policies; though it may have been wiser not to include the discussion even in a privately circulated monograph"<sup>20</sup> (Marschak, 1946, quoted by Epstein, 1987, p. 105, my emphasis).

Carl Christ, was hired by Koopmans "to clean up the Klein model, re-estimate it for the period 1921-47, and [to try to] 'do it right' from the Cowles perspective" (Mirowski, 2012, p. 156). But he did not get promising results either. In the conference on business cycles held at the NBER at the end of 1949, Christ's results were heavily criticised by prominent economists such as Friedman, Leontief, Hildreth or Metzler. Even economists who had actively participated in the conception of the model – like Klein, Marschak and Koopmans – showed themselves sceptic about the results (Epstein, 1987; Mirowski, 2012). It was, in any case, the prelude of the end of the

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a sectoral model for manufacturing, but drifted more towards an interest in the underlying Keynesian macrotheory" (Klein, 1991, pp. 109-110).

<sup>19</sup> Albert Hart, a member of the Committee for Economic Development (CED), convinced Marschak and Klein to present, in 1945, the projections of their macroeconometric model to the Bureau of Budget, the Department of Commerce and the Federal Reserve Board. Klein was not very enthusiastic at the beginning because he not only thought that his results were too preliminary, but also because he thought that the results would be very pessimistic (Klein, 1991). "To [his] surprise, this first exercise, though premature, was very bullish" (Klein, 1991, p. 114). However, the reaction of the governmental agencies in Washington was not very optimistic. "The Cowles-CED projections were not taken seriously; the response in all cases was that we should wait until mid year 1946, when we would find 6 million unemployed again and a return to [the] conditions of the Great Depression" (Klein, 1991, pp. 114-115). The general belief of the U.S. economists that the economy would fall back into a slump period after World War II, might explain this negative reaction to the Cowles-CED projections. If the Cowles projections were not being taken *seriously* by the governmental agencies it was because the Commission lacked credibility, and not because her methods were rudimentary or not robust enough.

<sup>20</sup> Klein was of course aware of the scepticism that his macroeconometric modelling results provoked in the Cowles's directorship. With hindsight he described the situation in the following way:

"In general, the senior researchers at the Commission were not satisfied with the performance of models that had been constructed during the expansionary phase of the research program[me] and there was relatively little carry-on activity in empirical model building with repeated applications over sustained time periods" (Klein, 1991, p. 115).

econometric programme at Cowles, which driven by new (military) funding, was being directed towards different fields of research, especially towards activity analysis (Mirowski, 2002). But Christ himself had also a thing or two to say about Klein's model. He described Klein's predictions of the price level and disposable income for 1941 as "absurd" (Epstein, 1987), and hence, his criticism reinforced the general pessimism about macroeconometric empirical work from within the very Cowles Commission<sup>21</sup>.

Klein himself had been quite self-critical about his models. Not so much about his results, though, but especially about the estimation methods promoted at Cowles. With hindsight, Klein would recognize that it was not the methods developed at Cowles during the 1940s, which should be credited for the increase of the accuracy and usefulness of econometrics:

"If econometric results are today more useful than in the past, this is only partly a result of the particular method of estimation but much more significantly a product of painstaking research of a more pedestrian nature...I would expect marginal improvements of five or ten per cent through the use of more powerful methods of statistical inference...*The adoption of more powerful methods of mathematical statistics is no panacea*" (Klein, 1960, p 867, my emphasis).

"Great faith was placed on the ability of sophisticated statistical methods, particularly those that involved advanced mathematics, to make significant increments to the power of econometric analysis. I [Klein], personally, place more faith on the data base, economic analysis (both institutional as well as theoretical), political insight, and attention to the steady flow of information" (Klein, 1991, pp. 113-114)

Another recurrent criticism of the Cowles's methods of estimation increased the scepticism about the structural econometrics programme. It was not clear for the

