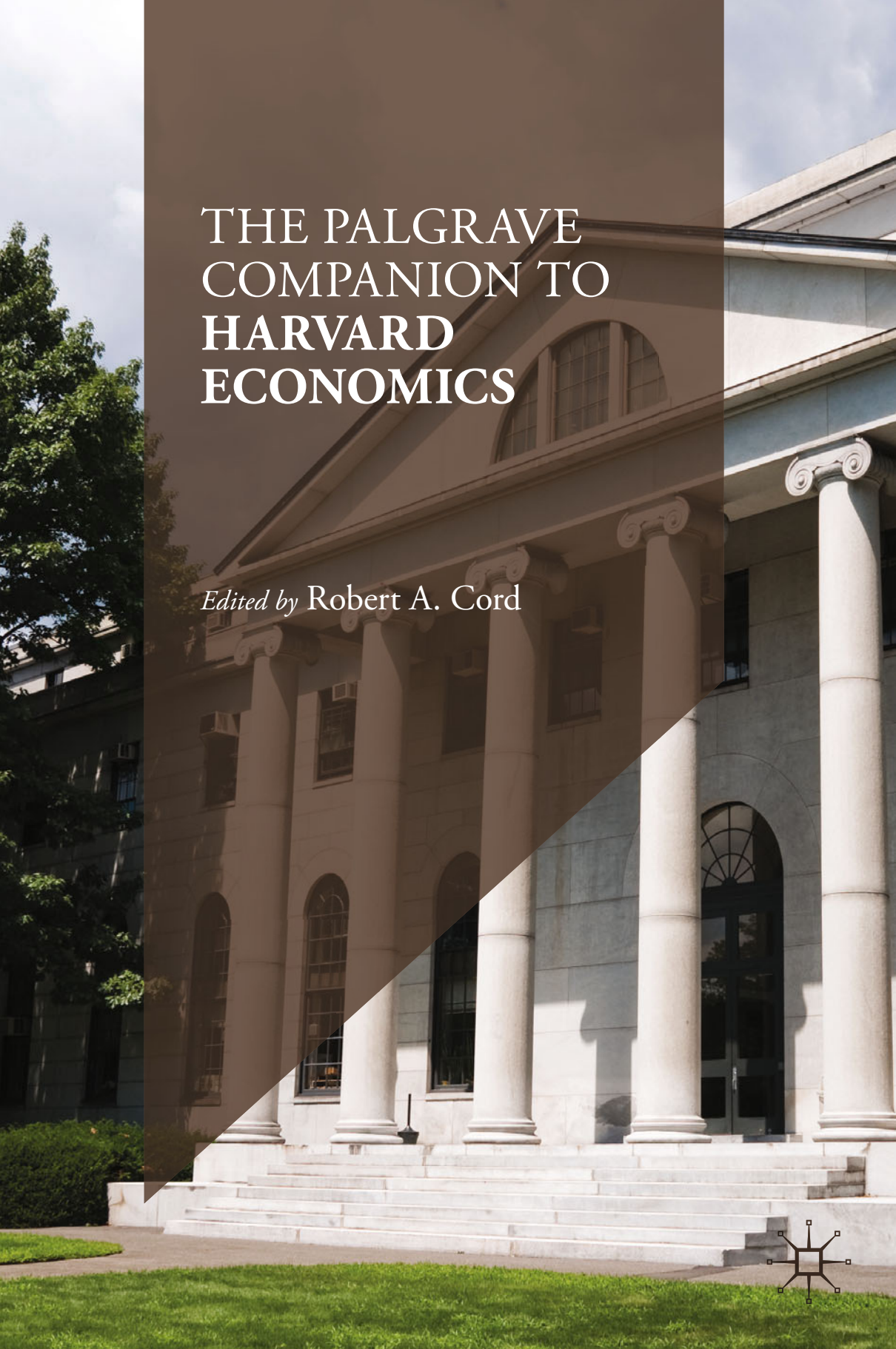

THE PALGRAVE COMPANION TO **HARVARD ECONOMICS**

Edited by Robert A. Cord

The Palgrave Companion to Harvard Economics

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For Christa and Jack

Introduction

These two volumes are about the economics and economists associated with Harvard University. They are the fifth in a series to be published by Palgrave examining the many and varied contributions made by important centres of economics. With only a very few exceptions, the focus of most history of economic thought studies, at least in terms of books,[1] has been on schools of thought. Such an approach provides valuable insights into how competing schools interact and how some come to predominate, for whatever reason and length of time, while others fall out of fashion or indeed never attain any particular notoriety. However, a key deficiency of such a *modus operandi* is that it often fails to illuminate the many processes and tensions that can and do occur at the level of the individual university, the personnel of which may be fighting internal battles for supremacy while at the same time trying to establish external hegemony.

Each volume in the series consists of two parts. The first contains a set of chapters which consider the contributions made by a centre where these contributions are considered to be especially important, this subject to a mixture of personal preferences and soundings from those who know better. The second, longer part is made up of chapters discussing the contributions of individual economists attached to a particular centre. "Attached" is the crucial word. Some economists are easy to identify with a single institution as they may, for example, have spent their whole academic careers at it. Those

[1] Articles are of course another matter.

who have moved from institution to institution are the more difficult cases. One way forward in these instances is to place an economist in the institution where they carried out their most important work, although this, in its turn, carries with it the danger of disagreement over what "their most important work" was or is perceived to be and how this has changed over time. Another factor perhaps worthy of consideration is an economist's education. Where such an education has been received at the knee of a master, to what extent has this influenced the subsequent work of the noted pupil and how should this be considered when that pupil has flown the nest and settled at another institution? Issues of leadership style, discipleship, loyalty, access to publication outlets and to financing also enter the frame. Finally, there are issues of practicality, including space constraints and unavailability of contributors, among others. Given this matrix of possibilities, disagreement about who should be in which volume is inevitable. However, I hope that the outrage will not be too great given the overarching goal of the series.

