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# Econometrics at Harvard<sup>1</sup>

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## Abstract:

The place of Harvard in the development of econometrics is assessed through the research of its faculty and the development of the courses they taught. Over the course of the century, as the content of econometrics was itself transformed, we find that the Harvard department of economics occupied a singular place in the econometric landscape. This idiosyncrasy was defined by large projects such as the Harvard barometer of the 1920s or the postwar development of input-output analysis, but also by the lack of common purpose that often characterized the work of its members. Through the storied halls of the university passed many of the best and brightest, but few were given the chance to pursue their projects durably, to the detriment of the constitution of a “Harvard econometrics.”

Keywords: Persons, Leontief, Jorgenson, business cycles, input-output, investment studies, microeconometrics, quasi-experiments, panel data

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Two questions can be asked of a chapter tracing the contributions of Harvard economists to econometrics: What role did Harvard econometricians play in respect to the larger evolution of the field? How did their contributions reflect the changing meaning of econometrics?

As to the first question, there certainly is some difficulty to delineate Harvard's contributions to econometrics; over the last century, there were periods of high creativity, often characterized by large projects which lasted one or two decades, and were followed by periods of relative apathy. But throughout, we are compelled to notice that Harvard's role in the development of econometrics, expected to be that of a leader, reveals itself often to be one of a follower of current trends; its pioneers stood somewhat outside of what is considered econometrics by contemporaries. One explanation of this state of affair is that many of the brightest economists passed through the University, but few remained durably, something specific to a university which has the capacity to attract the very best but is as limited as any other when it comes to predict those that will truly innovate. Thus, of those leading econometricians that were associated with Harvard, many were either already established or made their most important contributions after leaving Cambridge; this will be a recurring theme through this chapter.

As to the second question, there was clearly an evolution of the content behind the term "econometrics." Before it became used in the 1930s after the creation of the econometric society, there certainly was empirical work being done, and several Harvard economists participated in those pioneering studies. But in the 1930s, Ragnar Frisch and others came to define the subject of econometrics as the "unification" of economic theory, mathematics and statistics (Frisch, 1933), so that earlier statistical work lost its econometric character for the new brand of econometricians.<sup>4</sup> However, the meaning of the term narrowed down during the following centuries, to the point that econometrics and economic theory became separated, lending more importance in the history of econometrics to the early statistical work that was done without relation to economic theory.

In the following, we will try to give an idea of what constituted econometrics at different moments since the beginning of the 20<sup>th</sup> century, and how Harvard economists contributed to this evolution, through the development of their research and through the courses they taught.

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<sup>4</sup> The term was introduced for the first time by Frisch (1926: 1), in French (our translation): "Intermediary between mathematics, statistics and political economy, we find a new discipline which one can, for lack of a better term, designate under the name of *econometrics*."

## **I/ The Harvard Economic Committee and the construction of economic barometers (1910s-1920s)**

Before the creation of the Econometric Society and the statement of its aim by Frisch and others, the field of empirical economics was very fragmented. At Harvard, developments pertaining to the history of econometrics were done mostly through the work of the Harvard Economic Committee around the issues of business cycle forecasting and the estimation of demand.

The creation and development of the economics department at Harvard has been recounted by Edward Mason, who had been a member of the economics department since the early 1920s (Mason, 1982). In the early 20th century, the teaching was overwhelmingly not quantitative, although there was a course in statistics.<sup>5</sup> In 1910-1911, Allyn Young gave the first course in mathematical economics as a visiting professor from Stanford; Mason underlined that “It was also the last course in mathematical economics or econometrics given at Harvard until 1933 when Schumpeter gave a half-course entitled Introduction to the Mathematical Treatment of Economic Theory” (Mason, 1982: 404-405).

Mason did not mention the brief passage of Philip G. Wright, who arrived in 1913 at Harvard as Frank Taussig’s assistant before becoming an instructor in economics, and though he only stayed for four years before departing for the U.S. Tariff Commission, he managed to publish several articles in the *Quarterly Journal of Economics*, including a review of Henry L. Moore’s book on business cycle (Wright, 1915), where he presented one of the first explanations of the identification problem in empirical economics.

The construction and development of the Committee on Economic Research and the Harvard Economic Service that was put in place to publicize its research were by far the most important development of the 1920s at Harvard’s economic department, especially through the construction and diffusion of the barometer of business conditions, which served as a model to many other business cycle research institutes created throughout the world.<sup>6</sup> The Committee was created in 1917 by Bullock, who became its first chairman. Warren Persons proved to be a crucial recruit

<sup>5</sup> “[William Z.] Ripley, who joined the Department in 1902 ... offered the only course in statistics until [Edwin] Day took over in 1915” (Mason, 1982: 402-403). In 1918-1919, Persons took over the class on statistics which was still classified in the category “Economic Theory and Method.” Starting from the academic year 1919-1920, a new category specifically for statistics was created and the lecture was split between theory and practice, both taught by Day.

after his arrival in 1918, and he was put in charge of the economic forecasts and the editing of the *Review of Economic Statistics*, first published in 1919.<sup>7</sup> The Committee issued the *Review* as a quarterly, initially with monthly supplements which were replaced by a weekly letter on “economic conditions” in January 1922; the Harvard Economic Service was the private institution in charge of this latter publication (Friedman, 2009: 66-67). Although the forecasting activities were not immediately profitable, it was a sprawling operation which caused some unease among Harvard’s alumni and personnel, especially after its failure to predict crises (a first economic slowdown in 1924, and of course the 1929 crisis).

Nevertheless, the work of the Committee marked the history of econometrics through its empirical approach to business cycles and the methods it pioneered. Persons’ central idea was to separate what he identified as the four main components in economic time series, which he believed acted together to create the observed movement: the secular trend, seasonal fluctuations, business cycle oscillations and accidental events. It has already been argued that its main influence was on the treatment of statistical time series (Morgan, 1990: 57, 63), because Persons described at length his methodology to detrend time series and dispose of seasonal fluctuations in the first issue of the *Review of Economic Statistics*. He proposed two methods to detrend time series: moving averages and curve fitting, and he favored the results of the second method which gave an explicit measurement of the trend (Persons, 1919b: 13).

There was less arbitrariness on the issue of seasonal fluctuations, because the period of measurement was known, with the month “adopted here as the most satisfactory unit” (Persons, 1919b: 18). The objectives were the same as in the case of trends, finding fluctuations in monthly data, measuring them and correcting them (Persons, 1919b: 18). Persons proceeded experimentally by comparing different series and how they behaved, and argued “that a study of seasonal variation should be based upon percentages in which the preceding month is the base” (Persons, 1919b: 22). Finally, he examined “Cyclical and Irregular Fluctuations” together, concluding that they were virtually impossible to distinguish. The rest of his paper was dedicated

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<sup>6</sup> These institutes modeled after the Harvard Economic Service were often financed by the Rockefeller foundation; on their development and the influence of the Harvard barometer see Craver (1986), Friedman (2009) and Hagemann (2021).

<sup>7</sup> After the demise of the Economic Service, the *Review* was taken over by the Economics Department and it subsequently took its modern form of a leading generalist journal; it was eventually renamed in 1948 *The Review of Economics and Statistics*. About Persons’ career and his work at Harvard, see Foster (1939), Mason (1982), Morgan (1990: 56 sq) and Friedman (2009).

to applying the pre-processing described above to fifteen time series, but it only formed the first part of his inquiry into business conditions.

The second part was published in the next issue, and it described the process of construction of the Harvard barometer.<sup>8</sup> After he was able to isolate and standardize the business cycle by correcting his monthly series for the secular trend and seasonal fluctuations (Persons, 1919a: 115), Persons' approach was to group together the series that showed similar and simultaneous fluctuations. Although he only represented three curves (A, B and C) on the opening charts of his papers, he actually created five groups, each separated by two to six months. The groups were based on pairwise comparisons between series, and Persons devised a contraption (described with pictures in his paper) to facilitate this comparison: after the charts were "drawn on translucent drawing cloth," they were placed on the glass of a box illuminated from the inside (Persons, 1919a: 121). The "observers" working with him recorded qualitatively the size and direction of the correlation, the direction and extent of the lag and its consistency, before he computed the Pearson correlation coefficient on the pairs showing the most promises (Persons, 1919a: 127). Persons discussed briefly the problem of the statistical significance of the coefficient, but at this time he still felt that the detour through "the intricacies of the mathematics of the theory of probability" was "an unnecessary digression" (Persons, 1919a: 124). Nevertheless, he did spend the next three pages introducing basic elements of probability theory for statistical inference, and he presented several correlation coefficients with probable error intervals.