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<sup>21</sup> Other criticisms also reinforced the pessimistic environment towards structural econometrics. Herman Wold (1951), for instance, "continued to question the logical status of causality in the simultaneous model. Robert Solow (1951) wanted an explanation for why simultaneous equations estimators and OLS estimators seemed to yield similar results in practice...In an obscure review written in German, Tinbergen[']s...comments read as a most diplomatic refusal to endorse the new methodology" (Epstein, 1987, p. 112) not to mention Friedman's (1957; 1958a; 1958b) criticisms, also documented in Epstein (1987). Theil (1954) "outlined a theorem showing that the generalized variance of least-squares estimates of the parameters in a single equation is at least as small as that of limited-information- maximum-likelihood estimates" (Klein, 1956, p. 217). And, of course, also the NBER members attacked the lack of results of the Cowles methods:

"[T]he only satisfactory test of the usefulness of [the Cowles] methods is their fruits, and these have not yet been attained, or if attained, have not yet been made generally available" (Vining, 1949, p. 77).

economists why they should adhere to the methods of estimation like the maximum-likelihood-method (MLM) or the limited-information-maximum likelihood method (LIML), when more pedestrian methods such as the ordinary least squares (OLS) seemed to generate equally accurate results with a considerable amount of less effort. Economists, as prominent as Robert Solow, expressed their doubts about the superiority of the Cowles's methods.

“Robert Solow (1951) wanted an explanation for why simultaneous equations estimators and OLS estimators seemed to yield similar results in practice. Solow was also of the opinion that ‘when they differ, good sense often favo[u]rs least squares’” (Epstein, 1987, p. 112).

Beyond the technical issues largely discussed in Klein (1956; 1960) and Epstein (1989), it is worth noting that the economic community was just not convinced that they should undertake such a painstaking effort in order to get some results that seemed to be just as good as those obtained by means of more pedestrian methods. Not every economist had the possibility of embracing such a burdensome task, since macroeconometric modelling needed of a great amount of institutional and financial support, the disposition and eagerness to undertake tedious and teamwork-based work, and some kind of warranty that the results would return the sweat, the trouble and the time invested, proving its usefulness in governmental affaires (or in the industry, as Klein would show from the 1960s).

In short, Klein resumes the key message of the experience of the econometric programme at Cowles in the following paragraph:

“Important lessons to be learned from the Cowles Commission experience are that statistical consistency or unbiasedness is not the most important property of estimators; precision is associated much more with variance or efficiency. It is possible to trade consistency for error variance and come out ahead. It is important to grasp the simultaneity of the macroeconomy but not necessarily to tie statistical estimation methods exclusively to this property. *It is more important to be able to update, correct, or revise estimates on the basis of a steady flow of important new information, and very flexible methods of estimation are needed for this purpose.* The highly flexible methods can be more powerful in simple form than the more complicated procedures that we were following at the Cowles Commission. In particular, for an economy where detailed information is important, it is preferable to aim for large systems...and to handle them by relatively flexible, simple statistical methods instead of paying enormous attention to complicated estimation procedures for smaller manageable systems...*I believe that the spirit of what we were trying to achieve in that beginning period can best be reached by statistical methods*

*that are simpler than those that we thought were most powerful at the Cowles Commission”* (Klein, 1991, pp. 115-116, my emphasis).

*Klein’s appeal to institutional reality and his ‘methodological structuralism’*

Klein’s methodology also marks an important contrast with the Cowles’s. Nell and Errouaki (2013) have labelled Klein’s appealing methodology as *methodological structuralism*<sup>22</sup>. Contrasting with the Cowles’s methodological individualism, Klein’s methodological structuralism does not necessarily start with the individual as the one and only unit of analysis. Klein recognizes the existence of the individual, of course, and understands that her behaviour affects the economy in a way or another. But Klein’s focus is on the discovery of the underlying structure of the economy. He understands that it is the social and economic institutions, which constitute the most fundamental pieces of this underlying structure.