The next volume in the series will examine MIT.

Robert A. Cord

Contents

Volume II

List of Figures

List of Tables

Part I

Themes in Harvard Economics

1

Econometrics at Harvard

Vincent Carret and Michaël Assous

1 Introduction[1]

Two questions can be asked of a chapter tracing the contributions of Harvard economists to econometrics: What role did Harvard econometricians played in respect of the wider evolution of the field, and how did their contributions reflect the changing meaning of econometrics?

As to the first question, there is some difficulty in delineating Harvard's contributions to econometrics. Over the last century, there were periods of high creativity, often characterised by large projects which lasted one or two decades, and were followed by periods of relative apathy. But throughout, Harvard's role in the development of econometrics, expected to be that of a leader, was often one of a follower of current trends; its pioneers stood

[1] We thank Neil Shephard, Kevin Hoover, Roy Weintraub, Steven Medema and Samuel Demeulemeester for comments on this chapter, as well as participants at the Lunch Seminar of the Center for the History of Political Economy at Duke University. We also wish to thank Irwin Collier for communicating to us some archival material regarding the teaching of econometrics at Harvard. See the archives references at the end of the chapter for a list of the abbreviations used.

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somewhat outside of what is considered econometrics by contemporaries. One explanation of this state of affairs is that although many of the brightest economists passed through the University, few remained on a durable basis. 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 throughout 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 with the creation of the Econometric Society, there certainly was empirical work being done, and several Harvard economists participated in these pioneering studies. However, Ragnar Frisch and others came to define the subject of econometrics as the 'unification' of economic theory, mathematics and statistics (Frisch 1933: 2), so that earlier statistical work lost its econometric character for the new brand of econometricians.[2] The meaning of the term narrowed during subsequent decades, to the point where econometrics and economic theory became separated, lending more importance in the history of econometrics to the early statistical work that was performed without any relation to economic theory.

In the following, we will try to give an idea of what constituted econometrics at different points since the beginning of the twentieth century, and how Harvard economists contributed to this evolution, through the development of their research and through the courses they taught.

