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The Microeconometrics of Household Behaviour: Building the Foundations, 1920-1960

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

 I can confirm that a version of Chapter IV is published in *History of Political Economy* (Cheng 2020). I can also confirm that this thesis was copy edited for conventions of language, spelling and grammar by Scribbr B.V.

I declare that my thesis consists of **62,297** words excluding bibliography.

Abstract

This thesis explores the early history of microeconometrics of household behaviour from the interwar period to the 1960s. The analytical framework relies on a model of empirical knowledge production that captures the scientific progress in terms of its materialistic supplies and intellectual demands. Under this framework, the thesis traces how microdata at the household level first appeared in the econometrician's research agenda and how the discipline was consolidated by communities of econometricians.

This study contains four substantive chapters. The first three chapters are selective case-studies charting three important approaches in the development of microeconometric practices. The first chapter reviews the interwar literature. Among those decentralised practices, Arthur Bowley's analysis on family expenditure stands out as one of the earliest exemplars. The second and third chapters explore the formation of two communities in the post-war period: Richard Stone's Department of Applied Economics (DAE) at Cambridge, and Guy Orcutt's Social Systems Research Institute (SSRI) at Wisconsin. With the benefit of the new microdata and the introduction of computer-based calculation, Stone and his crew created a cooperative group that produced the first series of microeconometric publications driven by intellectual problems and economic questions. By contrast, Orcutt came to the analysis of microdata driven by his dream of microsimulation, a bottom-up method of microeconometric modelling, more heavily dependent on computing power and designed for revising public policies. After frustration at the SSRI, he finally finished a household simulation model at the Urban Institute. Taking the DAE and SSRI as examples, the fourth chapter assesses both the internal academic relationships of these groups, and the consolidation of both literatures using bibliometric data and network analysis. The results demonstrate the ways in which the DAE was a more interconnected network than the SSRI. The citation analysis offers an alternative way in understanding the formation of econometric knowledge based on community relations rather than the supply of materials or intellectual demands.

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Chapter I. Introduction

In 1930, the Econometric Society was founded by a group of economic statisticians and mathematical economists for 'the advancement of economic theory in its relation to statistics and mathematics' (Roos, 1933). Followed by the first issue of *Econometrica* in 1933, econometrics as a new subdiscipline of economics began to spread in the profession. The new community took decades to grow. However, by 1969, when the first Nobel Prize in economics was awarded to Ragnar Frisch and Jan Tinbergen for their contributions to the analysis of economic dynamics, econometrician as a professional identity had been widely recognised and 'macroeconometrics' firmly established.

Analogous development can be found in microeconometrics, a term popularised in the 1960s.[1] When James Heckman and Daniel McFadden won the 2000 Nobel Prize for their analysis of selective samples and discrete choices, the prize release defined microeconometrics as 'a methodology for studying micro data, i.e., economic information about large groups of individuals, households, or firms' (Nobelprize.org, 2000).[2] Initially, the term was adopted in contrast to macroeconometrics, the econometric analysis of the whole economic system.[3] Microeconometrics deals with individual-level data and investigates the choice and behaviour of economic agents, such as consumers, producers, and households; macroeconometrics concerns market- or national-level data for an understanding of the economic phenomena at a higher level, such as inflation, business cycles, and economic growth.

The professionalisation of macroeconomics and macroeconometrics started in the mid-1930s. Modern macroeconomics grew out of the early business cycle economics and J. M. Keynes's (1936) *General Theory*, which theorised the aggregated economic mechanism with relatively few microfoundations. Afterwards, the term macroeconomics was adopted to accommodate the epistemic need of Keynesian economics. Similarly, Jan Tinbergen (1936)

[1] A quick search from Google Ngram indicates that the term microeconometrics was first used in 1954 and the frequency of its use increased in the 1960s. https://books.google.com/ngrams

[2] This coincides with the textbook definition of microeconometrics, as formulated, for example, by Cameron and Trivedi (2005, 3), who described it as 'the analysis of individual-level data on the economic behavior of individuals or firms'.

[3] Ngram shows that the term macroeconometrics first appeared in 1946.

produced the first econometric model of the Dutch economy to explore annual business cycles and fluctuations of aggregated economic variables. With the same technique, Tinbergen (1939) finished another study on the U.S. economy. Afterwards, the term macroeconometrics was gradually separated from econometrics to match grand economic systems of equations that applied time series to the simultaneous equation modelling framework and estimations of structural parameters.

The main reason behind these macro divergences was the failure of orthodox economic theories that had worried economists during the Great Depression. Unlike their *laissez-faire* predecessors, Keynes and Tinbergen both believed that their macroeconomic theory and macroeconometric modelling would play a role in redirecting economic policies.[4] Their approaches directed the first trend of post-war macroeconometrics in the 1940s. One of the earliest syntheses of Keynes's theory and Tinbergen's method was advanced by Lawrence Klein (1950) at the Cowles Commission and later through his seminal work co-authored with Arthur Goldberger (1955). Klein's model and its probabilistic foundations (Haavelmo, 1944) set up the Haavelmo-Cowles programme.[5] The programme treated the economy as a combination of stable and interdependent structures, and thus, aggregate-level stochastic observations and the technique of simultaneous equations modelling were applied to measure these structures. In the late 1950s, most econometricians would agree that the Haavelmo-Cowles programme was the most prominent research programme in macroeconometrics.

In contrast, microeconometric analysis explores the behaviours of individual units: people, households and firms. Given the fundamental difference between the macro and micro aspects of data, microeconometrics inevitably developed its unique set of research questions and methodologies. Such features suggest that microeconometrics entails a history and scientific tradition that are distinctive from its macro alternative.

Nevertheless, the current understanding of the development of microeconometrics shows a mysterious discontinuity. On the one hand, as recorded in George Stigler (1954), economists and statisticians had been using economic microdata as far back as Ernst Engel in the 1850s, and Stigler traced this historical trajectory until the 1920s. On the other hand, some commentators have suggested that microeconometrics began its professionalisation only in the 1970s. For instance, Martin Shubik (1970, 420) and Lawrence Klein (1974, 43) still regarded microeconometrics as a 'scarcely begun' and a 'growing' discipline. If Shubik's and Klein's

[4] However, Keynes and Tinbergen did not agree with each other. For the Keynes-Tinbergen debate, see Morgan (1990, 121–5).
[5] Section 5.3 provides a historical review for this statement.

perceptions are accurate, the historical development of microeconometrics between the 1920s and 1970s is still a blank page. Thus, the formation of microeconometrics from the usage of microdata to its methodological progress since the 1920s deserves further attention.

Whether the discontinuity really exists, however, is the real mystery for the history of econometrics literature has ignored the micro aspect for decades. In contrast, since the mid-1980s, many historians have written on the history of macroeconometrics. Important contributions include Epstein (1987) and Qin (1993) on the chronology of the Haavelmo-Cowles programme, Morgan (1990) and Hendry and Morgan (1995) on its thematic histories, and Gilbert (1988) on its development in Britain. Despite the eventual specialisation of these historical questions, as seen in the work of Boumans (2004; 2015) on measurement issues, Chao (2009) on the consumption function and Pinzón-Fuchs (2017) on macroeconometric modelling, a history of microeconometrics as a scientific enterprise is still lacking.

This intellectual gap in the history of econometrics leads to the general research question of this thesis: What are the historical foundations of microeconometrics? In answering this question, the thesis focuses on the development of household data and its econometric analyses between 1920 and 1960. This introductory chapter contextualises this study's focus in three ways. First, sections 1 and 2 elaborate the justifications behind the choice of this subject. Section 1 overviews how recent labour economists have provided insights into their particular histories of microeconometrics, and section 2 explains why focussing on household data helps to pin down a methodologically relevant and historically coherent chunk of microeconometrics. Afterwards, sections 3 and 4 develop the theoretical framework; the former clarifies the concepts of practice and exemplar, and the latter introduces the analytical framework – the model of empirical knowledge production. Finally, section 5 provides some common historical backgrounds, including the emergence of household data, the popularisation of general-purpose computers, and the rise of the Haavelmo-Cowles programme. After showing the theoretical framework and historical prerequisites, section 6 outlines the structure of this study.

1. The Large Gap in the History of Microeconometrics

In recent years, the history of microeconometrics has attracted the interest of some applied microeconomists. The most notable example is the literature on the 'credibility revolution' (Angrist & Pischke, 2010), which claims that empirical microeconomics experienced a methodological revolution in the 1980s that addressed early critiques of econometrics (Hendry,

1980; Sims, 1980; Leamer, 1983). This experimentalist turn featured two characteristics. The first is the inclination toward a 'clear-eyed focus on research design' (Angrist & Pischke, 2010, 6), following Ronald Fisher's experimental inference strategy in his approach to causality:

> Design-based studies are distinguished by their prima facie credibility and by the attention investigators devote to making both an institutional and a data-driven case for causality.

The Angrist-Pischke account attributes the rise of experimental design in microeconomics to the popularisation of empirical toolkits in replacing traditional structural regressions, such as the instrumental variable, difference-in-difference estimation, and regression discontinuity design (Panhans & Singleton, 2017). These methods are only reasonable when the second characteristic – the new experimental and natural-experimental microdata – became available to applied economists. Since such microdata is collected or observed under a randomisation rationale that matches the Fisherian criteria of the designed experiment, a reduced-form modelling strategy can thus guarantee the causal relationship. With improved data quality, there is no need to specify structural models designed for fitting observational data.