“[Klein] approach[es] the explanation of economic events in terms of a social world made up of institutions, roles, responsibilities, powers and so on...[and] considers the socio-economic system to be made up of ‘structured objects’ whose powers exist independently of our knowledge or perception...The policeman *has the power* to arrest us, and the President *has the power* to call up the National Guard, whether we know it or not. These objects, relationships, powers and duties constitute the basis of the causal relationships that economic science describes. Employers *can* hire and fire workers and *can* order them around; firms *can* move capital from place to place opening and closing plants” (Nell and Errouaki, 2013, p. 430, my emphasis)

But the examples that Nell and Errouaki provide could be also applicable from the perspective of methodological individualism: the policeman could individually decide to arrest us, or the president could call the National Guard just as a result of some kind of individual decision. What is relevant here is that the structure of the economy (and society) provides a special kind of *power* to particular organizations and individuals. It is

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<sup>22</sup> In fact, Nell and Errouaki (2013) have treated Klein’s methodology as if it presented no significant difference with the Cowles’s methodology. Their claim is that the structural econometrics programme at Cowles was in the “right” way, and that econometrics today should take again this track and come back to the structural econometrics programme (ibid). My aim is not to appraise whether Nell and Errouaki’s claim is just or not, nor is it to say what econometricians should do today, for I am not trying to do any kind of normative statement about econometrics. From a historical point of view, however, I disagree with Nell and Errouaki (2013) in their intention of bringing the Cowles and Klein’s methodology to a common level. I think that they wrongly equalise Klein’s position with that of the Cowles. Nell and Errouaki take Haavelmo’s position as if it were the Cowles’s official stand. Yet, even if Haavelmo visited the Cowles for a few years, and even if he developed his 1944 paper during his time there (Bjerkholt, 2007), Haavelmo does not represent the Cowles’s official position. It is rather Marschak and Koopmans who, as research directors, better represent the Cowles’s official position.

not that the employer optimises her choice, but that she has the power to hire or fire, while the employee has no power at all to keep her job or to get a new one, whether her choice is the result of a process of optimisation or not. In short, taking into account the powers, institutions, rules, responsibilities and roles of the individuals in the economy and society, economists would count with more (relevant) information to find out about the world, which would be very valuable in the building of their econometric models.<sup>23</sup>

In order to improve her model, the researcher has the possibility (or the duty) of introducing more accurate a priori information into the mathematical model, which reflexes, for example, the relation of power between the employers and the employees in the labour market. A priori information is a kind of knowledge about the economy as a *whole* and “is [therefore] independent of the particular sample being used [and]...may consist of economic theory, a knowledge of economic institutions, a knowledge of technology, or empirical results from independent samples” (Klein, 1957, p. 2).

A priori information stemming from “knowledge of technology” means that some improvement could be attained by the development and refinement of more sophisticated methods of statistical inference. As I have already evoked, the improvement of this kind of knowledge would be much closer to the line of research of the Cowles Commission. Klein did not think that these technical improvements would be decisive for the advancement of econometrics. He rather thought that it was the improvement of institutional reality and the refinement of data, which would decisively contribute to the improvement of econometric modelling:

“The building of institutional reality into a priori formulations of economic reality and the refinement of basic data collection have contributed much more to the improvement of empirical econometric results than have more elaborate methods of statistical inference” (Klein, 1960, p. 867).

Klein did not just defend his intuitive idea, which prayed that “the more that relevant information is used, the better are estimates that make use of it” (Klein, 1985, p. 8). He actually found a formal way of expressing and demonstrating that the use of more relevant a priori information would render the estimators statistically more efficient. To show his result he came back to a concept he had learned from his PhD thesis director, Paul Samuelson: Samuelson’s principle of Le Chatelier.

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<sup>23</sup> Nell and Errouaki (2013) argue that Klein’s methodological structuralism would overlap Nell’s methodological institutionalism. “Both approach the explanation of economic events in terms of a social world made up of institutions, roles, responsibilities, powers, and so on” (ibid, p. 430).

“Samuelson showed that the equilibrium values of the diagonal elements of a certain matrix of bordered second-order derivatives of a consumer’s utility function become smaller and smaller as more restrictions are placed on the maximization of the utility function...He showed in effect that price sensitivity (elasticity) is reduced as additional restrictions are imposed... I [Klein] *noticed that there was an analogy between the matrices of second-order derivatives in utility theory and in maximum likelihood theory for econometric estimation...As a priori information is added, the bordering increases and the estimation-efficiency measures improve*” (Klein, 1985, p. 8 my emphasis).