2 The Harvard Committee on Economic Research and the Construction of Economic Barometers (1910s and 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 carried out mostly through the work of the Harvard Committee on Economic Research (CER) on 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 since the

[2] The term was introduced for the first time by Frisch (1926: 1; italics in original), 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*'.

early 1920s (Mason 1982). In the early twentieth century, there was little teaching of quantitative issues, although there was a course in statistics.[3] In 1910–1911, Allyn Young gave the first course in mathematical economics as a visiting professor from Stanford. Mason noted 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' (ibid.: 404–405). However, Mason failed to mention the brief stay at Harvard of Philip G. Wright, who arrived in 1913 as Frank Taussig's assistant before becoming an instructor in economics. Although he was only at Harvard for four years before departing for the US Tariff Commission, Wright published several articles in the *Quarterly Journal of Economics*, including a review of Henry L. Moore's book on business cycles (Wright 1915), where he presented one of the first explanations of the identification problem in empirical economics.

The creation and development of the CER and the Harvard Economic Service (HES) that was put in place to publicise its work were by far the most important developments of the 1920s, especially with respect to the construction and diffusion of the barometer of business conditions, which served as a model for many other cycle research institutes.[4] The CER was established in 1917 by Charles J. Bullock, who became its first chairman. Warren Persons proved to be a crucial recruit after his arrival in 1918, and he was put in charge of economic forecasts as well as editing the *Review of Economic Statistics*, first published in 1919.[5] The Committee issued the *Review* as a quarterly publication, initially with monthly supplements which were replaced in January 1922 by a weekly letter on "economic conditions"; the HES, a private institution, was in charge of this latter publication (Friedman 2009: 66–67). These forecasting activities were not immediately profitable. Moreover, it was a sprawling operation which caused some unease among Harvard's alumni and officials, especially after the failure to predict the slowdown in 1924 and of course the 1929 crisis.

[3] '[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 started teaching 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.

[4] These institutes, which were modelled after the HES, 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).

[5] After the demise of the HES, the *Review* was taken over by the Economics Department and subsequently took its modern form of a leading generalist journal; it was eventually renamed in 1948 as *the Review of Economics and Statistics*. On Persons' career and his work at Harvard, see Foster (1939), Mason (1982), Morgan (1990: 56 sq.) and Friedman (2009).

Nevertheless, the work of the CER was important in the history of econometrics due to its empirical approach to cycles and the methods it pioneered. Persons' central idea was to separate what he identified as the four main components in time series, which he saw as acting together to create observed movements: the secular trend, seasonal fluctuations, cycle oscillations and accidental events. It has been argued elsewhere that the CER's main influence was its treatment of time series (Morgan 1990: 57, 63), because Persons described at length in the first issue of the *Review of Economic Statistics* his method for detrending time series and smoothing seasonal fluctuations. He proposed two approaches to detrending: moving averages and curve fitting, favouring the second method which gave an explicit measurement of the trend (Persons 1919a: 13).

There was less arbitrariness on the issue of seasonal fluctuations because the period of measurement was known, with the month 'adopted…as the most satisfactory unit' (ibid.: 18). The objectives were the same as in the case of trends: finding fluctuations in monthly data, measuring them and correcting them (ibid.). 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' (ibid.: 22). Finally, he examined "Cyclical and Irregular Fluctuations" together, concluding that they were virtually impossible to distinguish. The rest of his paper was dedicated to applying the pre-processing described above to fifteen time series, although this only formed the first part of his inquiry into business conditions.

The second part of his research was published in the next issue *of the Review of Economic Statistics*, and it described the process of constructing the Harvard barometer.[6] After he was able to isolate and standardise the cycle by correcting monthly series for the secular trend and seasonal fluctuations (Persons 1919b: 115), Persons' approach was to group together series that showed similar and simultaneous fluctuations. Although he only presented three curves (A, B and C) in the opening chart of "An Index of General Business Conditions", Persons actually created five groups, each separated by two to six months. The groups were based on pairwise comparisons between series, and Persons devised a method 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 (ibid.: 121). Persons' assistants recorded the size and direction of any correlations and the direction and extent of any lags and their consistency, before he computed the Pearson