This was how the five groups of series were formed, and the barometer describing economic cycles were built. The three curves representing the movements of the speculative (A), business (B) and money markets (C) were extracted from those five groups "in order to secure a clearer picture of the time relationship of the cyclical movements than is given by five groups of series of monthly items" (Persons, 1919a: 114). It was those curves and their patterns of lags that were presented in the Economic Service's monthly supplements and weekly letters to its subscribers, to forecast the evolution of the economy (Friedman, 2009: 65 ff.).<sup>9</sup>

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<sup>8</sup> Persons had already partially described it in a previous paper published in 1916, while he was not yet at Harvard (Persons, 1916).

<sup>9</sup> A detailed exposition of their method of interpretation of the indices was only published in 1927 in the *Review of Economic Studies*. Written by Bullock, Persons and Crum (1927), this was in large part an answer to Karl Karsten's (1926) critique of their index; on this debate and how Harvard economists' developed a similar idea as Slutsky ([1927] 1937) on spurious cycles arising from statistical measurements, see Assous and Carret (2022: 10-12).

Persons ideas influenced the approach of time series used by early econometricians, but it hardly participated to the project of unifying economic theory, statistics and mathematics which became central in the 1930s. Persons argued that he did not want to build a theory (Persons, 1919a: 115); this was contested by Schumpeter, who arrived at Harvard shortly after he had left, and who believed that he had fallen prey to “a theory that was all the more dangerous because it was subconscious” (Schumpeter, 1986: 1131), when he had decided to separate the structure of the economy into different components. The idea that economic time series needed to be detrended was also contested by Mitchell who deplored that “[s]ecular trends of time series have been computed mainly by men who were concerned to get rid of them” (Mitchell, 1927: 212). Yet Persons’s approach of decomposing the business cycle influenced early econometricians such as Ragnar Frisch, even though the latter eventually approached business cycles with macro-dynamic models rather than with the data-driven analysis of Persons.<sup>10</sup>

### *The place of quantitative economics at Harvard in the 1930s*

Through its new recruits, the Committee was able to noticeably increase the size of the department of economics. Edwin Frickey, who had arrived at Harvard in 1917, and William L. Crum, who arrived in 1923, were recruited by Bullock and Persons to work on the Committee (Backhouse, 2017: 107). Although Bullock retired in 1934 and Persons left in 1928 (Foster, 1939: 412), Frickey and Crum remained and stayed as faculty members for several years (Mason, 1982: 414). As Persons, Frickey did not participate in the project of unification between economic theory and statistics as his approach rejected any use of economic theory (Backhouse, 2017: 107-108). In that sense, it was closer to the NBER approach associated with Mitchell and Burns, who favorably reviewed Frickey’s work (Burns, 1944).<sup>11</sup> Crum was also more of a statistician who nevertheless engaged in debates around business cycle theory, defending the legacy of the Harvard Economic Service with Bullock in 1932 (Bullock and Crum, 1932).

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<sup>10</sup> See Carret (2021) on this influence and how Frisch reinterpreted Persons’ ideas in one of the first macro-dynamic models. Frisch referred to Persons during his Yale lecture given in 1930 (Bjerkholt and Qin, 2010: 15), and he eventually built a model that produced both growth and cycles from the same mechanism, an idea that was at the heart of another Harvard economist, Richard Goodwin, when he published his nonlinear model of the business cycle (Goodwin, 1951).

<sup>11</sup> The Harvard Economics Department was also associated with the NBER through the work of Edwin F. Gay, a leading economic historian at Harvard during the first third of the century who became the first president of the NBER (Mason, 1982: 405 ff.).

The Committee also availed itself of the services of Elizabeth Gilboy, who became its secretary and a very active researcher during the interwar.<sup>12</sup> In the period 1930-1941, in addition to a book about *Wages in eighteenth century England* (Gilboy, 1934b) and another about work relief (Gilboy, 1940), she published around twenty articles in leading economic journals. Many of her publications were related to the estimation of demand curves either through extensive reviews of the works published on the subject such as those of Moore (Gilboy, 1930) and Marschak (Gilboy, 1931a), and the debates between Leontief and Schultz (Gilboy, 1931b) and Leontief and Frisch (Gilboy, 1933), or empirically in numerous studies that were part of a reorientation of the activity of the Committee on Economic Research (Gilboy, 1932a; 1932b; 1934a; 1937).<sup>13</sup> In the early 1930s, she noted that “the derivation of demand curves and the elasticity of demand by various statistical methods has become practically a branch of economics in itself” (Gilboy, 1932a: 376), showing the fast development of the themes at the origin of econometrics; she was also instrumental in bringing Leontief at Harvard in the early 1930s (see section II and Bjerkholt, [2016: 46, 50]).

Frickey and Crum continued well into the 1940s to teach statistics at Harvard, a subject that was taken by the young Paul Samuelson in the early 1930s. In a 1999 letter to Stephen Stigler, Samuelson argued that the course given by Crum was “not so much a course *on* statistics as *against* statistics” (cited in Backhouse, 2017: 106, original emphasis), and he preferred to follow the classes of Edwin B. Wilson on this subject. Wilson was a professor at Harvard’s Institute of Public Health who gave courses in mathematical economics and statistics at the Economics Department during the late 1930s (Backhouse, 2017: 149), and who also published several contributions to econometrics using Moore’s periodogram analysis on business cycle data (Wilson, 1934a; 1934b). Samuelson, during his time as a junior member of Harvard’s society of fellows in the late 1930s, did not really improve the state of Harvard econometrics, as most of his papers written during this period were concerned with economic theory and business cycle

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<sup>12</sup> On Gilboy’s main works, see Trezzini (2016: 285-286); Gilboy obtained her PhD from Radcliffe, Harvard’s Women College in 1929 under the supervision of E. Gay (Thomas, 2000: 168) and quickly joined the Committee.

<sup>13</sup> The introduction of her papers noted that her work was part of a larger “general study of demand undertaken by the Harvard University Committee on Economic Research” (Gilboy, 1932a: 376). After the demise of the Harvard economic service in the early 1930s, the Committee on Economic Research seems to have pivoted toward those studies and other works, and continued to finance econometric works, including Leontief’s early forays in input-output in the 1930s (Leontief, 1936b: 105; 1937: 109).



theory.<sup>14</sup> He did however produce for Hansen's *Fiscal Policy and Business Cycles* (Hansen, 1941) a statistical study of the consumption function that was inserted as an addendum to chapter XI of the book (Samuelson, 1941), a work which was in line with many other wartime and immediate postwar studies trying to predict the postwar economic activity in the United States.

The class on mathematical economics that was taught during the 1930s had been advocated by several members of the department at the beginning of the decade, in particular by Schumpeter who was involved in the construction of the Econometric Society and was aware of the necessity to train students to use mathematical tools.<sup>15</sup> While he taught the first iteration of the course, he was not a good fit and he subsequently let Wassily Leontief teach it from the mid-1930s to the late 1940s.<sup>16</sup> It was only in the early 1950s, that econometrics made a decisive entry into the curriculum, after Harvard recruited new faculty members who pushed toward a development of econometrics. It was also during this period starting in the 1940s that Leontief's research project began to take off, and that he developed his own particular brand of econometrics.

## **II/ The Harvard Economic Research Project: Leontief's approach to econometrics (1940s-1970s)**

The period of the forerunners ended around the time of the Second World War, and while quantitative economic theory developed at a rapid pace, econometrics *per se* still struggled to make its way into university departments. At Harvard, where he arrived in 1932 after a short spell at the NBER (Kohli, 2001: 192), Leontief led the way with his particular brand of econometrics, through the development of the input-output approach.

Before he threw himself behind the development of this program, Leontief made important breakthroughs during the late 1920s and in the 1930s, even before he arrived at Harvard. He

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<sup>14</sup> It was not until the 1940s, after he moved to the Massachusetts Institute of Technology (M.I.T.), that he started working on a statistical project that was continued by Lawrence Klein (Backhouse, 2017, chapter 17).

<sup>15</sup> See Schumpeter (1933), the first article published in *Econometrica*, about "The Common Sense of Econometrics." Alvin Hansen, who arrived at Harvard in 1937, was also from the beginning an associate editor of *Econometrica*, until he was replaced by Schumpeter in July 1938 (he stayed in this position until his replacement by John Hicks in January 1950). Afterward, Leontief was for many years the most involved Harvard economist in the Econometric Society; he was a council member of the Econometric Society between 1951 and 1954, was elected vice-president of the society in 1953, and president during 1954. In January 1959, he joined the editorial board of *Econometrica*, which he left in late 1967.