With the emergence of design-based microeconometrics in the 1980s, Angrist and Pischke (2009, 26) praised their advisor Orley Ashenfelter as a 'pioneering proponent' by bringing experimental and quasi-experimental design into social science. Other than that, they maintained a relatively open interpretation of the history:

> Accounting for the origins of the credibility revolution in empirical economics is like trying to chart the birth of rock and roll. Early influences are many, and every fan has a story. (Angrist & Pischke, 2010, 5)

Following their rock-and-roll analogy, charting the credibility revolution leads to a labour-economist version of the history of microeconometrics. This version is intertwined with the rise of the 'Chicago approach' and its empirical applications of microdata to labour issues. Biddle and Hamermesh (2017) argued that the Chicago approach is a microeconometric analysis in labour economics consolidated by a generation of Chicago-affiliated economists back in the mid-1950s.[6] The Chicago approach adopts the neoclassical inferential strategy to derive the reduced-form econometric model for microdata estimations. As discussed by James Heckman (2017, 1840), 'Chicago economics emphasized the value of economic models in interpreting and guiding collection of data and making forecasts and constructing policy counterfactuals'. The methodology has been widely practised since the 1960s, especially in the explanations of labour phenomena and evaluations of policy treatment effects. Some classic research questions include the following: How does unionism impact relative wages? Do the

[6] The leading figures were H. Gregg Lewis, Jacob Mincer, Gary Becker, George Stigler and James Heckman.

working hours of female employee increase with respect to wages? How do years of schooling affect real wages? What is the efficacy of a training programme and its effectiveness in another environment? Based on the scope of the questions, Chicago econometricians proceeded to collect relevant individual data. The adjusted data were fed into the reduced-form models identified from the Chicago price theory, and then the estimated parameters were applied to other contexts to make policy predictions.

Since Heckman and McFadden received the Noble Prize in 2000, the history of micro-econometrics in labour economics is better understood now. In his Nobel lecture, Heckman (2001, 674–5) offered historical hindsight that described the storyline of microeconometrics as divided into four themes: *the surge of new data since the post-war period, the 'empirical failure' of Cowles macroeconometrics, the empirical discovery of heterogeneity of economic agents*, and *the difficulty in obtaining structural parameters*. In Heckman's account, labour micro-econometrics began when new microdata were collected and when macroeconometrics was criticised as unreliable for policy evaluations. First, he observed that econometricians benefited from 'the flood of micro data that began to pour into economics in the mid 1950s' (Heckman, 2001, 677). The data flood was verified by Stafford's (1986) survey of labour economics articles from six major economic journals, which concluded that the usage of microdata significantly increased between 1965 and 1983.[7] The efficient estimation of the microdata was due to advanced computers, which improved the efficiency of the econometric computations; as Heckman stated, 'The advent of micro surveys coupled with the introduction of the computer … made it possible to produce hundreds, if not thousands, of regressions quickly' (Heckman, 2001, 677). This observation was supported by Brownstone (1983, 81), who argued that microeconometrics was 'facilitated by the increasing availability of micro data sources and the decreasing cost of computers'. Second, in the 1960s, the Cowles simultaneous equation modelling approach was gradually perceived as an empirical disappointment. Thus, it became necessary to solve its pitfalls with microeconometrics as an alternative research programme. Heckman elaborated this data-driven demand:

> Microeconometrics extended the Cowles theory by building richer economic models in which heterogeneity of agents plays a fundamental role and the equations being estimated are more closely linked to individual data and individual choice models. (Heckman, 2001, 676)
>
> A demand for low-dimensional economically interpretable models to summarize the growing mountains of micro data was created, and there was increasing recognition that

[7] Stafford attributed those new panel and cross-sectional datasets to the Current Population Survey (1940–, microdata first available in 1962), the National Longitudinal Survey (1966–), the Survey of Economic Opportunity (1966–9) and the Michigan Panel Study of Income Dynamics (1968–).

> standard regression methods did not capture all of the features of the data, nor did they provide a framework for interpreting the data within well-posed economic models. (677)

Therefore, microeconometrics was motivated by the distrust of the econometric relations inferred from solely macro-models with few aggregate-level data. The crisis of the Cowles approach can be further divided into two distinct issues. The first is the 'aggregation problem' (Theil, 1954), which questions the stability of empirical relations between individual decisions and aggregated outcomes. When such a problem occurs, there is no reliable way to translate the macro-level data into the dynamics of micro-entities and also account for economic agents' versatile characteristics. Second, the Cowles approach is criticised for its oversimplification of the economic system, which does not comprehensively unfold the causal economic structures.[8] Even under a fully identified structure, the aggregate-level data are too few to warrant its parameters statistically – the problem of underdetermination (Liu, 1960). This issue highlights the macroeconometrician's practical dilemma in regard to the unstable structure and lack of data that makes it impossible to obtain true structural parameters.

Another social force behind the Cowles approach's failure in Heckman's account was the epistemic demand for policy evaluations starting in the mid-1960s, as he elaborated, 'the ensuing demand for information about the characterization, causation, and solutions to social problems and the public demand for the objective evaluation of social programs directed toward specific groups' (Heckman, 2001, 676). Policy needs in the US were derived from the activist proposals of John F. Kennedy's New Frontier and Lyndon B. Johnson's Great Society, both of which contained a series of welfare programmes for revising social problems, such as inequality, poverty, education and medical care.

In sum, Heckman's explanations imply that the history of microeconometrics can be approached from the increasing supply of microdata and computing powers and the demand for micro-level estimates to test neoclassical microeconomic theories, contrast the Cowles approach, and inform social policies. Nevertheless, although Heckman has shown the potential of this pattern of knowledge production to explain the history of empirical labour economics, his emphasis on labour economics misses other significant developments in micro-econometrics between 1920–70. Recalling the rock-and-roll analogy, the history of micro-econometrics entails versatile versions of stories. Proceeding with this idea, apart from the one outlined by labour economists, a comprehensive examination of alternative histories of various elements in microeconometrics is needed.

[8] See Epstein (1987, Chapter 4).

2. The Historical Scope of this Thesis

The time frame of this thesis focuses on the period between the 1920s and the 1960s. The limited scope of the time frame does not imply the history before is unimportant, but econometrics then was an immature field. Before the 1920s, very few econometricians used the regression analysis, Keynesian macroeconomic theory and Tinbergen's macroeconometric system were not even in the air, and economists, in general, were not interested in offering policy advice through quantitative explorations. It was not until the Progressive Era and the Great Depression that applied economists started to perceive their worlds differently. Epistemic demands for the calculations of social reforms and policy interventions emerged during the interwar period to counteract the failure of *laissez-faire* economics. Against this background, a group of economists gradually shifted to applying statistical methods to explain economic phenomena, forming the first generation of econometricians. An apparent consequence of this intellectual movement was the establishment of the National Bureau of Economic Research in 1920, the Econometric Society in 1930, and the Cowles Commission in 1932. Since then, econometricians began to focus on methodological issues of microdata and its empirical application.

Three levels of microdata can be traced to the historical foundation of microeconometrics: individual, household, and firm. Instead of looking at all three, this thesis adopts household data as the unit of analysis and focuses on its practical use to understand household behaviours.

Three reasons justified that tracing household data helps to locate a large, new, and coherent chunk for the scope of this thesis. First, household data between 1920 and 1960 is one of the most commonly used microdata in econometric practices. As the primary source of household data, family expenditure surveys have a long tradition in the US and UK, stretching back to the late 19th century.[9] Since the early 20th century, these surveys have been used as the primary evidence for computing the cost-of-the-living index (Stapleford, 2009; Searle, 2015). At the same time, economists in the 19th century were interested in the empirical relationship between income and expenditure, dating back to the English poverty surveyors in the 18th century and Ernst Engel's (1857) budget study. The emergence of new expenditure surveys and its influence on the Engel tradition needs further exploration. Furthermore, household expenditures are informative data connected to economic theories, such as the concept of commodity demand, consumption, and saving. The early emergence of expenditure surveys

[9] The history of family expenditure surveys will be reviewed in Section 5.1.

was a timely supply for economists and econometricians who were curious about not only the Engel curve but other theoretical issues regarding household behaviours.

Second, some histories of individual-behavioural data in economics have already been addressed in the secondary literature. For instance, Heckman's account of empirical labour economics is about understanding the behaviours of the individual as workers. Under this framework, the individual-behavioural data, such as wages, earnings, working hours, and education level, are interpreted as variables in the labour market models under the neoclassical theory of labour supply and demand. Another example is the history of experimental economics, which has been well-documented (Moscati, 2007b; Svorenčík, 2015). The purpose of these laboratory experiments is to test an individual's behavioural choice assumptions of economic theories, such as rationality and preference. Nevertheless, the practical concerns of economic experiments are not about statistical issues but the process of acquiring data.