Klein had seen the difficulties of applying the estimation methods of the Cowles Commission in everyday econometrics. He had also recognized that these methods did not throw clearly superior results than more pedestrian methods like the OLS (Klein, 1950; 1955). Even if Klein tried to praise the methods developed at Cowles during his stay at the Cowles Commission, it was clear for him that practical and applied problems proved that more pedestrian methods were at least as effective as the more sophisticated methods (*ibid*). Klein thought that econometrics was a matter of understanding the economy, and this better understanding of the economy had to be reflected in the integration of institutional reality in the mathematical construction of the econometric models.

The implementation of structural macroeconomic models in a particular country provides a good example of the importance of taking into account institutional reality. Klein was aware of the fact that every country wanting to build a macroeconomic model, should undertake a serious study of the particular way in which its economic and social institutions really worked. Long before he embarked into Project LINK<sup>24</sup>, Klein recognized that there were particular institutional factors typical of every country that must be taken into account, in order to build adequate and sound models.

“A workable model must be *dynamic* and *institutional*; it must reflect processes through time, and it must take into account the main institutional factors affecting the working of any particular system. Different features must be built into adequate models of such diverse economies as the United States, Canada, the United Kingdom, Japan, the Netherlands, etc. Models of non- capitalistic societies will differ even more radically from the models of capitalist countries, with investment not an endogenous magnitude” (Klein, 1954, p. 279).

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<sup>24</sup> Project LINK started in 1968 and was initially founded by the Rockefeller Foundation. It “sought to integrate the macroeconomic models of different countries, which eventually included Third World countries and socialist nations, into a total simultaneous system” (Mariano, 2008, p. 8). For a more comprehensive account of Project LINK see (Bodkin *et al.*, 1991).

## Concluding Remarks

Lawrence R. Klein conceived econometrics as a pluralistic scientific tool for economic planning. As a pluralistic tool, econometrics could integrate different (and sometimes conflicting) theories and hypotheses, providing the possibility of better understanding and intervening the economy. Intervention should, according to Klein, go beyond the reforms of the capitalist system; intervention should be carried on in order to really change the economic system, and its underlying structures of power. The daily practice of econometrics would imply a field of applied and theoretical research characterised by a great deal of teamwork and discussion, not only because of the technical impediments of the 1940s and 1950s, but also in the computer era where calculations should not be a problem (Klein and Mariano, 1987). Econometrics would also be a discipline characterised by a lot of tinkering and thinking, crossing the boundaries between theoretical and applied work in economics, statistics and mathematics, and also between history and politics. As a method of inquiry, the scientific practice of *econometric modelling*, would provide the possibility for economists to find out about the world.

Klein did not just “look” at the model in order to learn from it; while building and manipulating the model, Klein was tinkering, thinking, adjusting, discussing and thinking again<sup>25</sup>. On the one hand, Klein’s openness towards Neoclassical, Keynesian and Marxian economic theory played an important role in the building of his models, providing him with the possibility of enriching his hypotheses and of specifying his model equations in various ways. Also his methodological structuralism and his taking into account of institutional reality proved much more flexible and more applicable to a variety of contexts, than the more rigid methodological individualism promoted at Cowles. On the other hand, Klein promoted the idea that macroeconomic modelling was not a “once-and-for-all-job” (Klein, 1950; 1955), but rather a practice consisting on the rethinking, re-discussing, re-specification and re-estimation of the models, and on the inclusion of new relevant institutional information and data.

In a nutshell, two elements characterised Klein’s image of econometrics. Klein not only softened the rigid econometric approach from the Cowles, making it less theoretical and more familiar to the reality of economists’ practices; he also enriched the econometric approach by introducing elements of *institutional reality* in his models, rendering econometric modelling not only a tool ready to intervene in the economy, but also a practice allowing to find out about economic relations and phenomena. These elements made of Klein’s image of econometrics an image capable of being

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<sup>25</sup> Klein’s way of handling the practice of econometric modelling might be close to Morgan and Morrison’s (1999) description: “we learn...from building and from manipulating [the model]” (p. 12), rather than just from looking at the model.

disseminated throughout the economics community, revealing econometrics as a powerful scientific tool, applicable to all economic streams (main or not), providing some standards of how to actually undertake econometric studies. In short, Klein's image of econometrics rendered econometric modelling a feasible and useful practice for economists.

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