<sup>16</sup> Leontief replaced Schumpeter in 1935, while the latter replaced the retiring Taussig in the main course on economic theory (Backhouse, 2017: 118-119). See Collier (2016b, 2018) on the early evolution of this class.

gained international recognition with his analysis of the simultaneous determination of empirical demand and supply curve (Leontief, 1929), but his approach was criticized by Frisch and led to a debate shortly after he arrived at Harvard, which became known as the “Pitfalls” debate.<sup>17</sup> In the 1930s, he also contributed to the theory of index numbers (Leontief, 1936a), and how they could be used to measure a general price level.<sup>18</sup>

But his main project after he arrived at Harvard was the development of an empirical model of the interrelations of the productive sectors of the economy. It was the undertaking and successful realization of this gigantic task which took up most of his career and brought him fame and recognition.<sup>19</sup> Faced with the lack of data, the inadequacy of the classical theories of production and the dearth of computational tools, Leontief “set about compiling the first table with the aid of a modest grant and a few graduate students” (Dorfman, 1995: 306). His first results were published in the *Review of Economic Statistics* in 1937 (Leontief, 1937), and used in his seminal monograph on *The structure of American economy, 1919-1929* (Leontief, 1941).

It was around that time, in the 1940s, that the first courses on the input-output approach and the structure of the American Economy were proposed by Leontief (HUCI, 1953: 99; 1954: 106). Later on, especially in the 1960s, he delegated some of the responsibilities of these classes to other members of his staff; Gilboy, for instance, taught several iterations of the course on “The Economy of the United States” (HUCI, 1961: 99). She had joined the Harvard Economic Research Project (HERP) at its creation in 1948, becoming Associate Director in 1958 and eventually Acting Director in 1964-1965 (Thomas, 2000: 168).

Through the creation in 1948 of HERP, which he directed for 25 years, Leontief extended and developed what became known as input-output analysis, which proposed a very particular approach to the relation between economic theory and data analysis characteristic of econometrics. This approach became an important tool of applied economics and economic policy in the postwar period, as it was adopted almost immediately after World War II by the

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<sup>17</sup> Reproduced in Hendry and Morgan (1995: 22 ff.). See also Chipman (1998: 79-83) and Bjerkholt (2016: 41 ff.) on the issues at stake. Leontief also contributed to demand and supply studies with a paper on the cobweb mechanism presenting an analysis of nonlinear demand and supply schedules (Leontief, 1934).

<sup>18</sup> See Dorfman (1973: 433) for an appraisal of the paper and Bjerkholt (2016: 101 ff.) on the context of its publication in *Econometrica* in the aftermath of the pitfalls debate.

<sup>19</sup> The crowning achievement came in 1973 when he received the fifth Nobel Prize in economics “for the development of the input-output method and for its application to important economic problems” (Nobel citation available at [www.nobelprize.org/prizes/economic-sciences/1973/leontief/facts](http://www.nobelprize.org/prizes/economic-sciences/1973/leontief/facts), consulted in September 2021).

U.S. Bureau of Labor Statistics (Kohli, 2001). The Bureau began a fruitful collaboration with Leontief in the early 1940s, which led to the development of a large model with 400 sectors. Leontief also pioneered the application of input-output to solve problems in the estimation of employment (Leontief, 1944), international trade (Leontief, 1946a) or the analysis of inflationary processes (Leontief, 1946b), in addition to the countless applications to economic development that were made through the computation of input-output tables for many countries, which culminated in a study led by Leontief for the United Nations (Dorfman, 1973: 438-439).

The concern to develop an empirical approach to the economy led him to a singular econometric approach, which shunned most of the approaches of his contemporaries who remained for him too close to the “empty boxes of abstract, theoretical argument” (Leontief, 1948: 390).<sup>20</sup> He was a vocal critic of both probabilistic econometrics, and the fact that most of the data used by economists came from outside sources. His approach, central to the development of input-output analysis, was that of “direct observation,” based on the careful collection of empirical data about production processes.<sup>21</sup>

Leontief’s perspective on econometrics led him to have some original points of view about the debates that agitated the community of econometricians. For instance, about the famous “measurement without theory” debate between the Cowles Commission, arguing for a theoretical analysis of the data, and the NBER, advocating an inductive approach, Leontief took a third way: “The difference between the direct empiricism of the Mitchell-Burns reference cycle technique and the sophisticated statistical positivism of the Cowles Commission school should not be overemphasized. Both, although recognizing the importance of using ‘outside’ information, derive their explanatory schemes mainly from observations of the very same data which they are trying to explain” (Leontief, 1948: 407).

The core of his argument against the Cowles Commission approach to econometrics was that the identification problem could be better resolved through the use of direct observation whereas

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<sup>20</sup> See also Leontief (1936b: 105). Leontief (1948) was a survey of econometrics commissioned to him, which might seem like a strange choice as he rejected most of the contemporary use of statistical inference; but it should be remembered that the Cowles Commission approach to econometrics and the probabilistic framework developed by Haavelmo were still in their phases of construction. Leontief, through his position at Harvard and his sprawling input-output approach, was a major figure of a still emerging field.

<sup>21</sup> See also Leontief’s introduction to Leontief et al. (1953), where his issue with inferential econometrics is clearly presented. His methodology of direct observation has been discussed by Kohli (2001: 195-199) and was recently studied by Akhabbar (2021).

“[t]he Cowles Commission econometricians are inclined to minimize the practical significance of this type of empirical study” (Leontief, 1948: 398).<sup>22</sup> The heart of the matter was that for Leontief, economists should be able to do their own empirical work, in the same way that physicists, and in particular astronomers, have developed their science through the accumulation over centuries of carefully collected astronomical data, that eventually led to the major theoretical discoveries of the scientific “revolution” in the 16th and 17th centuries.<sup>23</sup> This led him to reject both abstract models and sophisticated statistical tools, a position he continued to defend forcefully in the 1970s.

At this time, although his work was widely recognized, a certain bitterness can be seen in his rejection of what had become the mainstream of econometric thought, at a time when he was facing the fact that econometrics had taken a path that he did not approve of. His 1970 Presidential Address in front of the American Economic Association was a scathing indictment not only of purely abstract models far-removed from any empirical reality, but also of contemporary econometric practice relying on complex statistical procedures: “Alongside the mounting pile of elaborate theoretical models we see a fast-growing stock of equally intricate statistical tools. These are intended to stretch to the limit the meager supply of facts” (Leontief, 1971: 2-3). Leontief scorned the value scale that he believed ranked empirical analysis below “formal reasoning” and scoffed at the fact that “[d]evising a new statistical procedure, however tenuous, that makes it possible to squeeze out one more unknown parameter from a given set of data, is judged a greater scientific achievement than the successful search for additional information that would permit us to measure the magnitude of the same parameter in a less ingenious, but more reliable way” (Leontief, 1971: 3).

It may have been that, in spite of all his apparent success, Leontief saw a certain rejection from his peers of his empirical approach; in his 1982 *Science* article, he complained that the present state of econometrics “is likely to be maintained as long as tenured members of leading economics departments continue to exercise tight control over the training, promotion, and research activities of their younger faculty members, and by means of peer review, of the senior

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<sup>22</sup> This critique echoes in the past Leamer’s (1983) critique, and Leontief’s emphasis on better data collection and a better research design similarly parallels the solutions that were proposed through the quasi-experimental approach (Angrist and Pishke, 2010: 106), without the statistical apparatus that was developed with it (see section VI).

<sup>23</sup> A remark he made in 1982 in *Science* (Leontief, 1982: 104); see also the same point made by Dorfman (1973: 436) about Leontief.

members as well” (Leontief, 1982: 107). His disappointment may have also arisen from the fact that the new generation of econometricians who questioned the Cowles Commission approach to econometrics did not seem to go in the direction that he advocated. In 1982, when he discussed in his *Science* paper the ‘translog’ production function of his Harvard colleague, Dale Jorgenson (see section IV of this chapter), he deplored that “while the labels attached to symbolic variables and parameters of the theoretical equations tend to suggest that they could be identified with those directly observable in the real world, any attempt to do so is bound to fail” (Leontief, 1982: 104). The dynamic model that he tried to develop was also criticized by Denis Sargan (1958), who argued that it was unstable such that “the Leontief dynamic model is not adapted to explaining the actual movements of the economic system and it would be better to regard the Leontief system as strictly a planning system. This conclusion is purely negative, and this is as far as we can go on purely a priori grounds” (Sargan, 1958: 392). Another obstacle that was faced by Leontief was more of a political nature: while his work in the 1940s was closely associated with the Bureau of Labor Statistics and other agencies, especially military, the Eisenhower administration deemed the input-output approach too conducive to central planning and eliminated all fundings toward it (Kohli, 2001: 207-208).<sup>24</sup>

Leontief’s influence on econometrics may have not been as important as he would have liked it, but he certainly exerted a decisive influence on postwar economics and econometrics, through the development of the input-output approach as well as through the formation of several generations of economists who had a deep influence at Harvard and elsewhere. His PhD students include (among others) Robert Triffin (1939), Abram Bergson (1940), Paul Samuelson (1941), Hollis Chenery (1950), Thomas Schelling (1951), Robert Solow (1951), Vernon Smith (1955), Richard Quandt (1957) and Dale Jorgenson (1959).<sup>25</sup>

### **III/ The development of the quantitative approach to economics at Harvard during the 1950s-1960s**

The 1950s were a pivotal decade in the transition of the Harvard Economic Department from interwar economics to the quantitative approach that came to define the postwar period. The

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<sup>24</sup> The use of input-output tables in government agencies was however resumed with the Kennedy administration (Kohli, 2001: 191).