[6] Persons had already partially described it in a previous paper published in 1916 before he arrived at Harvard (Persons 1916).

correlation coefficient on the pairs showing the most promise (ibid.: 127). Persons discussed briefly the problem of the statistical significance of the coefficient, although he felt that a detour through 'the intricacies of the mathematics of the theory of probability' was 'an unnecessary digression' (ibid.: 124). Nevertheless, he did spend the next three pages introducing basic elements of probability theory for statistical inference and 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' (ibid.: 114). It was those curves and their patterns of lags that were presented in the HES's monthly supplements and weekly letters to its subscribers when forecasting the evolution of the economy (Friedman 2009: 65ff.).[7]

The ideas put forward by Persons influenced the approach to dealing with time series data by early econometricians, even if they made little contribution 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 1919b: 115); this was contested by Schumpeter, who arrived at Harvard shortly after Persons 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 time series needed to be detrended was also contested by Wesley Clair 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' approach of decomposing the cycle did influence early econometricians such as Frisch, even though Frisch himself would end up analysing cycles with macro-dynamic models rather than through a data-driven approach.[8]

[7] 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 et al. (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 view to Slutzky (1937) on spurious cycles arising from statistical measurements, see Assous and Carret (2022: 10–12).

[8] See Carret (forthcoming) on this influence and how Frisch reinterpreted Persons' ideas in one of the first macro-dynamic models. Frisch referred to Persons during his Yale Lectures 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 Richard Goodwin's work on the nonlinear model of the business cycle (Goodwin 1951).

2.1 The Place of Quantitative Economics at Harvard in the 1930s

Through its new recruits, the CER was able to significantly 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 CER (Backhouse 2017: 107). Although Persons left in 1928 and Bullock retired in 1934 (Foster 1939: 412), Frickey and Crum remained as faculty members for several years (Mason 1982: 414). Frickey did not participate in the project of trying to unify economic theory and statistics as his preferred approach rejected any use of economic theory (Backhouse 2017: 107–108). Indeed, it was closer to the National Bureau of Economic Research (NBER) methodology associated with Mitchell and Arthur Burns, the latter of whom favourably reviewed Frickey's work (Burns 1944).[9] Crum was also more of a statistician who nevertheless engaged in debates around cycle theory, defending the legacy of the HES (Bullock and Crum 1932).

The CER availed itself of the services of Elizabeth Gilboy, who became its Secretary and a very active researcher during the inter-war period.[10] During 1930 to 1941, in addition to *Wages in Eighteenth-Century England* (Gilboy 1934a) and another about work relief (Gilboy 1940), Gilboy published around twenty articles in leading economics journals. Many of her works were related to the estimation of demand curves either through extensive reviews of the works published on the subject by others, such as those of Henry L. Moore (Gilboy 1930) and Jacob Marschak (Gilboy 1931a), and the debates between Wassily Leontief and Henry Schultz (Gilboy 1931b) and Leontief and Frisch (Gilboy 1933), or her own empirical studies that were part of a reorientation of the activity of the CER (Gilboy 1932a, b, 1934b, 1937).[11] 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

[9] The Harvard Economics Department was also associated with the NBER through the work of Edwin F. Gay, a leading economic historian at Harvard in the opening decades of the twentieth century who became the first president of the NBER (Mason 1982: 405ff.).

[10] On Gilboy's main works, see Trezzini (2016: 285–286). Gilboy obtained her PhD from Radcliffe College in 1929 under the supervision of Gay (Thomas 2000: 168) and quickly joined the CER.

[11] Gilboy's research 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 HES in the early 1930s, the CER seems to have pivoted towards studies of demand and other works, and continued to finance econometrics research, including Leontief's early forays into input–output in the 1930s (Leontief 1936a: 105; 1937: 109).

branch of economics in itself' (Gilboy 1932a: 376). Gilboy was also instrumental in bringing Leontief to Harvard in the early 1930s (see Section 3 of this chapter and Bjerkholt 2016: 46, 50).