Last, the development of microeconometrics of households forms a more coherent chunk than individual-firm data in capturing the development of microeconometrics. One of the earliest uses of individual-firm information was in the empirical examinations of Gibrat's law, which argued that the distribution of firm size is lognormal.[10] This stream of empirical literature was developed in the 1950s by Peter Hart and Sigbert Prais in the UK and Herbert Simon and co-authors in the US (Sutton, 1997, 43). Nonetheless, this literature mainly applies firm-size information to explore the statistical properties of firm-size growth and does not explicitly relate the empirical evidence to economic theories. Such appeal to factual presentation deprioritises individual-firm data as the unit of analysis.

Furthermore, from the data supply perspective, separating from the population census, the census of manufactures in the US was first conducted in 1905 (United States Census Bureau, 2021). However, census microdata from industries was only made available around the late 1970s. As observed by Kallek (1975, 257), 'even today [1975] most economic research related to the enterprise or firm is limited to the utilization of aggregated data'. Such observation suggests the potentially missing history of firm microeconometrics since the 1970s, which is, however, outside the thesis's time frame.

[10] See Sutton (1997) for a review of Gibrat's law.

3. The Theoretical Concepts of this Thesis

This thesis follows two theoretical lines in its historical examinations of household microeconometrics. The first is the concept of the *scientific exemplar*, introduced by Thomas Kuhn (1970) in his postscript to the second edition of *The Structure of Scientific Revolutions*. He (1970, 175) redefined his famous scientific paradigm in two complementary ways:

> [The first] … stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community … [the second] … denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.

Kuhn emphasised the social aspect of the paradigm as the constellation of group commitments and the practical aspect as the exemplar. Both elements are collected in the 'disciplinary matrix' (Kuhn, 1970, 181–7). On the one hand, the group commitments cover the cognitive aspect of scientific theories, including theoretical assumptions, values and symbolic generalisations that serve as standard models and formulas with which scientists communicate. On the other hand, the exemplar entails shared examples that guide scientists in solving practical problems. These examples are learned during scientific education, such as a textbook exercise, a laboratory experiment or a numerical analysis of the blackboard:

> … these shared examples should, however, be added at least some of the technical problem-solutions found in the periodical literature that scientists encounter during their post-educational research careers and that also show them by example how their job is to be done. (Kuhn, 1970, 187)

From Kuhn's point of view, while group commitments tell scientists where and what to see about scientific puzzles, only exemplars offer recipes for how to deal with them. The exemplar demonstrates the practical knowledge that scientists have learned from engaging in scientific practice and assumes that a typical scientific method is superior to another. In this regard, the Kuhnian exemplar can be interpreted as a *methodological paradigm*, one that is embodied from the method's practical aspect and normative commitments. The methodological paradigm can further influence scientist's behaviour and reshape the consensus of communities. Thus, to trace a methodological paradigm is to identify how a particular method transitions from a set of warranted beliefs and bundles of techniques to its actual application in the scientific community.

The main advantage of the exemplar as a methodological paradigm is that it separates the scientific method from theoretical entities and historicises the method itself. This notion is applicable to this thesis since econometric research is, by definition, related to methods. For

instance, it is evident to every learned economist that a demand theorist is not equivalent to a demand analysist. The former is mainly concerned with the theorisation and mathematisation of the price-quantity market system, whereas the latter is concerned with empirical estimation and issues of measurement. The introduction of the Kuhnian exemplar and methodological paradigm offers insight into what econometricians are doing instead of what economists are thinking. In this vein, an exemplar in econometrics should demonstrate how a specific style of econometric analysis is performed.

The Kuhnian exemplar is a convenient term that distinguishes the concept of doing (and believing in) econometrics from the rest of economic research. This demarcation leads to the second theoretical line of this thesis: the French school of historical epistemology, an approach to the history and philosophy of science rooted in the contributions of French scholars.[11] The approach was recently crystallised by Thomas Stapleford (2017), who applied it in the history of economics. The main task of historical epistemology is to systematically examine *practices*, defined by Stapleford (2017, 7) as the 'collections of behavior that are teleological, subject to normative evaluation by broad groups, and exhibit regularities across people in a constrained portion of time and space'. Thus, historical epistemologists study the historical process of these practices that 'contribute to generating or sustaining formal knowledge that makes truth claims'. Therefore, the formation of knowledge through historical epistemology is a holistic one, as it does not simplify any possible elements susceptible to that knowledge. The fundamental elements of this framework are human action and its outcomes. Part of the outcomes will become forms of practices and weave a hierarchical system. The task of historical epistemologists is to identify these practices and demonstrate the extent to which they are individually linked with each other. These links will ultimately map a complex network of knowledge formation built on every practical microrelation in the context.

Drawing upon these two theoretical lines, this thesis adopts the terms 'exemplar' in the Kuhnian sense and 'practice' used in historical epistemology to analyse the historical foundations of microeconometrics. To this end, adopting the term practice assumes that microeconometric knowledge can be deducted from various human actions and are intertwined with multiple internal and external factors. Furthermore, while both terms may accurately capture scientist's goals and communal rules, exemplar places more emphasis on the paradigmatic aspect than practice. Hence, the term 'practice' is used to refer to any result of

[11] Prominent figures of this tradition are linked to Jean Cavaillès, Gaston Bachelard, Georges Canguilhem, and Michel Foucault. For a recent overview, see Peña-Guzmán (2020).

individual or collective actions made by microeconometricians, whereas ‘exemplar’ describes any practice recognised by the community as a methodological paradigm. These two concepts will reappear in the analytical framework in the next section and in the following chapters.

4. The Model of Empirical Knowledge Production

This thesis developed a historical framework called the *model of empirical knowledge production* to address the formation process of microeconometric practices and exemplars. Empirical knowledge is defined as any practice that (1) includes quantitative evidence, (2) answers a theoretical or empirical question, and (3) is approved by the scientific community. This model sketches how empirical knowledge is consolidated through the actions of scientists. The model’s structure is illustrated as a flowchart in Figure 1. In general, the elements of empirical knowledge are its *materialistic supplies* and *intellectual demands* in the community. The former is any of the *physical input* that makes producing quantitative evidence practically possible; the latter is any of the *ideal forces* that raise questions answered by the quantitative evidence. Scientists’ actions drive the physical input and ideal force, which include any form of research activities triggered by personal motivations. The product of empirical knowledge become an exemplar for the community that triggers other scientist’s actions.

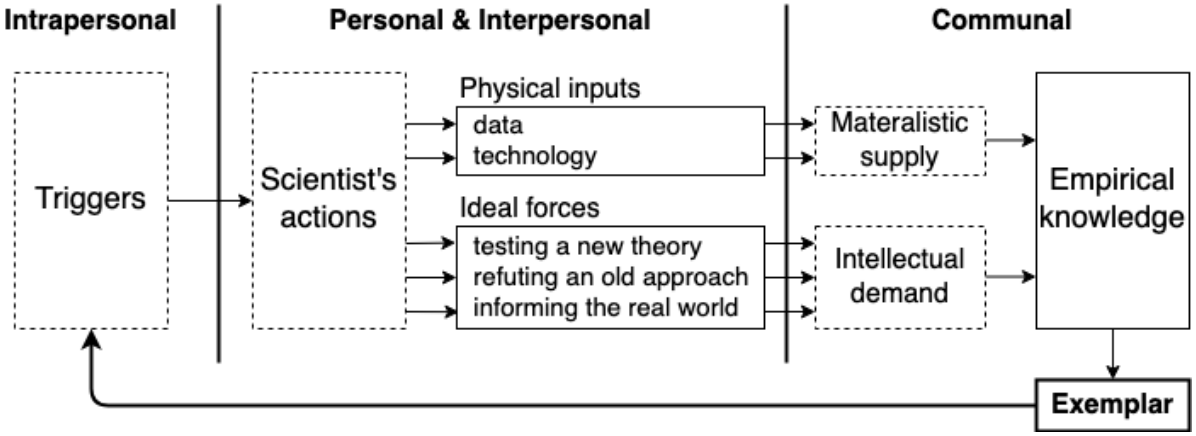


Figure 1 Structure of the model of empirical knowledge production

A fictional story illustrates the structure of this model. Suppose there are five characters in the specific context of empirical knowledge production: a statistician designs a household survey inspired by Marx’s *Das Kapital*; a computer scientist develops a regression programme after deciding to co-author with their economist colleague, who wants to test a new theory told

by their supervisor; an econometrician finds an old approach incredible after figuring out an alternative method of estimation; and a bureaucrat becomes curious about the state of poverty after Congress allocates a significant amount of its budget to fight poverty. The actions of these characters are teleological and subjected to intrapersonal triggers, such as 'reading about Marx', 'told by their supervisor', and 'a significant amount of budget is passed'. No matter what their triggers are, the actions will yield five consequences, respectively: the collection of new data, the improvement of technology, the intention to test a new theory, the distrust on an old approach, and the interest to information of the real world.

Personal and interpersonal actions have consequences at the communal level through two channels: the materialistic supply and the intellectual demand. The former involves changing external factors, and the latter involves the internal evolution of research questions. On the one hand, the materialistic supply is captured by the physical input of data and technologies that facilitate productivity. Changes in either one of the two affect the supply side. For example, to continue with the previous story, the supervisor-inspired economist cannot test their theory without a dataset from the Marxian statistician. Nevertheless, the economist can revise new estimates using an existing dataset with an advanced regression programme developed by the computer scientist. Thus, whenever data is collected or a programme is designed, the supply side emerges.