<sup>25</sup> According to the Mathematics Genealogy Project, he has had 21 students and 3518 ‘descendants’ ([genealogy.math.ndsu.nodak.edu/id.php?id=137087](http://genealogy.math.ndsu.nodak.edu/id.php?id=137087), consulted in September, 2021).

recruitment of a young and dynamic faculty was key to this transition, and it led to many institutional and organizational changes in the department. In the late 1940s, most statistical courses in the economic department were still taught by Frickey, the last link to the economic barometer approach of the 1920s. In addition to two graduate classes on the “Theory of Economic Statistics,” there was one “Introduction to Economic Statistics” for undergraduates, which was billed as a “non-mathematical course in statistical material and methods” concerned with the collection and charting of economic data. Schumpeter was giving the class on “Business Cycles and Economic Forecasting,” which subsequently became the territory of Hansen and Haberler who alternated teaching the class in the Fall and a seminar on the same subject in the Spring semester during most of the 1950s.

In the late 1940s, Richard Goodwin took charge of the class on mathematical economics which had been taught by Leontief since the mid-1930s; while the latter had made few references to the emerging econometric literature, Goodwin, who had already given an “Introduction to Mathematical Business Cycle Theory” in the Spring semester of 1944, left a large place to accelerator-multiplier models and discussions of the Tinbergen-Keynes debate about the econometric approach. During the Fall semester of 1950 he was discussing in his mathematical economics class the micro- and macro-dynamical economic systems that had been developed and discussed by the early econometricians.<sup>26</sup> The content of this mathematical course was subsequently transformed throughout the 1950s, as the center of mathematical economics transitioned from the works of the early econometricians to the postwar development of linear programming, input-output and game theory.

More important for econometrics was the arrival at Harvard of Guy Orcutt, who spent almost a decade teaching empirical and econometric methods at Harvard before he went on to develop his own branch of applied economics, microsimulation, at the University of Wisconsin, Madison. Compared to Frickey’s class, his “Introduction to Econometrics” given in the Spring semester of 1950 was definitely a mathematical course.<sup>27</sup> In the Fall semester of 1951, this class was

<sup>26</sup> HUCI (1950: 83). See also Collier (2019), and on the development of those models, especially through the meetings of the Econometric Society during the 1930s, see Assous and Carret (2022). A 1951 profile of Goodwin for *The Harvard Crimson* shows how little consideration was still given to mathematical economics: “his [Goodwin’s] position in academic life has been risky; he has chosen to pioneer in a field whose worth has yet to be demonstrated. Some economists fear that the use of mathematical symbols in a science of human behavior is a sterile departure from reality” (Ellsberg, 1951).

<sup>27</sup> “The matter will be presented in order of increasing mathematical difficulty. Only simplified models will be used to familiarize students with the econometric approach: and to complete their knowledge of mathematical tools

considerably expanded: the term “econometrics” was abandoned for “Empirical Economics,” with a first part on “National Income and Business Fluctuations” focusing on the methods of forecasting and a second part planned for the following year on “The Price Mechanism” (HUCI, 1950: 84). As Collier (2016a) reported, the classes discussed works of the Cowles Commission (Klein, Koopmans), Leontief’s input-output approach, as well as the NBER approach of Burns and Mitchell, Tinbergen’s League of Nations work and many others that were also referenced in Orcutt (1950), a paper he published in the *Review of Economics and Statistics*.

His paper about a “partial redirection of econometrics” (Orcutt, 1952b) is symptomatic of the changes that were going on in the field and Orcutt’s particular position; in it, he argued in favor of reorienting econometrics from forecasting the future to guiding an instrument of adjustment toward an actual goal. With reference to the engineering approach of control theory, he argued that the problem was not so much to predict but to diminish “the discrepancy between the actual and the desired” (Orcutt, 1952b: 195), in particular by identifying controllable, exogenous variables.<sup>28</sup> Overall, Orcutt became a critic of the Cowles approach to simultaneous equations modeling, an opinion which he voiced in several book reviews and papers during the 1950s,<sup>29</sup> and it was these disagreements which led him to try and develop his own approach while he was still at Harvard, and afterward at the University of Wisconsin, Madison (Cheng, 2020: 196 ff.).

Frickey continued to give his undergraduate and graduate statistics classes until 1951-1952, after which he departed and his class was supplemented by a much more developed class on the “Principles of Statistical Inference” which was taught outside of the Economics department by Orcutt and two other new recruits, Philip Rulon and Frederick Mosteller. The latter was listed by the department in Tintner’s 1954 survey of econometrics courses (Tintner, 1954: 95), but he was above all a statistician who went on to create Harvard’s statistics department in 1957.<sup>30</sup> This course became a prerequisite to enter the “Introduction to Economic Statistics” that Orcutt began teaching in the Spring semester of 1953; Orcutt also took charge of the graduate courses in the “Theory of Economic Statistics,” while a research seminar on quantitative economics was put needed in quantitative economic analysis” (HUCI, 1949: 79).

<sup>28</sup> Orcutt was heavily influenced by the engineering approach to economics; Leamer (1983: 31) attributed to him the quip that “[d]oing econometrics is like trying to learn the laws of electricity by playing the radio.”

<sup>29</sup> Orcutt (1951; 1952a). Orcutt (1952b), previously cited, was followed by comments from Koopmans, Tinbergen and Nicholas Georgescu-Roegen.

<sup>30</sup> Tintner’s survey still defined econometrics as “the application of mathematical economic theory and quantitative statistical methods to economic problems” (Tintner, 1954: 86). Tintner concluded from the survey that “in spite of the progress made in recent years econometrics is still far from being a popular subject” (Tintner, 1954: 85).

into place by himself, James Duesenberry and John Chipman (HUCI, 1952: 103). Chipman had been recruited after Goodwin's departure in 1951, and his mathematical courses initiated a shift toward general equilibrium approaches characteristic of the postwar period. His first course was concerned with "General interdependence systems; in particular, Leontief linear systems" (HUCI, 1951: 80-81), and the syllabus was divided between the "static Leontief model," "dynamic models," and the problem of the "allocation of resources" (Collier, 2020b).

The development of econometrics at Harvard entered a steady growth during the following years. Chipman began teaching the first "Econometrics" class in the Fall semester of 1953,<sup>31</sup> and this class became a full course named "The Construction and Testing of Econometric Models" the following year. While only a few years before, Orcutt did not have any textbook to rely on,<sup>32</sup> by the time Chipman created his lecture he had at least three textbooks at his disposal from Tinbergen (1951), Tintner (1952) and Klein (1953).<sup>33</sup> The outline of his class has a more familiar look to the modern econometrician, with five sections on specification, identification, estimation, verification and prediction. The same year saw the introduction of a class on "Statistical Methods in Economic Research," divided into three courses taught by Orcutt during the Fall and Spring semesters (HUCI, 1954: 110).<sup>34</sup>

Chipman left in 1955 and was replaced by Robert Dorfman who taught the undergraduate introductory statistics classes and took up the graduate mathematical economics class that had been split the previous year between "Neo-classical and Keynesian theory" in the Fall semester and "Operations Research" in the Spring semester. The faculty was also joined by John Meyer, a PhD student of Duesenberry and Orcutt, who started in the Fall of 1955 a course on "Quantitative Research on the Behavior of the Firm" based on modern statistical inference and data collection (HUCI, 1955: 92). Meyer and Edwin Kuh, another student of Orcutt and Duesenberry, had both defended in 1955 what amounted to the first dissertations in econometrics

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<sup>31</sup> The program describes the course as follows: "The construction and statistical application of stochastic economic models. Identification and estimation of parameters of a system of equations. Testing of econometric models, prediction and economic policy" (HUCI, 1953: 102).

<sup>32</sup> He referred to Marschak's mimeographed lectures "Introduction to Econometrics" (Collier, 2016a).