Frickey and Crum continued to teach statistics at Harvard well into the 1940s, a course that had been 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' (Samuelson quoted in Backhouse 2017: 106; italics in original), preferring to follow the classes of Edwin B. Wilson. Wilson was Professor at Harvard's Institute of Public Health who gave courses in mathematical economics and statistics at the Economics Department (ibid.: 149), and who also made several contributions to econometrics using Moore's periodogram analysis on business cycle data (Wilson 1934a, b).

During his time as a junior member of Harvard's Society of Fellows in the late 1930s, Samuelson did not really improve the state of Harvard econometrics, as most of his papers written during this period were concerned with economic theory and cycle theory.[12] However, he did write a statistical study of the consumption function that was inserted as an addendum to Chapter XI of Alvin Hansen's *Fiscal Policy and Business Cycles* (Samuelson 1941), a work which was in line with many other wartime and immediate post-war studies trying to predict near-term economic activity in the US.

The class on mathematical economics that was taught during the 1930s had been supported by several members of the Department at the beginning of the decade, in particular, Schumpeter who was involved in the creation of the Econometric Society and was aware of the necessity to train students in the use of mathematical tools.[13] While he taught the first iteration of the course, Schumpeter was probably not a natural fit and he subsequently let Leontief teach it from the mid-1930s to the late 1940s.[14] It was only in the early 1950s that econometrics made a decisive entry into the curriculum

[12] It was not until the 1940s, after he moved to the Massachusetts Institute of Technology (MIT), that Samuelson started working on a statistical project that was continued by Lawrence Klein (Backhouse 2017: Chapter 17).

[13] See Schumpeter (1933), the first article published in *Econometrica*, entitled "The Common Sense of Econometrics". Hansen, who arrived at Harvard in 1937, was an associate editor of *Econometrica*, until he was replaced by Schumpeter in July 1938 (who himself stayed in this post until his replacement by John Hicks in January 1950). Afterwards, Leontief was for many years the most involved Harvard economist in the Econometric Society: he was a Council member between 1951 and 1954, was elected vice president in 1953 and president for 1954. In January 1959, he joined the editorial board of *Econometrica*, which he left in late 1967.

[14] Leontief replaced Schumpeter in 1935, while the latter replaced the retiring Taussig on the main course on economic theory (Backhouse 2017: 118–119). See Collier (2016a, 2018) on the early evolution of this course.

after Harvard recruited new faculty members who advocated its greater adoption. Moreover, it was also around this time that Leontief's own research project began to take off, and that he developed his own particular brand of econometrics.

3 The Harvard Economic Research Project: Leontief's Approach to Econometrics (1940s to 1970s)

The period of the early adopters of econometrics 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, focused on input–output analysis.

Before his arrival at Harvard, Leontief had made important breakthroughs during the late 1920s and in the 1930s. He gained international recognition with his work on the simultaneous determination of empirical demand and supply curves (Leontief 1929). His approach was criticised by Frisch and led to the so-called Pitfalls debate.[15] In the 1930s, Leontief also contributed to the theory of index numbers (Leontief 1936b) and how they could be used to measure a general price level.[16]

Leontief's 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 this gigantic undertaking and its successful realisation which took up much of his career and brought him fame and recognition.[17] Faced with a lack of data, the inadequacy of classical theories of production and a 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 initial results were published in the *Review of Economic Statistics* in

[15] Described in Hendry and Morgan (1995: 22ff.). See also Chipman (1998: 79–83) and Bjerkholt (2016: 41ff.) on the issues at stake. Leontief contributed to demand and supply studies with a paper on the cobweb mechanism, presenting an analysis of nonlinear demand and supply schedules (Leontief 1934).

[16] See Dorfman (1973: 433) for an appraisal of the paper and Bjerkholt (2016: 101ff.) on the context of its publication in *Econometrica* in the aftermath of the pitfalls debate.

[17] The crowning achievement came in 1973 when Leontief received the Nobel Prize in Economics 'for the development of the input–output method and for its application to important economic problems' (https://www.nobelprize.org/prizes/economic-sciences/1973/leontief/facts).