On the other hand, the intellectual demand is characterised by the ideal forces among scientific communities. Such forces can be divided into three categories: testing a new theory, refuting an old approach, and informing the real world. Similar to the supply side, the formation of intellectual demand only needs one of them. In this sense, the demand side increases when the economist decides to empirically test a theory, when the econometrician finds the approach unconvincing, or when the bureaucrat writes a paper urging research into poverty line.

The final product of empirical knowledge is distinguished from either a change in the supply or the demand side. However, while academic knowledge requires the community's approval, satisfying one of the two sides does not always lead to empirical knowledge. For instance, the econometrician may not publish their evidence after refereed by one of their intellectual opponents, even if they have proposed an estimation that is driven by the demand or supply. The evidence becomes an accessible example of the community only if it is approved by academic rules. Only then can empirical knowledge be consolidated.

After consolidation, new empirical knowledge starts its second life cycle: it may be buried in the past literature or be recalled as an exemplar by other practices. In the latter case, the exemplar acts as a trigger that motivates and reshapes other scientist's actions. For instance,

although the econometrician's work is initially a result of their intention to refute an older approach, their publication may signal to other practitioners that the community requires such typical econometric knowledge. The responses to this signal result in very different actions. The statistician may cite the exemplar in a paper; the computer scientist may not care until the economist co-author finds it interesting; the economist may use it to teach their student; someone may decide to do a PhD under the econometrician; or the bureaucrat may use the exemplar as evidence in congressional policy hearings. If the econometrician is lucky, the exemplar will be recognised as part of their strong academic record and included in the contemporary textbooks. If not, the exemplar may stay in the library periodicals for 50 years until a historian of economics rediscovers it.

In sum, the growth of empirical knowledge in this model is an infinite process of iteration. Once the knowledge is recognised as an exemplar, it triggers scientists' actions. The actions will lead to new physical inputs or ideal forces that change the materialistic supply and intellectual demand. The new demand or supply consolidates new empirical knowledge, and the knowledge may be a new exemplar that triggers scientists in the next period. Thus, the task of historians of science is to reconstruct this historical process through the identifications of (1) how scientist's actions are motivated by the triggers, (2) how practices are consolidated by scientists' actions, (3) how practices are recognised as exemplars, and (4) how exemplars function as new triggers.

5. The Historical Background of Household Microeconometrics

Some historical background will be helpful to illustrate the history of household microeconometrics from scratch. These backgrounds are presented before the analyses of this thesis since they are crucial prerequisites in understanding the substantial chapters of this thesis. These prerequisites can be categorised into three historical themes: (1) the emergence of household microdata, (2) the popularisation of digital computers, and (3) the rise of the Haavelmo-Cowles programme in macroeconometrics. The first two themes relate to the materialistic supply of data and technology, and the last is a typical example of the intellectual demand of refuting an old approach. While the secondary literature well documents some of the histories, this section aims to attract some developments between 1920 and 1960 relevant to this thesis.

5.1 The Emergence of Household Microdata

Econometricians mainly use two types of household microdata in their analysis: household budget surveys and consumer finance surveys. These histories of microdata from the producer's side are complicated. For instance, Thomas Stapleford (2009) has shown that the creation of family expenditure surveys in the US cannot be separated from its political context. In this vein, Rebecca Searle (2015) demonstrated how the government-run budget surveys in the UK inherited the traditions of the British working-class study. However, these stories were less relevant to the scope of this thesis once the microdata was produced, made public, and transferred to econometricians. While these practitioners are primarily concerned with their empirical puzzles, the emergence of microdata provides new inputs to suit their demands for new estimations. In this sense, as will be shown in Chapters II and III, the household budget surveys were widely used by econometricians in various studies, from the real-world situation of poverty to the analysis of household behaviour. In Chapter IV, the survey of consumer finances was a crucial source for microsimulation and for studying the demand for durable goods.

The first type of household microdata was family budget surveys that mainly collected total expenditure, consumption of daily necessities, wage, and income. These surveys were usually conducted through questionnaires or personal interviews and by individuals or government agencies. Early budget surveys were run mainly by individuals (Stigler, 1954; Stone, 1997; Deeming, 2010). Individual-run budget surveys appeared in late 18th-century England. Two notable pioneering studies were by David Davies in 1795 and Fredrick Eden in 1797, which collected 127 and 86 budgets, respectively.[12] In continental Europe, Édouard Ducpétiaux conducted three surveys in Saxony and Prussia in 1848 and Belgium in 1855, which all together included around 200 budgets. Ducpétiaux's work on 153 Belgian families was studied by Ernst Engel in 1857, who developed the well-known 'law of consumption', which describes the negative relation between family income and its proportion of food expenditure (cf. Stigler, 1954, 95–8). Since then, Engel's law of consumption has opened a new research topic in consumer behaviours for statisticians and economists. The development of this theoretical line will be studied in Chapters II and III.

Later, the scale of individual-run budget surveys skyrocketed in late 19th-century England. Two important budget surveys were conducted by Charles J. Booth between 1886 and 1903

[12] For an econometric reanalysis of Davies's and Eden's data, see Gazeley and Verdon (2014).

and by B. Seebohm Rowntree in 1899. The former covered around 180,000 households in London, and the latter studied 11,560 working-class families in York. Their statistical analyses were known for their approximations of the 'poverty line' in estimating the proportion of poverty.[13] Such methodology profoundly influenced the interwar empirical studies of poverty.[14] As summarised in Deeming (2010, 776–7), from 1900 to 1950, 16 individual-run budget surveys were conducted to measure poverty. The largest of these surveys was one of London in 1929–30, which covered around 30,000 families by the London School of Economics under the direction of H. Llewellyn-Smith. Chapter II documents the methodological progress and applications of these poverty surveys during the interwar period.

Government-run budget surveys emerged in late 19th-century America. As discussed in Stapleford (2012, 162), between 1890 and 1950, the U.S. Bureau of Labor Statistics (BLS) conducted six major expenditure surveys that spanned between 8,000 and 60,000 families. The earliest government-run budget survey was performed in 1888–90 by Carrol Wright (Gazeley, Holmes, & Newell, 2018, 19). This cross-country survey included 8,544 budgets from the US and other European countries. Afterwards, the BLS surveys shifted their focus to the domestic sphere, and the scales exploded. The 1901–2 BLS survey included 25,440 U.S. families, and all other surveys since the 1910s have included at least 12,000 (Stapleford, 2012, 162).

Similar to the US, European statistics officials started to collect budget data in the late 19th century. The early attempts in western Europe before the 1930s have been summarised by Staehle (1935) and Gazeley, Holmes, and Newell (2018, 21–3). Among these European countries, the UK conducted the most budget surveys between 1900 and 1950. The first government-run survey in the UK was a survey of 1,944 samples by the Board of Trade in 1904 (BPP, Cd 2337).[15] Soon after, another series of budget enquiries in the UK, Germany, France, and Belgium were initiated by the Board of Trade in 1906. The statistical results were published between 1908 and 1910 (BPP, Cd 3864; Cd 4032; Cd 4512; Cd 5065). The same enquiry was repeated in the UK in 1912 (BPP, Cd 6955). Before the end of World War I, the 1918 Working Class Cost-of-Living Committee repeated the 1904 Board of Trade survey and collected 1,306 family budgets (BPP, Cd 8980; cf. Gazeley, Holmes, & Newell, 2018, 25–6). During the interwar period, the Ministry of Labour organised a survey of 10,762 working class families in 1937–8 (Nicholson, 1949), and the Civil Service Statistical and Research Bureau organised a

[13] For a review on the concept of poverty line, see Hennock (1991).
[14] See Bales (1999) for the public reactions to Booth's work.
[15] The 1904 survey was rediscovered by Gazeley and Newell (2011), who also provided the econometric estimates of that survey.

survey of 1,360 middle classes in 1938–9 (Massey, 1942). As will be shown in Chapter III, the econometric analysis of these two surveys culminated at the University of Cambridge under the guidance of Richard Stone.

These government-run surveys in the US and UK were primarily designed, as Stapleford (2009) and Searle (2015) have shown, to calculate the cost-of-living index. However, while these surveys were constructed for policy purposes, they would inevitably be considered for different uses with shifts in political concerns. For example, the U.K. 1918 working-class survey aimed to investigate the effect of the national rationing scheme (Gazeley & Newell, 2013). The U.S. 1935–6 Survey of Consumer Purchases, which covered almost 60,000 families, was carried when the federal government under the New Deal shifted its concern to consumer demand (Stapleford, 2007). For the latter case, interwar econometricians did match the government's hope and used the data to study the Engel curve and the Keynesian consumption function. These studies will be reviewed in further detail in Chapter II.

The second type of household microdata used was consumer finance surveys, which mainly referred to the Survey of Consumer Finances (SCF) implemented by the Survey Research Center at the University of Michigan. Using the interviewing survey and random sampling techniques, the SCF collected information on household finances, including savings, income, asset holdings,[16] and expectations for the future.[17] The survey was initially proposed when the federal government became worried about the possible inflation after World War II (Hosseini, 2003, 399). The Federal Board ultimately appointed George Katona as director of the SCF because of his past works on price control and inflation (Katona, 1942; 1945).[18] With the federal government's financial support, the Survey Research Center launched the SCF in 1946. The first SCF interviewed 3,058 spending units sampled nationwide from urban and non-urban areas (*Federal Reserve Bulletin*, 1947, 658).[19] Until 1960, the sampling scale remained around 3,000 units.