<sup>33</sup> See Collier (2021) for the reading list and the outline of the course, and Quandt (1960) for a comparison of these textbooks. We will see later that Harvard did get a hand in the construction of the second generation of econometrics textbooks.

<sup>34</sup> The same class became simply "Quantitative Methods" the next year and remained on the program through the following decades.



at Harvard, both concerned with problems of investment decisions.<sup>35</sup> The same year, Meyer joined the faculty of the Department of Economics at Harvard while Kuh joined the faculty of the M.I.T. Sloan School of Management as an associate professor. Shortly after, they collaborated on a book entitled *The Investment Decision: An Empirical Study* (1957) in which, on the basis of a cross-section analysis, two sets of determinants related to the productive capacity and liquidity of firms were tested.

Their works showed a certain turn toward microeconometrics, which was accentuated by Orcutt, whose program of microsimulation began to take shape at this time. Nevertheless, two of the three courses on quantitative methods were taught by Robert Solow in 1956-1957, the same year that Jan Tinbergen was invited at Harvard to give several classes and seminars in quantitative economics and business cycle analysis (HUCI, 1956: 92-93). Another addition to the faculty during this year was that of Stefan Valavanis, who took over the econometrics class. Valavanis and John Johnston, who was an ephemeral assistant professor at Harvard during the year 1957-1958, both authored econometric textbooks (Valavanis, 1959; Johnston, 1963).<sup>36</sup>

The year 1957-1958 marked both the apex and the end of this decade of development of quantitative economics and econometrics at Harvard during the 1950s; Mosteller created the statistics department where he was joined by Howard Raiffa, who taught a class on the “Theory of Statistical Decisions” (HUCI, 1957: 314) and began to develop his work in decision theory and operations research. The following year, Orcutt received an offer from the University of Wisconsin and he took Johnston with him, effectively leaving a giant hole in the department (Meyer was apparently also absent this year). The gap was mostly filled by Hendrik Houthakker, as a visiting professor from Stanford University, who took care of the classes on “Quantitative Methods” which were the only econometric courses left in 1958-1959. There still seem to have been some uncertainties on the recruitment during the following year, and during the Spring semester of 1960, the econometrics course was taught by Henri Theil (invited from the Netherlands School of Economics).

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<sup>35</sup> Kuh’s dissertation was titled “An Econometric Investigation of Accelerator and Profit Theories of Investment” and Meyer’s was “Business Motivation and the Investment Decision: An Econometric Study of Postwar Investment Patterns in the Manufacturing Sector.”

<sup>36</sup> See Farrell (1965) for a review of those textbooks. Valavanis died tragically in 1958 and his death was deplored by his colleagues as a great loss for Harvard’s budding econometric group (Valavanis, 1959: vii). On the distinctions between the “textbook” approach and other econometric methodologies, see Hoover (2006).

After that, the econometric curriculum was settled around the quantitative methods courses and two courses on econometric methods taught by Meyer. Houthakker permanently joined the Harvard department of economics in 1960, and took care of several of those classes.<sup>37</sup> Strangely, in the late 1950s and early 1960s, the references that were given by Houthakker or Dorfman and Johnston during their econometric classes were not to the texts of Klein, Tinbergen or Tintner but rather to Dixon and Massey's *Introduction to statistical analysis*, perhaps a sign of Leontief's and Orcutt's influences in moving the department away from the Cowles Commission approach.<sup>38</sup> This reorganization signaled a stabilization of econometrics, at the same time that the "Mathematics for Economists" class became mandatory for PhD candidates (HUCI, 1960: 99). However, the excitement of the 1950s had conspicuously died down at Harvard, and the 1960s were very quiet compared to the previous decade. Houthakker, who received the Clark medal in 1963 and presided the Econometric Society in 1967, was the main driver of econometrics at Harvard, but the econometric courses were left to more junior faculty who did not make their marks there. The slowdown in econometric activity was felt by Zvi Griliches, who arrived at the end of the decade.<sup>39</sup>

The 1960s were nevertheless the zenith of the development of large macroeconomic models, and several Harvard economists were involved in those projects, such as Duesenberry who was a key figure in the Brookings model.<sup>40</sup> Another important advocate of this approach was Otto Eckstein, who produced one of the largest macroeconomic models while on the Harvard faculty. Eckstein had defended his PhD thesis about "Water-resource development" with Arthur Smithies in 1955 (Eckstein, 1958: viii), in a subject that subsequently became a central part of econometrics, "The Economics of Project Evaluation" (the subtitle of his thesis), approached through cost-benefit principles. His work on macroeconomic models began shortly after his thesis, with a paper co-written with Duesenberry and Gary Fromm (Duesenberry et al., 1960), in which they tested the stability of a macroeconomic model under different

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<sup>37</sup> Houthakker was also the editor of the *Review of Economics and Statistics* from 1972 to 1992.

<sup>38</sup> See for instance SCORLE, (HUC 8522.2.1), Box 8, Folder "Economics, 1960-1961" and Box 7, Folder "Economics, 1957-1958."

<sup>39</sup> "Before that [Griliches' arrival] there was no reasonable econometrics. You had to go back a decade earlier when John Meyer was teaching, and Ed Kuh was teaching up at MIT, but there wasn't that much going on here at the time" (Krueger and Taylor, 2000: 179).

<sup>40</sup> We thank Kevin Hoover for calling our attention on this point.

simulated scenarios.<sup>41</sup> He continued to build his model during the 1970s, with the help of several people including Martin Feldstein, another well-known Harvard economist (Eckstein et al., 1974: 595). The model he built (Eckstein et al., 1974) and its forecasts were eventually commercialized through the firm he set up, Data Resources Inc. (Backhouse and Cherrier, 2017: 108-109). Eckstein remained a proponent of the approach throughout the 1970s-1980s, at a time when it was being challenged and new econometric tools were being developed; his optimism paid off, as he sold his company in the late 1970s for more than 100 million dollars (Bendheim, 1979).<sup>42</sup>

It remains that, during the 1960s, most of the graduate econometric courses were taught by invited professors and young scholars who went on to have or had already distinguished careers elsewhere, such as Phoebus Dhrymes who was assistant professor in 1962-64, Edwin Mansfield visiting in 1963-64, Orcutt visiting in 1965-66 from the University of Wisconsin, Chipman in 1966-67 from Minnesota and Mark Nerlove in 1967-68 from Yale. It was only at the end of the decade that a series of new recruits opened the door again to develop econometrics at Harvard, in particular with the arrival of Griliches and Dale Jorgenson who became the pivots of the quantitative wing of the department.

#### **IV/ Mastering the territory between economics and statistics in the 1970s**

The year before Griliches and Jorgenson arrived at Harvard, the department was joined by Kenneth Arrow and Christopher Sims (promoted from PhD student advised by Houthakker to assistant professor). Sims taught the graduate “Econometric Methods” course and the change in program is noticeable from the previous years.<sup>43</sup> Arrow inaugurated a course during the Spring semester on the “Theory and Estimation of Production Functions” (HUCI, 1968: 133), done conjointly with Chenery. The following year (1969-70), they were joined by Griliches and Sims and the course became a year-long research seminar, while Griliches and Jorgenson took up the old Econ. 226 which had been turned into a “Research Seminar in Econometrics” only during the

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<sup>41</sup> This work was financed by HERP (Duesenberry et al., 1960: 749). In the early 1970s, Eckstein was also in charge of the econometric seminar (HUCI, 1972: 199), which shows the hold that macroeconometrics still had.

<sup>42</sup> His good fortune was short-lived; he died in 1984 at the age of 56.

<sup>43</sup> The course summary reads “The theory of stochastic processes with applications to the construction and testing of dynamic economic models. Analysis in the time domain and in the frequency domain, in discrete time and in continuous time” (HUCI, 1968: 133). Before that, the courses (usually taught by invited professors) were always some variations on “Introductions to...”

previous year.<sup>44</sup> Griliches and Jorgenson took charge of the “Econometric Methods” courses, a class they continued to teach regularly.

Griliches had a somewhat unusual trajectory; born in 1930 in Lithuania, he suffered the war in all its atrocity, and it was only at the end of a “harrowing journey” that he ended up in an Israeli Kibbutz in the late 1940s (Krueger and Taylor, 2000: 171 ff.). His breakthrough came when he went to the University of California (UC), Berkeley, where he studied agricultural economics, a subject conducive to empirical studies who has maintained a close relationship with econometrics since the inception of the latter.<sup>45</sup> Griliches then moved on to the University of Chicago where he rubbed shoulders with the Cowles Commission before its departure to Yale, and obtained his PhD in 1957. He remained on the faculty of Chicago until 1969, at which point he was offered a position at Harvard to fill in the lack of econometric faculty (Krueger and Taylor, 2000: 179).