1937 (Leontief 1937) and used in his seminal monograph on *The Structure of American Economy, 1919–1929* (Leontief 1941).

It was 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 staff. For instance, Gilboy 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 HERP, which he directed for 25 years, Leontief extended and developed input–output, which proposed a very particular approach to the relation between economic theory and data analysis characteristic of econometrics. It became an important tool of applied economics and economic policy in the post-war period, adopted almost immediately after the Second World War by the US Bureau of Labor Statistics (BLS) (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) and 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 a variety of countries, culminating in a study led by Leontief for the United Nations (Dorfman 1973: 438–439).

The goal of developing an empirical approach to the economy led Leontief to a singular econometric approach, which shunned most of the methods of his contemporaries who remained for him too close to the 'empty boxes of abstract, theoretical argument' (Leontief 1948: 390).[18] He was a vocal critic of both probabilistic econometrics, and the fact that most of the data used by economists came from outside sources. Leontief's approach was that of "direct observation", based on the careful collection of empirical data about production processes.[19]

[18] See also Leontief (1936a: 105). Leontief (1948) was a survey of econometrics which he was commissioned to write. This might seem like a strange choice as he rejected most of the contemporary use of statistical inference. However, it should be remembered that the Cowles Commission approach to econometrics and the probabilistic framework developed by Trygve Haavelmo were still in their early phases. Leontief, through his position at Harvard and his sprawling input–output approach, was a major figure within a still emerging field.

[19] See also Leontief's introduction to Leontief et al. (1953), where his objections to inferential econometrics are clearly presented. His preferred method of direct observation has been discussed by Kohli (2001: 195–199) and was recently examined by Akhabbar (2021).

Leontief's views on econometrics led him to have some original insights on some of the debates that preoccupied the community of econometricians. For instance, with respect to the famous "measurement without theory" debate between Cowles, which argued for a theoretical analysis of data, and the NBER, which supported an inductive approach, Leontief adopted 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 overemphasised. Both, although recognising 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 Leontief's argument against the Cowles approach to econometrics was that the identification problem could be better resolved through the use of direct observation, whereas 'Cowles Commission econometricians are inclined to minimise the practical significance of this type of empirical study' (ibid.: 398).[20] 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, 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 sixteenth and seventeenth centuries.[21] 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 recognised, a certain bitterness can be seen in Leontief's rejection of what had become the mainstream of econometric thought, at a time when econometrics had taken a path that he did not approve of.[22] His 1970 Presidential Address at 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

[20] This was an early manifestation of Leamer's (1983) later critique, and Leontief's emphasis on better data collection and research design similarly paralleled the solutions that were proposed through the quasi-experimental approach (Angrist and Pischke 2010), without the statistical apparatus that was developed with it (see Section 7 of this chapter).

[21] A reflection of a remark Leontief made in 1982 in *Science* (Leontief 1982: 104). See also Dorfman (1973: 436) on Leontief.

[22] See Carret (2022) on Leontief's bitterness and his debates with the economists at Cowles, in particular Klein and Tjalling Koopmans.

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 (ibid.: 3).

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 members as well' (Leontief 1982: 107). When Leontief discussed the "translog" production function of his Harvard colleague, Dale Jorgenson (see Section 5 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' (ibid.: 104).

This disappointment may have arisen from the fact that the new generation of econometricians who questioned the Cowles approach did not seem to adopt Leontief's methodology. The dynamic model that he tried to develop was criticised 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 (ibid.: 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 BLS and other government agencies, especially military, the Eisenhower administration deemed the input–output approach to be too conducive to central planning and removed all of its funding (Kohli 2001: 207–208).[23]

[23] The use of input–output tables in government agencies was however resumed under the Kennedy administration (Kohli 2001: 191).