[16] Asset holdings included liquid assets and durable-goods purchases, such as houses and automobiles.
[17] In the SCF, participants' attitudes were clarified into 'Good, or very good', 'Good in some way; it depends', 'Bad, or very bad', 'Don't know', and 'Not ascertained' (Katona & Mueller, 1953, 16).
[18] George Katona (1901–1981) was born in Budapest, Hungary and received his PhD in experimental psychology from the University of Göttingen in 1921. Until 1933, he worked in Germany as a journalist and a psychologist writing on hyperinflation. Katona moved to the States in 1933. From 1942 to 1944, he was research associate at the Cowles Commission working on a project with Theodore Yntema on price controls (Katona, 1945). In 1944, Katona joined the Division of Program Surveys of the U.S. Department of Agriculture and was hired as co-director of the pilot study of Survey of Liquid Assets Holdings, Spending, and Saving, the prototype survey of the SRC. He was put in charge of the SRC from 1947 until the survey discontinued in 1971. Katona is also credited as the 'founding father of old behavioral economics' (Hosseini, 2011). See Wärneryd (1982) for Katona's biography.
[19] The spending unit was defined as 'all persons living in the same dwelling related by blood, marriage, or adoption, who pool their incomes for their major items of expenses' (*Federal Reserve Bulletin*, 1947, 662).

Since its initiation, the SCF has been applied by Katona to build his behavioural economics programme (Hosseini, 2011; Edwards, 2012). Meanwhile, econometricians have also discovered its value in studying consumer behaviours. Since the 1950s, some econometricians, including Lawrence Klein and James Tobin, have used the SCF as a primary source to better understand consumption and saving behaviours and demands for durable goods. The SCF also inspired Guy Orcutt, who later brought the survey into his microsimulation programme. Chapter IV explores the history of Orcutt's work and its relation to the SCF.

5.2 The Popularisation of General-Purpose Computers

In the 1920s, most econometricians solved their computation problems by hand and mechanical calculators. As shown in Morgan (1990, 139n), back then one of the practical issues of computation was the simplification of calculations to reduce the computation burden. Although Wassily Leontief had used electro-mechanical computers in the 1930s, it was not until the 1950s that econometricians were able to use digital computers to perform calculations (Backhouse & Cherrier, 2017, 106–7).[20] The main driving force behind these interests was the popularisation of general-purpose computers. From 1955 to 1965, the stock value of general-purpose digital computers in the US witnessed a 78% annual growth rate from US $370,000 per month to $194 million (Chow, 1967). Such development in the 1950s could be further categorised into two trends. First, some research universities in the UK and US started constructing their computer system, and second, decreasing manufacturing costs led to the commercialisation of digital computers. As a result, econometricians began to engage with computer-based calculations once these computers were made available. Until the 1960s, the computer programming technique was one of the econometrician's must-learn skills, and some econometricians were already familiar with writing punch-card programmes for performing statistical analyses (Backhouse & Cherrier, 2017, 108).

Since the first general-purpose computer, the Electronic Numerical Integrator and Computer (ENIAC), put in operation in 1946, many groups have initiated new projects on large-scale digital computers. As reported in a survey prepared by the Office of Naval Research (1950), until the late 1940s, apart from the ENIAC, there were other 22 places in the US and UK developing their digital computers, seven of which were research universities: Princeton, MIT, Harvard, and Berkeley in the US and Cambridge, Manchester, and Birkbeck College in the UK.

[20] See also Backhouse and Cherrier (2017) for a general history of computers in economics.

The numbers of digital computers grew steadily. Weik's (1955, 204–7) first survey of the U.S. electronic digital computing systems summarised that by the mid-1950s, around 30 manufacturers and at least ten American universities had built self-made electronic computers. Until Weik (1961, 1038–42) conducted the third survey, the total number of computer manufacturers almost doubled. Based on the boom of new computing systems, he estimated that the annual investment on these computers rose from 10 million dollars in 1953 to 100 million in 1956 and 1 billion in 1960 (Weik, 1960, 1027).

Manchester and Cambridge were the first two research universities in the UK to install their computing system in the late 1940s. In 1948, under the direction of F. C. Williams and Tom Kilburn, Manchester's Small-Scale Experimental Machine ran its first programme (Lavington, 1998). One year after, the Electronic Delay Storage Automatic Calculator (EDSAC) led by Maurice Wilkes was made operational at the Cambridge Mathematical Laboratory (Ahmed, 2013, Chapter 4). The EDSAC was then applied by Cambridge scientists in dealing with their respective computations. As will be demonstrated in Chapter III, Cambridge econometricians were early notable users of the EDSAC. Later, the EDSAC became the prototype of the Lyons electric office I (LEO I), one of the first computers for commercial business use developed by J. Lyons and Co (Caminer, 1997).

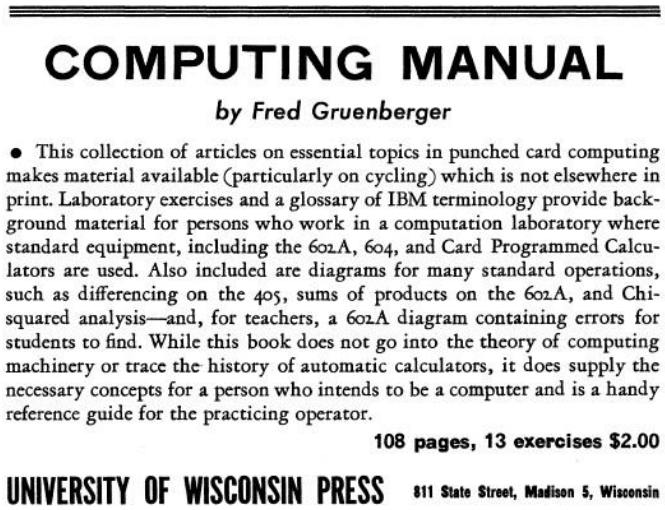
The emergence of LEO I in 1951 suggested that around 1950, for private manufacturers, the commercialisation of general-purpose computers was the way forward. This trend was also seen in the US back in 1949, when the International Business Machine (IBM) announced its Card Program Calculator (CPC), which combined the tabulating and calculating functions of a digital computation system.[21] The operations of the CPC included addition, subtraction, multiplication, division, and square root, and it also provided routed interconnections between storage, card punch, and printer (Pugh 1995, 155). The commercialisation of the CPC signalled IBM's attempt to enter the technical computing market (Akera, 2002, 774). As estimated by IBM, before 1955, around 700 CPCs were installed in governmental agencies and other research institutions.

Thanks to IBM's Applied Science Department, headed by Cuthbert Hurd,[22] the CPC began to be advertised in many research institutions starting in the late 1940s. As argued by Akera

[21] The first model of CPC contained electronic calculating punch IBM 604 and accounting machine IBM 402.
[22] Cuthbert Hurd (1911–1996) started his academic career as an applied mathematician. In 1948, he taught statistics at Carbide and Carbon Chemicals Corporation and realised the potential application of IBM computers for applied scientific research. After joined the IBM in 1949, he was in charge of inquiries about the CPC and organised conferences on the Monte Carlo technique. He was later employed as the head of the Applied Science Department with the responsibility of promoting IBM equipment among scientific customers until 1955 (Akera 2002, 776; Pugh 1995, 158).

(2002), along with his friend and consultant John von Neumann, Hurd established the brand of CPC through the supervision of its uses in various industries, such as aviation industries, MIT's Radiation Laboratory, and military services during the Korean War.

Hurd was also instrumental in promoting the CPC to the economics community when econometricians were becoming interested in time-saving technologies for their tedious calculations.[23] In 1950, Hurd and his IBM colleagues, Walter McNamara and George Ridgeway, joined the Econometric Society (*Econometrica*, 1950). No direct record showed that they were econometricians in any sense; instead, Hurd and his colleagues were more like technical experts of IBM's new merchandise. Hurd attended three of the society's annual meetings in September 1952, December 1953, and August 1955 and chaired sessions on applying digital computers in econometrics (*Econometrica*, 1953; 1954; 1956). During the first half of the 1950s, econometricians' demand for the CPC emerged. In 1952, the computing manual of the CPC was first advertised in the back matter of *Econometrica* (Figure 2) for 'a person who intends to be a computer' of the IBM machine,[24] which signalled that the econometric practices at the time had turned into the age of commercialised computers.



COMPUTING MANUAL

by Fred Gruenberger

● This collection of articles on essential topics in punched card computing makes material available (particularly on cycling) which is not elsewhere in print. Laboratory exercises and a glossary of IBM terminology provide background material for persons who work in a computation laboratory where standard equipment, including the 602A, 604, and Card Programmed Calculators are used. Also included are diagrams for many standard operations, such as differencing on the 405, sums of products on the 602A, and Chi-squared analysis—and, for teachers, a 602A diagram containing errors for students to find. While this book does not go into the theory of computing machinery or trace the history of automatic calculators, it does supply the necessary concepts for a person who intends to be a computer and is a handy reference guide for the practicing operator.