When he arrived at Harvard, he was already a well-recognized economist, having received the Clark medal in 1965 for his work on technological change. One of his most important empirical works in Cambridge came after his presidential address at the Third World Congress of the Econometric Society in 1975, which was published in a 1977 paper where he measured the returns to education (Griliches, 1977). Griliches continued this line of work at Harvard with another paper written with William Mason, “Education, Income, and Ability” (Griliches and Mason, 1972), where they measured the interaction between education and ability and its effect on economic growth. As underlined by Nerlove (2001), it was Griliches’ interest in the proper measurement of ability which eventually led him to develop an econometric methodology able to deal with unobservable variables, and which influenced the development of panel data econometrics. Griliches was also very concerned by the collection of data, the “data infrastructure” (Krueger and Taylor, 2000: 183 ff.), something which must have pleased Leontief.

In contrast to Griliches, Jorgenson was an insider who defended his PhD at Harvard with Leontief in 1959, before making a name for himself at UC Berkeley in the 1960s. Two years

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<sup>44</sup> It was known as the seminar on “Quantitative Economics and Mathematical Economics” since it had been put into place by Orcutt, Duesenberry and Chipman in the early 1950s (see the lists of courses, in particular HUCI [1952: 103; 1963: 109; 1968: 133; 1969: 146]. The “and Mathematical Economics” was added in 1963).

<sup>45</sup> “That’s where I got into econometrics. Agricultural economics was then the main quantitative applied side of economics” (Krueger and Taylor, 2000: 174). See also Fox (1988).

after returning at Harvard, he received the Clark medal for his work in economic theory and statistical methods, with the citation presenting him as “a master of the territory between economics and statistics.”<sup>46</sup> The same year, he published a widely influential survey of the econometric theories of investment behavior that had been developed in the 1960s (Jorgenson, 1971). These studies had been initiated at Harvard by Chenery (1952) and the work of Meyer and Kuh during the 1950s, as Jorgenson noted in the survey.<sup>47</sup> Jorgenson underlined how they had contributed to “the development of new econometric techniques for representation of the time structure of economic behavior” (Jorgenson, 1971: 1143), and these works were far from over and occupied much of his own research in the following years.

While at Harvard, he pursued this line of research through the development of econometric models of producer behavior, which led to the development of econometric estimation techniques summarized in Jorgenson (1986). So far, statistical tests of the theory of production were run with reference to the constant elasticity of the substitution production function displaying additive and homogeneous properties, but this function proved problematic in the presence of more than two inputs or two products. In collaboration with Laurits Christensen and Lawrence Lau, two of his former PhD students at UC Berkeley, this led Jorgenson to address that issue by elaborating a new class of econometric models of production based on the transcendental logarithmic (so-called ‘translog’) price possibility frontier (Jorgenson, 1972; Christensen et al. 1973). In support of this modeling work, he developed statistical methods aimed at estimating nonlinear models of simultaneous equations (Jorgenson and Laffont, 1974), which had a durable influence on the modeling of producer behavior.

Related and parallel to this work in production theory, he also developed with his students the translog utility function modeling consumer behavior (Christensen et al., 1975), which was applied on American data in Jorgenson and Lau (1975). This approach had a considerable effect on econometrics, in spite of Leontief’s doubts (Leontief, 1982: 104). Jorgenson’s contributions in this line continued afterward in empirical studies of economic policies (Hoffman and

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<sup>46</sup> The citation is available on the AEA website (consulted in September 2021): [www.aeaweb.org/about-aea/honors-awards/bates-clark/dale-jorgenson](http://www.aeaweb.org/about-aea/honors-awards/bates-clark/dale-jorgenson).

<sup>47</sup> “The point of departure for the large body of empirical research on investment behavior during the past decade has been the flexible accelerator model of Chenery and Koyck” (Jorgenson, 1971: 1141). Chenery’s paper was an outcome of HERP and had been discussed in Harvard seminars during the late 1940s (Chenery, 1952: 1). Chenery gained another title to fame in the early 1970s as the owner of Secretariat, when the latter won the American Triple Crown.

Jorgenson, 1977; Hudson and Jorgenson, 1978), consumer preferences (Conrad and Jorgenson, 1978), and economic growth (Jorgenson and Nishimizu, 1978). This work on growth was particularly developed during the following decades, as it led him to a new accounting of economic growth (Jorgenson et al., 1987) and eventually to his interpretation of the 1990s growth resurgence through the study of investment in information technologies (Jorgenson, 2001), both approaches which were developed in the volumes on *Productivity* about the information age and its influence on American growth (Jorgenson et al., 2005).<sup>48</sup>

While Sims left almost immediately for the University of Minnesota, where he spent the next twenty years developing his own approach to econometrics, Arrow, Griliches and Jorgenson maintained the activity of the department in econometrics. In 1970-71, they were joined for some years by Edward Leamer, who inaugurated a research seminar about the “Bayesian Analysis of Economic Data” (HUCI, 1971: 165) and taught the “Quantitative Methods I” course which had been reoriented toward “probability theory and statistical inference” (HUCI, 1970: 168), before he left for UCLA in 1975. During the decade, as the graduate curriculum settled around the core courses of quantitative and econometric methods, and the research seminar on econometric modeling, the department was again joined by junior faculty and visiting professors who made important contributions to the discipline, such as Truman Bewley (assistant professor, 1972-1978), Gary Chamberlain (assistant professor, 1975-1979, before he came back in 1987 as full professor) or Lawrence Lau (visiting from Stanford in 1978-79).

Griliches and Jorgenson also played a pivotal role in training the new generations of econometricians at Harvard; Griliches was the advisor of many PhD students who went on to have distinguished careers, including Robert Barro (1970), Gary Chamberlain (1975) and Ariel Pakes (1979), who all made most of their career at Harvard, and Jorgenson has had more than 52 PhD students at Harvard.<sup>49</sup>

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<sup>48</sup> For a complete list of Jorgenson’s publications, which we can only briefly summarize here, the reader is referred to his Harvard webpage: [scholar.harvard.edu/jorgenson/publications](https://scholar.harvard.edu/jorgenson/publications), consulted in September 2021.

<sup>49</sup> According to the mathematical genealogy project, [genealogy.math.ndsu.nodak.edu/id.php?id=201549](https://genealogy.math.ndsu.nodak.edu/id.php?id=201549), consulted in September 2021.

## **V/ New sources and new powers: panel data, time series and the rise of computations since the 1980s**

At the end of the century, several Harvard economists contributed to the quickly developing field of time series analysis. In the early 1980s, Mark Watson was recruited in the Harvard Economics Department, first as assistant professor (1980-84) and then as associate professor (1984-1986), and soon after he created the course on “Time Series Methods in Econometrics,” which eventually became “Econometric Methods III” in the Spring semester of 1984 (HUCI, 1983: 231). Topics addressed in the course included the emerging toolbox of econometrics, “Box-Jenkins method, spectral analysis, state-space model, seasonal adjustment, forecasting, multivariate time series models; causality and distributive lags” (HUCI, 1982: 308).

After Watson’s departure, the course was given by Lars Hansen who was visiting for a year in 1986-87 (HUCI, 1986: 268) and then by James Stock who was recruited by the Kennedy School of Government (and by the department of economics after 2002), and the themes of the course evolved to integrate the most recent advances on time series analysis, most notably ARIMA and VAR models during the mid-1980s (HUCI, 1985: 248). During the 1990s, he incorporated to the course his own research (Stock, 1987; 1994; Sims et al., 1990; Stock and Watson, 1993) on cointegration (HUCI, 1991: 259) and structural breaks (HUCI, 1993: 265). His course proved to be highly successful as it remains on the program of the Harvard graduate curriculum in econometrics to this day. We can also mention the work of Marcelo Moreira, assistant and associate professor at Harvard from 2002 to 2008, who worked on time series econometrics (Jansson and Moreira, 2006) and structural models (Moreira, 2003).<sup>50</sup> Stock and Watson also wrote an introductory textbook in econometrics in the early 2000s (Stock and Watson, 2003), which is now in its fourth edition, and Stock dabbled in stylometry with a study of Wright’s contribution to instrumental variable regression in the late 1920s (Stock and Trebbi, 2003).