Leontief's impact on econometrics may have not been as significant as he would have liked, but he certainly exerted a decisive influence on post-war economics and econometrics, through the development of input–output as well as through the mentoring of several generations of economists important in their own right, both at Harvard and elsewhere. His PhD students included (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).[24]

4 The Development of the Quantitative Approach to Economics at Harvard During the 1950s and 1960s

The 1950s were a pivotal decade in the transition of the Harvard Economics Department from inter-war economics to the quantitative approach that came to define the post-war period. The recruitment of a young and dynamic faculty was key to this transition, and it led to many institutional and organisational changes in the Department. In the late 1940s, most statistical courses were still taught by Frickey, the last link to the economic barometer research of the 1920s. In addition to two graduate classes on the "Theory of Economic Statistics", there was one entitled "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 gave the class on "Business Cycles and Economic Forecasting", which subsequently became the territory of Hansen and Gottfried 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 (HUCI 1949: 80–81).

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, allowed for significant discussion of multiplier–accelerator models and the Tinbergen–Keynes econometrics debate. During the Fall semester of 1950, he was discussing

[24] According to the Mathematics Genealogy Project, Leontief had 21 doctoral students and has 3,537 "descendants" (consulted in May 2022).

the micro- and macro-dynamical economic systems that had been developed and discussed by the early econometricians.[25] The content of the mathematical economics course was subsequently transformed throughout the 1950s, moving from studying the works of the pre-war econometricians to the post-war development of linear programming, input–output and game theory.

More important for econometrics at Harvard was the arrival of Guy Orcutt, who spent almost a decade teaching empirical and econometric methods before he went on to develop his own branch of applied economics, microsimulation, at the University of Wisconsin–Madison. Compared to Frickey's class, Orcutt's "Introduction to Econometrics" given in the Spring semester of 1950 was clearly a mathematical course.[26] In the Fall semester of 1951, it was considerably expanded: "Econometrics" was abandoned for "Empirical Economics", with a first part on "National Income and Business Fluctuations" focusing on methods of forecasting and a second part planned for the following year on "The Price Mechanism" (HUCI 1950: 84). As Collier (2016b) noted, the classes discussed the work of Cowles (Klein, Koopmans), Leontief's input–output, the NBER approach of Mitchell and Burns, Jan 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*.

Orcutt's paper attempting a "partial redirection of econometrics" (Orcutt 1952a) was symptomatic of the changes that were going on in the field and Orcutt's particular position. In it, he argued in favour of reorienting econometrics from forecasting the future to guiding an instrument of adjustment towards 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' (ibid.: 195), in particular by identifying controllable, exogenous variables.[27] Overall, Orcutt became a critic of the Cowles approach to simultaneous equations modelling,

[25] HUCI (1950: 83). See also Collier (2019), and on the development of these models, especially through the meetings of the Econometric Society in the 1930s, see Assous and Carret (2022). A 1951 profile of Goodwin in *The Harvard Crimson* shows how little consideration was still given to mathematical economics: '[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).

[26] 'The matter will be presented in order of increasing mathematical difficulty. Only simplified models will be used to familiarise students with the econometric approach: and to complete their knowledge of mathematical tools needed in quantitative economic analysis' (HUCI 1949: 79).

[27] 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'.

an opinion which he voiced in several book reviews and papers during the 1950s,[28] and it was these disagreements which led him to try and develop his own approach while he was still at Harvard and afterwards at Wisconsin (Cheng 2020: 196ff.).

Meanwhile, 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 one 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.[29] "Principles of Statistical Inference" became a prerequisite for "Introduction to Economic Statistics" that Orcutt began teaching in the Spring semester of 1953; he also took charge of a graduate course in the "Theory of Economic Statistics" and also established a research seminar on quantitative economics with 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 towards general equilibrium approaches characteristic of the post-war period. His first course was concerned with "General Interdependence Systems; In Particular, Leontief Linear Systems" (HUCI 1951: 80–81), with the syllabus divided between the "static Leontief model", "dynamic models" and the problem of the "allocation of resources" (Collier 2020a).

The development of econometrics at Harvard entered a steady growth phase over the following years. Chipman began teaching the first "Econometrics" class in the Fall semester of 1953,[30] and this class became a full course named "The Construction and Testing of Econometric Models" the following year. Just a few years before, Orcutt did not have any textbook to rely on;[31] by the time Chipman created his class he had at least three textbooks at

[28] Orcutt (1951, 1952b). Orcutt (1952a) was followed by comments from Koopmans, Tinbergen and Nicholas Georgescu-Roegen.