108 pages, 13 exercises $2.00

UNIVERSITY OF WISCONSIN PRESS 811 State Street, Madison 5, Wisconsin

Figure 2 Advertisement for CPC's computing manual (Econometrica, 1952, Back Matter)

In the late 1940s, Ragnar Frisch (1948, 372) predicted that the development of computational techniques would 'come to revolutionize the whole field of econometrics'. The introduction of the IBM machines to econometrics standardised the computational procedure

[23] For example, when Alfred Cowles (1938, vii) calculated the common-stock index in the United States, it was recorded over 1,500,000 work sheets were made and spent 25,000 human computer hours in total.
[24] Different to today's concept, it was the period when 'computer' (or 'computor') was the technician who operated the computing machine.

and improved its efficiency. Oskar Morgenstern (1963, 91–2) noted that while Ragner Frisch in 1934 programmed 100 multiplications of six decimal digits in an hour, IBM 7090 in 1960 could perform 10,000 multiplications per second. These IBM computers were sold to research universities and applied by econometricians to make tabulations, run regressions, and perform statistical tests. Subsequently, the IBM machines were widely adopted in economic theoretical and empirical investigations with immense improvements in computing speed.[25] For instance, the CPC arrived at the University of Michigan in 1952 (Scott, 2008) and was soon used by Lawrence Klein and Arthur Goldberger (1955). The Klein-Goldberger model was the first macroeconometric model aided by digital computers, with calculations of 15*15 matrix maximum likelihood estimations and moments of variables (Klein & Goldberger, 1955, 71; Renfro, 2004). Another notable example was Guy Orcutt's Monte Carlo simulation with the IBM 704, as discussed in Chapter IV.

5.3 The Rise of the Haavelmo-Cowles Programme in Macroeconometrics

Under Jacob Marschak's directorship, the simultaneous-equation econometric methodology was elaborated in the early-1940s at the Cowles Commission. The development was linked to two determinants: Trygve Haavelmo's (1943; 1944) probability approach and his simultaneous equation modelling framework (Christ, 1952; 1994; Hildreth, 1986; Epstein, 1987, Chapter 2; Morgan, 1990, Chapter 8; Qin, 1993, Chapters 1 and 2). From the early-1950s onwards, the Haavelmo-Cowles programme was the dominating exemplar among macroeconometric practices (Qin, 2015). However, while the time-series data were flawed, and the simultaneous model was unrealistic, some contemporary practitioners found the Haavelmo-Cowles framework not credible. As will be discussed in Chapters III and IV, these two critiques of the framework triggered the practical turns toward microdata in the post-war period.

In 1932, Alfred Cowles founded the Cowles Commission for Research in Economics in Colorado Springs. Closely affiliated with the Econometric Society, this non-profit corporation aimed 'to advance the scientific study and development … of economic theory in its relation to mathematics and statistics' (Christ, 1952, 11). At the time, the commission's research did not serve a systematic aim. The first two monographs published were collections of the previous works of Charles Roos (1934; 1937), the commission's first research director. The third monograph (Cowles, 1938) was the first actual product of commission's research project,

[25] For a review of those applications, see Backhouse and Cherrier (2017).

which constructed comprehensive time-series price indexes for the predictions of stock market behaviour. These early works of Colorado-Cowles were irrelevant to what is understood as the Haavelmo-Cowles programme today.

After Roo's departure in 1937, the commission struggled to recruit a new research director. As recorded by Christ (1952, 18), during the Cowles summer conference in 1937, none of the three potential candidates – Ragnar Frisch, Jacob Marschak of the Oxford Institute of Statistics, and Theodore Yntema of the University of Chicago – were willing to take the position. Because Colorado was not geographically appealing for the economic and statistics research, the commission decided to search for another location. In 1938, following Henry Schultz's sudden death, the economics department at the University of Chicago lost its representative of econometrics. It thus opened the possibility for the commission when the department needed someone fill in the vacancy (Christ, 1952, 20). In September 1939, the Cowles Commission moved to Chicago, and Yntema became the new director.

During Yntema's directorship between 1939–42, many staff members of the commission were allocated elsewhere to help with the war effort. Even Yntema was on leave in 1940 at the Defense Commission and on part-time leave in 1942 at the War Shipping Administration. Three monographs were published – one was on the history of silver money (Leavens, 1939), and the other two were on the statistical analysis of economic time series without addressing any issues of structural modelling (Tintner, 1940; Davis, 1941). Although moving towards econometrics, the monographs during these years still came from previous works carried from the members at the Colorado-Cowles. In 1942, Yntema resigned from the Cowles Commission and became research director of the Committee on Economic Development.

Yntema's successor was Marschak, who had spent four years at the New School for Social Research after leaving Oxford in 1939. Marschak arrived in Chicago at the beginning of 1943. Under Marschak's directorship until 1948, another three monographs were published before the war ended: two on general equilibrium theory by Mosak (1944) and Lange (1944) and one by Katona (1945), a field study on price control and rationing. However, these monographs were interested in topics different to econometrics. After all the authors left Chicago around the end of 1945, the commission did not publish any more monographs until 1950.

From 1943 onwards, Marschak formed a research group on econometrics at the commission. Prominent figures of the group included Tjalling Koopmans, who later directed the commission between 1948–54, Trygve Haavelmo, Meyer Girschick, Herman Rubin, and Lawrence Klein. Over the next few years, the group initiated multiple research projects in econometrics. Among these attempts, Haavelmo formulated one of the most prominent traditions in

macroeconometrics, which would eventually be recognised as the Haavelmo-Cowles programme.

The Haavelmo-Cowles programme was built on one idea and two theoretical foundations: the concept of social engineering, the probability approach (Haavelmo, 1944), and the simultaneous equation modelling framework (Haavelmo, 1943). First, as cited in Epstein (1987, 61–2), Marschak stated the aim of the commission in his report for the Rockefeller Foundation,

> The basic principles of the statistical analysis of *systems* of relationships (such as supply and demand equations) have been revised … The traditional method of least squares … must be replaced by certain other methods when the problems is one of "social engineering" (advice to firms, government agencies). (Marschak, cf. Epstein, 1987, 61)

Marschak advocated the idea that the economy was analogous to an engineering system in hoping that economists could be 'social engineers', referring back to his earlier quote on R. T. Bye (1940, 282): 'The ultimate justification of all science is the power it gives us to make things go the way we want them to' (cf. Marschak, 1941, 448). In this regard, the task of econometrics for Marschak was to systematically model the economy and then apply statistical tools for further manipulations. As discussed in Chapter IV, this engineering analogy was a crucial trigger for Guy Orcutt's simulation work.

Second, often attributed as the 'probabilistic revolution' in econometrics, Haavelmo's 1944 monograph was the milestone that introduced probability theory into the analysis of economic time series. As explored by Morgan (1990, 230–42), before the 1930s, econometricians were generally reluctant to apply the probability theory to time series while its data could not be observed independently. In contrast, Haavelmo (1944, iii) argued that a set of economic time series should have been treated as one observation of variables,

> … it is not necessary that the observations should be independent and that they should all follow the same one-dimensional probability law. It is sufficient to assume that the whole set of, say *n*, observations may be considered as one observation of *n* variables (or a "sample point") following an *n*-dimensional joint probability law, the "existence" of which may be purely hypothetical.

This assumption validated that a set of economic variables was one stochastic sample representing the aggregate outcome of various economic relations. In this sense, the statistical inference from annual economic time series fits into the theory of hypothesis testing, implying that the economic theories were probabilistic statements and testable statistical hypotheses once rigorously formulated.

Finally, once Haavelmo's probabilistic assumption was accepted, a systematic relationship of structural equations presenting economic theories could be statistically estimated. In the 1930s, this modelling style was practised by Frisch and Tinbergen and formalised by

Koopmans (Morgan, 1990, Chapters 3 and 4; Qin, 1993, 44–52). Under such a framework, one stochastic observation at a particular point needed to be simultaneously determined. Haavelmo (1943) formulated this point through simpler examples, showing that probabilistic assumptions and simultaneous equations were fundamental to estimating an economic system using time-series data.

Therefore, the rationales behind the Haavelmo-Cowles programme are threefold: (1) economic theories are causal descriptions of the economic world; (2) econometric models are formalised theories presenting as a system of structural relations; and (3) time-series data are stochastic observations that determine the system simultaneously. Accordingly, econometricians have three goals: to identify measurable and representative economic variables, estimate the parameters of these variables in testing theories, and apply the specified model in revising economic policies.

The programme's first series of research output was published in two monographs in 1950 with papers that appeared in *Econometrica* and other journals. The tenth Cowles monograph was a collection of the papers from the 1945 Cowles conference on simultaneous equation systems (Koopmans, 1950), and the 11th by Klein (1950) constructed a Haavelmo-Cowles macroeconometric model of the US using the time series between 1921–41. Subsequently, the Klein study became the benchmark for contemporary projects of macroeconometric modelling (Pinzón-Fuchs, 2019, 419).