Much of this macroeconomic work was accompanied and served by computational advances which pervaded all the econometric work done at the end of the century. Harvard distinguished itself in the development of the first computers, which were used from the onset in economics (Backhouse and Cherrier, 2017: 106 ff.). In the mid-1940s, it took 24 hours to compute the first

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<sup>50</sup> Like Stock, Moreira was a student of Thomas Rothenberg at UC Berkeley, who was himself a student of Franklin Fisher who had obtained his PhD at Harvard in 1960 with John Meyer, the student of Duesenberry and Orcutt.

ten by ten input-output matrix of Leontief on the so-called “Mark I” computer which was developed conjointly by IBM and Howard Aiken at Harvard University; at the end of the decade, it took almost two months for a 38 by 38 matrix on the “Mark II” (Mitchell, 2007). Econometrics played a role in the progress made by computers, as many students of Leontief were from the engineering department and contributed to programming the input-output system; one of the most distinguished of those students was Kenneth Iverson, who defended his thesis advised by Aiken and Leontief in 1954. In it, he presented the results of his use of the “Mark IV Calculator” (Iverson, 1954) to compute an input-output system described by a set of linear differential equations. In the late 1950s, there were already faculty meetings discussing the use of computer time and the replacement of the IBM-650 with more up-to-date machines (Collier, 2020a). And yet the final examination of Meyer’s “Econometric Methods” class in the Spring semester of 1961 could still be to discuss the affirmation that “[c]omputers are the most worthless and illiterate of clerical assistants for an economic research project” (Harvard University, 1961: 55).

From the late 1970s onward, computational tools took up more importance. During the Spring semester of 1979, the first course on “Computational Methods for Economists” was given in the economics department, with a goal toward applications based on the programming language FORTRAN (HUCI, 1978: 264). The course did not stick to the curriculum and it was not until the Spring semester of 1982 that computational issues explicitly reappeared in a course on mathematical optimization indicating the “use of computer to solve problems” (HUCI, 1981: 309). The use of computers durably made its way into the econometric methods course around 1986-87, when the course was taught by Christopher L. Cavanagh and Jorgenson, and the summary indicated “[a] series of computer exercises emphasizing data analysis given, using such packages as LIMDEP, RATS, and TSP” (HUCI, 1986: 268). The following year, those exercises were expanded in “Econometric Methods I,” also taught by Jorgenson who was in charge alone or jointly with another faculty member of most of those classes well into the mid-2000s. In the Spring semester of 1990, LIMDEP was replaced by SAS (HUCI, 1989: 259) and STATA was first mentioned in the late 1990s (HUCI, 1997: 258), with these last two tools still in use today.

In the mid-1990s, “computer packages” made their entry into the undergraduate curriculum with the new Econ. 1123, “Applied Econometrics” (HUCI, 1995: 256), and a few years later the graduate course summary for Econometric Methods I only mentions in passing the use of



“standard econometric packages” (HUCI, 1997: 258), showing how much the personal computer had already permeated the econometric culture. Through changes in the econometrics curriculum, those courses that made some use of computers tended to grow in number during the early years of the 21st century, in particular with discussion of applied econometric articles reproduced with “computer exercises based on these papers” (HUCI, 2000: 276).

Of course the increasing use of computers developed as an answer and as a consequence of the need to analyze bigger datasets that were becoming available at this time. From the late 1970s onward, panel data in particular came to take a major importance and were being discussed during Harvard’s research seminars, the courses in applied econometrics and in the research of the faculty. The Harvard econometrician perhaps most associated with panel data analysis was Gary Chamberlain, a student of Griliches who received his PhD in 1975 and immediately joined the faculty as assistant professor. However, Chamberlain left Harvard after four years as assistant professor, and became associate professor at the University of Wisconsin, Madison in 1979, where he did most of his pioneer work on panel data: during the following eight years, he produced seven of his ten most cited publications, something which must have influenced the decision to offer him a professorship at Harvard in 1987, that he retained until his retirement in 2018.

Nevertheless, while he was still on Harvard’s faculty, he published in 1979 as a NBER working paper a longitudinal analysis of labor market data which remains his most cited work (Chamberlain, 1979). It is no surprise then that the first time that longitudinal data are mentioned in the curriculum was in 1978-79 in the summary of Econ. 2262, “Special Topics in Econometrics,” a course created in the early 1970s by Jorgenson, and which Chamberlain was supposed to teach (the course was moved to the following year but did not seem to have happened since Chamberlain left).<sup>51</sup>

It was not until the mid 1980s that panel data reappeared on the curriculum, first at the graduate level through the discussion of the modeling of firms behaviors in their fiscal and financial environments, and the “econometric issues that arise in trying to use such data sources

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<sup>51</sup> Its summary read: “Methodology for micro data, particularly longitudinal data. Emphasizes statistical models for individual heterogeneity. Topics include qualitative choice models, latent variable models, and stochastic process mobility models. Empirical applications will focus on life-cycle approaches to labor market behavior” (HUCI, 1978: 266).

as the Compustat tapes and the Census Longitudinal Establishment files” (HUCI, 1984: 241). These discussions were animated during the “Workshop in Applied Econometrics of Firm Behavior” by Griliches, Lawrence Summers and other members of the department, and maintained for the next four years before they were integrated in Econ. 2230, “Quantitative Economics: Data Sources and Methods,” still centered on the Longitudinal Surveys of Labor Force Experience and other longitudinal data sets (HUCI, 1986: 268). After his return to Harvard in 1987, Chamberlain taught many of the undergraduate and graduate courses which discussed the methodological questions related to “the use of longitudinal data” (HUCI, 1993: 263) and the application of econometric theory to cross-section and longitudinal data (HUCI, 1992: 252). The importance of longitudinal data for the new type of econometrics that was being developed can be gathered from the fact that, starting from 1989-90, the term “panel data” alone appears in 65 different course summaries, in addition to the title of a new course on the “Analysis of Cross Section and Panel Data” given by Marcelo Moreira in 2003-2004 (HUCI, 2003: 308) and absorbed the following year under the title “Advanced Topics in Microeconometrics” (HUCI, 2004: 314).

This exponential spread of the interest in panel data is not accidental, but the cause and consequence of the shift of a large part of what was now the econometric profession toward quasi-experimental methodologies that entailed looking for so-called natural experiments, a transformation dubbed a “Credibility Revolution” by its proponents (Angrist and Pischke, 2010); longitudinal data were a perfect source of such experiments as they followed a population through time, and thus allowed researchers to identify the effects of policies that were adopted during the period surveyed.<sup>52</sup>

## **VI/ Econometrics graduating from handmaiden to maitre d’: quasi-experimental econometrics at Harvard since the 1990s**

In 2006, the editors of the *Palgrave Handbook of Econometrics* argued that: “the dominance over the last ten or twenty years of econometric developments in driving economics forward is a notable change from the situation faced by the subject some 25 years or so ago” when econometrics was still the “handmaiden” of economics (Mills and Patterson, 2006: xiii). The transformations of econometrics since the 1980s and its shift toward a new approach has been

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<sup>52</sup> See Panhans and Singleton (2017, esp. 138, 149) on these developments and the role of panel data in the development of quasi-experimental methodologies.

widely noted, and several Harvard econometricians have contributed to the new orientation. This shift was a consequence of the different crises afflicting econometric research during the 1980s, especially the problems concerning what constituted a valid causal inference. The content of econometrics accelerated a shift toward methods of statistical inference, away from the models that characterized it until the early postwar.<sup>53</sup> For instance the “Econometrics and Quantitative Methods” subcategory of the Harvard list of courses was dominated by mathematical models based on dynamic analysis or linear programming in the early 1950s, but they were progressively moved toward the subcategory of “Economic Theory” and by the 1990s “Quantitative Methods” had come to be completely associated with econometric courses and their tools.

At Harvard, this shift can be seen through the list of courses and the orientation of the faculty research. Although we don’t have the full list of papers that were presented at the econometrics seminar, we know that at some point in the early 1980s, it was conducted jointly with M.I.T.; this appears in the list of courses as well as in the acknowledgment of some of the most important papers of that era such as Halbert White’s seminal papers on the measurement of errors and the maximum likelihood estimator (White, 1980; 1982), which were both discussed at this seminar.<sup>54</sup> While it seems not to have always been the case that the seminar was joint, this practice resumed in the late 1990s and 2000s.<sup>55</sup> At a time when M.I.T. was host to some of the most innovative econometricians and economists leading the work in new methods of causal inference, the proximity between the two universities fostered the emergence of a new generation of economists who were instrumental in the new econometrics. For instance, Joshua Angrist and Jörn-Steffen Pischke were both at M.I.T. during the 1990s, as were Abhijit Banerjee (himself a Harvard PhD in 1988) and from the late 1990s onward, Esther Duflo. Michael Kremer (Harvard PhD, 1992), their Nobel partner and contributor to the experimental approach, spent the 1990s between the two institutions before settling at Harvard in the 2000s and 2010s. Similarly, Jack Porter got his PhD from M.I.T. in 1996 and spent the next eight years as assistant and associate

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<sup>53</sup> Panhans and Singleton (2017) detailed the context of this shift and how it led to the rise of quasi-experimental research in econometrics, and underlined (2017: 130) that this shift was one from models to methods.