[29] 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 that 'in spite of the progress made in recent years econometrics is still far from being a popular subject' (ibid.: 85).

[30] The programme 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).

[31] He referred to Marschak's mimeographed lectures "Introduction to Econometrics" (Collier 2016b).

his disposal, namely Tinbergen (1951), Tintner (1952) and Klein (1953).[32] The outline of his class has a more familiar look to the modern econometrician, with sections on specification, identification, estimation, verification and prediction. The year 1954 also 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).[33]

Chipman left Harvard in 1955 and was replaced by Robert Dorfman who taught the undergraduate introductory statistics classes and also the graduate mathematical economics class that had been split the previous year between "Neoclassical 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 in the Fall of 1955 started teaching 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 at Harvard, each concerned with investment decisions.[34] In 1955, Kuh joined the faculty of the MIT Sloan School of Management as Associate Professor. Shortly after, he and Meyer 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 joint work displayed a certain turn towards microeconometrics, which was accentuated by Orcutt, whose programme of microsimulation was beginning to take shape. Nevertheless, two of the three courses on quantitative methods were taught by Robert Solow in 1956–1957, the same year that Tinbergen was invited to Harvard to give several classes and seminars in quantitative economics and cycle analysis (HUCI 1956: 92–93). Another addition to the faculty at this time was Stefan Valavanis, who took over the econometrics class. Valavanis and John Johnston, who was Assistant Professor at Harvard during 1957–1958, both authored econometric textbooks (Valavanis 1959; Johnston 1963).[35]

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[32] 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 play a part in the creation of the second generation of econometrics textbooks.

[33] The same class became simply "Quantitative Methods" in the next year and remained on the programme over the following decades.

[34] Kuh's dissertation was entitled "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".

[35] See Farrell (1965) for a review of these textbooks. Valavanis died tragically in 1958, his passing being a matter of deep regret for his colleagues and a great loss for Harvard's budding econometrics

The year 1957–1958 marked both the apex and the end of a decade of development of quantitative economics and econometrics at Harvard. 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 on decision theory and operations research. The following year, Orcutt accepted an offer from the University of Wisconsin and took Johnston with him, leaving a giant hole in the Economics Department (Meyer was apparently also absent during that year). The gap was mostly filled by Hendrik Houthakker, Visiting Professor from Stanford University, who took care of the classes on "Quantitative Methods" which were the only econometric courses left in 1958–1959. During the Spring semester of 1960, the econometrics course was taught by Henri Theil (invited from what was then the Netherlands School of Economics).

Following this, the econometrics curriculum settled around the quantitative methods courses and two courses on econometric methods taught by Meyer. Houthakker permanently joined Harvard in 1960 and took care of several of those classes.[36] Strangely, in the late 1950s and early 1960s, the references that were given by Houthakker or Dorfman and Johnston during their econometrics classes were not to the texts of Tinbergen, Tintner or Klein but rather to Dixon and Massey's *Introduction to Statistical Analysis*, perhaps a sign of Leontief's and Orcutt's influence in moving the Department away from the Cowles approach.[37] This reorganisation signalled a stabilisation of econometrics teaching, 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 in comparison. Houthakker, who received the John Bates Clark Medal in 1963 and was president of the Econometric Society in 1967, became the main driver of econometrics at Harvard, although teaching of the econometrics courses was left to more junior faculty who did not make much of a mark. The slowdown in econometric activity would have been noticed by Zvi Griliches, who arrived at the end of the decade.[38]

group (see Valavanis 1959: vii). On the distinction between the "textbook" approach and other econometric methodologies, see Hoover (2006).

[36] Houthakker was also the editor of the *Review of Economics and Statistics* from 1972 to 1992.

[37] See for instance SCORLE (HUC 8522.2.1), Box 8, Folder "Economics, 1960–1961" and Box 7, Folder "Economics, 1957–1958".

[38] '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' (Griliches in Krueger and Taylor 2000: 179).