From the 1950s onwards, the Haavelmo-Cowles programme started to appear as an exemplar in economic bachelor trainings. Qin (2015) interpreted the programme as the Kuhnian 'normal science' of the community between 1950 and 1970. Qin's survey of econometrics textbooks since the 1950s showed that ten of 12 textbooks spent at least 15% of their total pages introducing the simultaneous-equation modelling technique (279). For instance, Jack Johnston's *Econometric Methods* (1963), one of the most worldwide used introductory textbooks of econometrics, had 21% of its pages on the technique. Even until now, carefully chosen variables, systems of equations, and theory-based structural models are still crucial methodological commitments when teaching undergraduate econometrics.[26]

Despite its dominance, the Haavelmo-Cowles approach had its methodological pitfalls. One of the earliest critiques was made by its Chicago colleague, Milton Friedman, and his camp of economists, such as Rutledge Vining. Boumans (2016a) situated the battleground over the

[26] Some literature, such as Epstein (1987) and Keuzenkamp (2000), treats the Haavelmo-Cowles programme as 'textbook econometrics'.

differences between the Marshallian and Walrasian methodology and between the commission and the National Bureau of Economic Research (known as the 'Koopmans-Vining' controversy or the 'measurement without theory' debate). These struggles were based on the contrasting views about whether the economic inference was derived from a single equation or a set of equations and whether economic measurement was theory-driven or data-driven.

In addition to Friedman, some econometricians came to suspect the empirical validity of the Haavelmo-Cowles programme (Epstein, 1987, Chapter 5). The criticisms focused on the reality of Haavelmo's stochastic assumption of time series and simultaneous equations. On the one hand, econometricians at the University of Cambridge examined whether the aggregate time series were independent observations of the economy. In other words, if the series were serial-correlated, the estimated structural parameters were potentially biased. This autocorrelation problem of time series worried Richard Stone and Guy Orcutt, as addressed in Chapters III and IV. On the other hand, Herman Wold proposed that if the idea of a causal chain captured economic relations, the economic system would be recursively, not simultaneously, determined (Morgan, 1991). As discussed in Chapter IV, this concern would ultimately facilitate Orcutt's distrust of the Haavelmo-Cowles programme.

6. The Goals of this Thesis

The first goal of this thesis is to provide a historical account of household microeconometrics through a thematic reconstruction from an empirical knowledge production perspective. Based on the prerequisites above, this thesis documents how selected communities of econometricians used microdata to solve their puzzles and consolidate their research programmes. Three historical themes of microeconometrics using household-level data are presented in the following three chapters. Chapter II reviews the interwar literature and is divided into two parts on Arthur L. Bowley's seminal analysis of the Engel curve and on other empirical studies in the 1930s. Chapter III recounts the first series of contributions to household microeconometrics, led by the 'captain' Richard Stone and his 'Cambridge crew' of econometricians at the Department of Applied Economics. Chapter IV examines Guy Orcutt's intellectual journey of microanalytic simulation, a pioneering approach in microeconometric modelling that synthesises multiple household-level datasets using the Monte Carlo method.

Framed under the model of empirical knowledge production, the historical analyses of Chapters II–IV concentrate on the evolution of personal triggers to the consolidation of

microeconometric knowledge. The following important questions are addressed: What were the triggers of econometrician's actions? What were the materialistic supplies that allowed econometricians to use microdata? What were the intellectual demands that pushed econometricians toward the micro-level analysis of those materials? What were the central theoretical and empirical issues faced by econometricians? What were the crucial exemplars to solve these issues? How did these exemplars prompt other econometrician's works? A range historical sources, including published papers, interviews, personal archives, institutional records, and government documents, are used to answer these questions.

Drawing upon the new thematic histories of microeconometrics, the next goal of this thesis is to explore the possibility of assessing scientific communities and the contributions of their exemplars with bibliometric data. The bibliometric history of economics literature is still few and needs more practical applications. Chapter V provides a comparative study of the two communities of microeconometricians at Cambridge and Wisconsin discussed in Chapters III and IV. An empirical framework using bibliometric data and citation and network analysis methods is used to evaluate the interpersonal relationships and community activities involved in the formation of microeconometric knowledge within both institutional contexts. The evidence will help contextualise the qualitative findings of Chapters III and IV.

Chapter VI concludes the thesis. In summarising the findings from the substantial chapters, the chapter reassesses the model of empirical knowledge production. The advantages and limitations of this study are discussed to highlight some possible directions for future research.

Chapter II. Arthur Bowley and the Development of Interwar Microeconometrics

1. Introduction

This chapter reviews the interwar literature on household microeconometrics. The literature is fully covered by some secondary commentaries. For instance, Stigler (1954) has studied the empirical literature of consumer behaviour before the interwar period; Leser (1963) has sketched the development of the Engel curve; Mirowski and Hands (1998) have explored the role of budget constraints in interwar demand analysis; and Thomas (1989; 1992) and Chao (2019) have documented the early history of consumption function. This chapter focuses on the relationships between econometricians, theoretical entities, and empirical data to identify representative episodes in the development of microeconometrics. Therefore, instead of charting a chronology of economic publications using microdata, the materials reviewed will rely on the exemplars that bridge the entire historical development from the interwar to post-war period and facilitate discussion in the econometrician's community. Based on these criteria, early non-English exemplars were omitted since this chapter aims to construct the foundations of microeconometrics in the English-speaking world.

The development of interwar microeconometric practices presents a decentralised picture in terms of the data sources used. At the beginning of the 20th century, many countries initiated survey collections of household income and expenditure. In a contemporary summary, Staehle (1935) listed a considerable range of budget materials across 18 countries, most of which were conducted in the 1920s. The scale of budget surveys grew even larger afterwards. For instance, the U.S. Survey of Consumer Purchases in 1935–6 covered over 20,000 families, and the U.K. Working-Class Household Expenditure Survey in 1937–8 encompassed 10,000. Although limited to contemporary computing power, econometricians started utilising these materials to answer economic questions. As this chapter will show, the inevitable trade-off between data and computation that econometricians had to either curtail the data into a smaller scale or use the averaged number as point estimates in the regressions.

Table 1 Main sources reviewed and their empirical concerns

	Micro ('household')	Macro ('aggregate')
(a) Data type	Family budget surveys	Economic time series
(b) Price-quantity relation		
Single commodity	Pigou (1910)	Moore (1914)*
('demand curve')	Gilboy (1932)	Marschak (1943)
('price elasticity')	Marschak (1931)	
(c) Income-expenditure relation		
Single commodity	Engel (1857)*	Marschak (1943)
('Engel curve')	Frisch (1932)	
('income elasticity')	Gilboy (1932; 1938)	
	Allen and Bowley (1935)	
	Marschak (1931; 1943)	
Total consumption	Stone and Stone (1938)	Staehle (1937)
('consumption function')	Gilboy (1938)	Stone and Stone (1938)**
('marginal propensity to consume')	Mendershausen (1939; 1940)	

* Used as a benchmark
** Discussed in Chapter III

Table 1 summarises the main sources reviewed in this chapter, and the second column identifies microeconometric exemplars. The empirical concerns examined by the interwar exemplars varied in the research themes that were covered. These concerns are categorised into two scopes of inference and two kinds of empirical relations. On the one hand, the scope of inference can be micro- or macro-oriented. For the former, family budget surveys were used to infer the *household* behaviour of a particular group of families, while the latter applied economic time series to understand the *aggregate* outcome of the whole *nation* or *market*. In this sense, the macro label consists of two concepts: one from Keynesian macroeconomics that takes the national economy as its unit of analysis, and one from any econometric market-level analysis of a specific market.

On the other hand, these surveys and time series served two tasks: the *price-quantity* and *income-expenditure* relations. Based on the data availability, these tasks can be further divided into a single commodity and total consumption. Under this framework, Table 1 lists each reviewed material's scope of data and its empirical task. For instance, Moore (1914) used the price-quantity time series of agricultural goods to study aggregate (or market) demand on single commodities to understand the price-quantity relation; and Engel's (1857) investigation of the relationship between income and food consumption was a study of the household income-

expenditure relation to a commodity. Some empirical works covered multiple concerns, such as Gilboy (1932; 1938), Stone and Stone (1938), and Marschak (1931; 1943).

As shown in the first column of Table 1, these empirical concerns can be divided into three areas of research: demand theory, the Engel curve, and the Keynesian consumption function. The first two areas first interested economists and statisticians back in the late 19th century after the publications of Alfred Marshall's *Principles of Economics* (1890) and Ernst Engel's (1857) family budget study on the 'law of consumption'. The third area of research emerged after J. M. Keynes's *General Theory* (1936) and his idea of consumption function and the marginal propensity to consume. While all of these theoretical entities entailed their own problem sets and empirical models, the main difficulty for interwar econometricians was that they were forced to use limited information to conduct empirical inferences. If the scope of data were incompatible with their theoretical concerns, they would sometimes rely on idealisations to guarantee that the inferences were credible enough to travel across the epistemic boundaries between theoretical entities.

The chapter is structured thematically and chronologically to address these theoretical concerns. Sections 2 and 3 on the Engel curve discuss Arthur L. Bowley, a social surveyor and the first professor of statistics at the London School of Economics. The reason for isolating Bowley from the other practices was because of his importance in bringing randomised sampling into poverty surveys and modern econometric techniques into the microanalysis of family budgets. His study on family expenditure (Allen & Bowley, 1935) established an exemplar for estimations of the Engel curve. These contributions qualified Bowley as one of the first modern microeconometricians. Sections 4 and 5 chart the development of budgetary demand analysis and consumption function to demonstrate the respective difficulties interwar econometricians faced when evidencing economic theories via the estimates of budget materials. Section 6 introduces Jacob Marschak's 'pooling method' (1943), the first synthetic approach in demand analysis to combine family budgets and national income time series. Section 7 concludes the chapter.