<sup>54</sup> “I am greatly indebted to ... the participants of the University of Western Ontario and Harvard / M.I.T. econometrics workshops for helpful comments and suggestions” (White, 1980: 817), see also White (1982: 1).

<sup>55</sup> This can be seen for instance in Jeffrey Wooldridge curriculum vitae ([econ.msu.edu/faculty/wooldridge/jmwvita.pdf](http://econ.msu.edu/faculty/wooldridge/jmwvita.pdf), consulted September 2021), where several presentations are made at Harvard / M.I.T. starting in 1992-1993, whereas from 1986 to 1990 they only appear at Harvard.

professor at Harvard. Alberto Abadie graduated from M.I.T. in 1999 and joined Harvard at the turn of the century, before going back across the street in 2016.

Through the graduate courses, a picture of the emergence of new methodologies clearly appears. In the late 1980s, the new course “Topics on Econometrics” showed that the issues had been acknowledged and presented the solutions devised to solve them.<sup>56</sup> These developments continued through regular changes in the content of Econ. 2140x, a series of courses on econometric methods. For instance, in 1993-94, a new course on “Causal Inference in Economics and Statistics” (Economics 2140e) was inaugurated by Guido Imbens and Donald Rubin, and aimed to cover the new tools of quasi-experimental econometrics.<sup>57</sup>

Donald Rubin (Harvard PhD, 1970), was recruited in 1984 as professor by the department of statistics and retired in 2018; Guido Imbens was recruited in 1990 as assistant professor (1990-94) and associate professor (1994-97). It was during this time in the early 1990s that they produced some of their most recognized works, also in collaboration with Joshua Angrist, who had himself done a brief stint as assistant professor at Harvard in 1989-1991, before joining M.I.T. in 1996. Angrist was still at Harvard when he signed two of his most well-known papers, one of the first use of a natural experience with the draft lottery (Angrist, 1990), and another written with Alan Krueger (Angrist and Krueger, 1991) which marked the beginning of the explosion of analyses of natural experiences in econometric journals (Panhans and Singleton, 2017: 146). During the following decade, Harvard economists contributed to most of the debates surrounding the defining methodologies of quasi-experimental economics, randomized control trials, instrumental variables, difference-in-differences and regression discontinuity, in particular through the work of Angrist and Imbens (1991; 1995), Imbens and Angrist (1994) and Angrist, Imbens and Rubin (1996), or in the 2000s in Chamberlain and Imbens (2004) and Andrews et al. (2006).

Rubin has also played an important role in the more general statistics literature through his formulation of the problem of causal inference, leading to a framework addressing problems of

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<sup>56</sup> “Second half focuses on econometric methods insensitive to structural specification, e.g. robust, semi-parametric and non-parametric estimation methods” (HUCI, 1989: 260).

<sup>57</sup> “Approaches to casual (sic) inference in economics and statistics. Covers randomized experiments, observational studies with and without ignorable treatment assignment, instrumental variables and sensitivity analysis. A number of applications from economics are discussed, including returns to education and evaluation of training programs” (HUCI, 1993: 265).

identification and estimations of treatment effects (Rubin, 1974; 1990; see also Holland [1986] on Rubin's model of causal inference). Rubin and Imbens (visiting from UC Berkeley) reunited in the Fall semester of 2005 with a course on "Causal Inference in Statistics and the Social Biomedical Sciences" and Imbens returned as Professor at Harvard the following year, in charge of Econ. 2140, the consolidated course in econometric methods now centered on quasi-experimental methodology.<sup>58</sup> Imbens and Rubin (2015) eventually published one of the main textbooks of this approach, *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*, which built on the content of this class that had been discussed since the 1990s (Imbens and Rubin, 2015: xvii). Imbens was also the champion of randomized controlled trials and quasi-experimental approaches at the turn of the decade when their exponential spread among economists began to be contested (Imbens, 2010).

Another Harvard contributor to this literature has been Pakes, who obtained his PhD at Harvard in 1979 with Griliches and joined the faculty, on which he remains today, twenty years later. Some of the applications of his work, for instance to the auto industry, have been done while on the Harvard faculty (Berry et al., 2004), and he did contribute (Pakes et al., 2007; 2015) to the analysis of bounds in the models discussed by Imbens and Wooldridge (2009: 51). During the first decade of the century, he was in charge of the graduate course in "Advanced Applied Econometrics" with coauthor Jack Porter, another course that was representative of the shift that had taken over econometrics.<sup>59</sup>

## **Conclusion: the latest developments in Harvard econometrics**

Since the 2010s, the Harvard department of economics has seen the arrival of several econometricians who form today the core of econometric research and teaching at Harvard. In addition to Jorgenson, Stock and Pakes, economists identifying as econometricians in the department include in the early 2020s Neil Shephard, Isaiah Andrews and Elie Tamer.<sup>60</sup> It does

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<sup>58</sup> "Econometric methods for cross-section and panel data. Topics include generalized method of moments, empirical likelihood, instrumental variables, bootstrapping, clustering, treatment effects, selection bias, difference-in-differences, qualitative choice, quantile regression, nonparametric methods, and semiparametric methods" (HUCI, 2006: 323). Several courses had been merged in 2000-2001, apparently in an effort to simplify the offering of econometric courses.

<sup>59</sup> The course summary reads: "An introduction to the theory and application of recently developed econometric techniques used in advanced applied work. Simulation techniques as well as semiparametric and nonparametric tools will be studied in a variety of empirical contexts" (HUCI, 1999: 270).

<sup>60</sup> See the current webpage of the department at [economics.harvard.edu/economic-fields/econometrics](https://economics.harvard.edu/economic-fields/econometrics), consulted in September 2021.

not appear however that these economists form part of a larger project that could define the identity of current econometrics at Harvard.

Tamer joined the department in 2014, after stints at Princeton and Northwestern; his work has focused on statistical inference and identification problems (Kline and Tamer, 2016; 2018; Khan et al., 2016; de Paula et al., 2018). Shephard joined the Harvard faculty in 2013 as professor of economics and statistics. He is today the Head of the Department of Statistics at Harvard, showing that the links between the economics and the statistics Departments have been strengthened by the recent development in econometrics at Harvard and elsewhere, almost 65 years after Mosteller created the statistics department. He had already gained international attention for his work in the analysis of financial markets volatility and on signal processing before his arrival at Harvard, and since then his collaborations have followed similar successful lines of research (e.g. Lunde et al., 2016; Pakel et al., 2021), as well as more applied work in a subject pioneered by Griliches, returns to schooling (Britton et al., 2016). He also worked on randomized experiments and causality in time series econometrics, subjects which we saw were influential at Harvard during the last decades (Bojinov and Shephard, 2019).

The latest recruit of the econometric wing of the economics Department is Isaiah Andrews, who joined the faculty in 2018 and received in 2020 a MacArthur Fellowship “for his work overcoming statistical inference problems in empirical economics” (Chang and Hsu, 2020). His published work has tackled the problem of parameter sensitivity (Andrews et al., 2017), the issue of “publication bias” (Andrews and Kasy, 2019), as well as statistical problems arising from weak identification (Andrews et al., 2019). This problem actually had strong Harvard connections, as Stock had already done influential work on weak identification, in particular in several papers from the turn of the century (Staiger and Stock, 1997; Stock, 2002; Stock et al., 2002). Andrews was awarded the Clark medal in 2021 for his contributions “to econometric theory and empirical practice” which “have improved the quality, credibility, and communication of quantitative research in economics”<sup>61</sup> signaling that the econometric legacy of Persons, Leontief, Jorgenson et al. is ensured for the coming years.

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<sup>61</sup> From the citation of the AEA, consulted in September, 2021 ([www.aeaweb.org/about-aea/honors-awards/bates-clark/isaiah-andrews](http://www.aeaweb.org/about-aea/honors-awards/bates-clark/isaiah-andrews)).

## Archives:

Harvard University:

- Syllabi, course outlines and reading lists in economics, 1895-2003 (SCORLE)
- “Courses of instruction” of the Faculty of Arts and Sciences (HUCI),  
URL: [listview.lib.harvard.edu/lists/drs-467484628](http://listview.lib.harvard.edu/lists/drs-467484628)
- Report of the President of Harvard College (PR),  
URL: [guides.library.harvard.edu/c.php?g=638791&p=4471938](http://guides.library.harvard.edu/c.php?g=638791&p=4471938).

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