2. Bowley as a Proto-Econometrician

In the United Kingdom, conducting household surveys to understanding social problems has a long tradition stretching back to the 18th century.[27] These social reformists were concerned with

[27] For a general history of this tradition, see Stone (1997) and Deeming (2010).

the empirical issue of poverty, especially how to demarcate the state of the poor – that is, the poverty line. This modern area of research began with Charles J. Booth's *Life and Labour of the People in London*, which surveyed around 180,000 working-class families and was published between 1889 and 1903. Booth's idea was then adopted by B. Seebohm Rowntree's (1901) poverty study on 11,560 working-class families in the City of York. Both Booth and Rowntree gave similar estimates on the proportion of the poor based on different demarcation methods (Deeming, 2010, 771–2),[28] and then explained the causes of that poverty by analysing people's economic and social backgrounds, such as unemployment, illness, and habits.

The studies of Booth and Rowntree can be interpreted as 'proto' econometrics at the household level while they were innocent of representative sampling techniques and economic theories. On the one hand, Booth's survey was designed without any rigorous criteria of representative sampling, as the concept of probability and randomisation were not prevalent among social researchers during Booth's era. Rowntree's study, to some extent, corrected Booth's problem by using a quasi-census study in an attempt to cover all working-class households of York. However, such a large sampling scale usually involved a costly research plan that constituted an entry barrier to social surveyors. On the other hand, focusing on the causes of poverty, Booth's and Rowntree's analyses applied mainly descriptive statistics to address the real-world phenomena. This factual information of economic variables was not prepared to confirm any economic theories or to test any models but to raise intellectual demands for additional policy reforms.[29]

When modern statistics emerged in the late 19th century, only a few statisticians were interested in using statistical data to explain economic phenomena. The British economic statistician who explicitly applied Karl Pearson's correlation analysis to economic time series was G. U. Yule, whose contributions to the time-series analysis of business cycles and his well-known curiosity in 'nonsense correlations' are well-documented (Yule, 1926; Morgan, 1990; Aldrich, 1995). In addition to Yule, two names appearing on John Aldrich's (2010, 117) list of 'economists' statisticians' were Francis Y. Edgeworth and Arthur L. Bowley. Edgeworth and Bowley both published extensively in the *Economic Journal* and *Journal of the Royal Statistical Society*, and they were recognised by contemporaries as economists and statisticians. While Edgeworth's work focused on mathematical statistics and economics, with little

[28] Booth had 30.7% and Rowntree had 27.84 percent.
[29] The idea of 'proto' econometrics assumes that econometrics requires the involvement of economic theories, corresponding to the aim of the Econometric Society quoted in Chapter I, 'the advancement of economic *theory* in its relation to statistics and mathematics' (Roos, 1933).

attention to statistical applications of real-world data, Bowley dealt with more empirical issues spanning from poverty surveys, official statistics, wages, cost-of-living index, and national income.[30]

Born in Bristol, a son of a minister in the Church of England, Arthur Lyon Bowley (1869–1957) studied mathematics at the University of Cambridge, where he obtained the BA degree in 1891 and MA in 1895.[31] His interest in social surveys likely originated from his gradual inclination toward Fabian socialism since the 1880s (Dale & Kotz 2011, 8). In 1895, encouraged by his teacher Alfred Marshall, Bowley started to lecture at the London School of Economics (LSE), a left-leaning social science research institution founded by the members of the Fabian Society. He became Reader in Statistics in 1908, Professor in 1915, and Chair in 1919. Before his retirement in 1936, he taught at Reading University College, University College London, and at St. John's School in Leatherhead. His lectures covered socialism, mathematics, statistics, and economics. He was a founding fellow of the Econometric Society in 1933 and president between 1938–9, and president of the Royal Statistical Society between 1938–40. Between 1940 and 1944, he succeeded Jacob Marschak as director of the Oxford Institute of Statistics.

Bowley began his career as a statistician. During the first few years at the LSE, he published two statistics textbooks that were considered vital introductory materials to the field (Bowley 1901; 1910).[32] Inspired by the Booth-Rowntree tradition, Bowley was the first to introduce the technique of representative sampling into poverty surveys in the UK (Bowley, 1906; 1913b; Aldrich, 2008). Although he was fully aware of correlation and regression analyses, Bowley approached his poverty surveys in a proto-econometric fashion. The reasons behind this were that the methodological standards of his surveys were Booth and Rowntree, and no microeconomic theory allowed him to analyse the microdata statistically.

Bowley's first use of the random sampling technique can be traced back to his presidential address to the Economic Science and Statistical Section of the British Association for the Advancement of Science.[33] Bowley (1906, 540) started by expressing his hopes that statistics would become an 'exact science' akin to the natural sciences. He thus distinguished between two types of statistics in the profession: *arithmetic* and *mathematical* statistics. The former provided a 'naked eye' comprehensive tabulation for observers, while the latter tackled the

[30] For a short summary of Bowley's work, see Darnell (1981).
[31] For Bowley's biography, see Dale and Kotz (2011, Chapter 1).
[32] Noted by his daughter Agatha Bowley (1972, 43), Bowley's *Elements of Statistics* (1901) was 'a standard textbook, virtually a best seller'. and by 1946 had 'run into six editions, with total sales of 12,500'.
[33] For a similar review, see Aldrich (2008, 10–2).

problem of statistical measurements that 'furnishes us with a microscope' (Bowley, 1906, 541). In other words, arithmetic statistics fits into the traditional meaning of data collection – detailed definition, categorisation, and tabulation. Mathematical statistics was more akin to statistical inferences, that is, obtaining estimates to aid in scientific reasoning. Bowley pointed out that although the U.K. government had made significant progress in arithmetic statistics around the late 19th century, this partial emphasis on data collection made statistics into 'only another name for accountancy':

> It is a sad reflection that, while so much care and labour are spent in accumulating and printing statistical tables, so few of them are of any real importance, and so few are intelligible, even to one who studies them carefully. (542)

He advocated that statisticians must realise their numbers were often flawed, and this shortage would threaten scientific preciseness. Therefore, shifting from data collection to reliable inferences was necessary,

> It must be recognised that most statistics are necessarily approximate; and just as in other scientific measurements the quantity is given as correct to so many significant figures, so in statistics the possible and probable limits of error should be estimated, and the false show of so-called mathematical accuracy given up. (543)
>
> We must candidly accept the fact that our raw material is imperfect, and our business is to remove the imperfections as far as we can, and, above all, to measure those we cannot remove. (546)

This dissatisfaction explained why Bowley urged that the current curriculum of statistics and economics should be revised to include more mathematical reasoning. In fulfilling this task, the professionalisation of statistics was the clear way forward – it was a decade ago before the LSE established the first chair of statistics in the United Kingdom for him. However, Bowley's point here is not only about statistics education in general, but the popularisation of statistical inference as a key in making statistics more scientific. During the first decade of the 20th century, there were still few analytical attempts to catch up with the increasing supply of official economic data. New statistical data were left *aliis exterendum* (for others to be threshed out) – an unambiguous fit to the motto of the Royal Statistical Society.[34] Bowley's view on statistics went beyond the society's motto and emphasised the role of statisticians as data interpreters rather than observers.

The antidotes Bowley suggested were the theory of probability and random sampling method that were 'persistently neglected' tools by practitioners when studying the actual distribution of economic variables (Bowley, 1906, 553). The method was based on

[34] For the debate on this motto within the society, see Hilts (1978).

Edgeworth's (1906) rationale of the 'law of great numbers', which argued that any sufficient number of randomised observations would converge to a true value.[35] Later in his address, Bowley presented the idea using the U.K. *Investor's Record* as an example of how an impartial selection of companies could be achieved by assigning quasi-random numbers. He first ordered all 3,878 companies from the record with numbers ranging from 1 to 3,878. Next, the quasi-random four-digit numbers were drawn from a table in the *Nautical Almanac*, in which the last digits were read consecutively while omitting numbers beyond 3,878. These four-digit numbers were subsequently assigned as the corresponding company numbers. As such, the numbers were drawn from a totally independent source that guaranteed the randomness of sampling. As Bowley confidently claimed,

> It was necessary to make certain, in some such way as this, that the *chances are the same for all the items of the group to be sampled, and that the way they are taken is absolutely independent of their magnitude*. (Bowley, 1906, 551)

Bowley then showed that from his sampling method, forecast values of the quasi-random samples from the record satisfactorily matched the actual distribution within calculated confidence intervals.[36] In this way, he also believed that such confidence intervals could be smaller under the law of great numbers:

> The *precision can be made as great as we please*, the probable and possible errors as little, *by increasing the size of the sample*. (552)

Thus, in his view, an actual randomisation coupling with the law of great numbers would preserve the credibility of sampling, and this 'very powerful weapon of research' (553) could be applied to the empirical research on poverty:

> It is frequently impossible to cover a whole area, as the census does, or as Mr. Rowntree here and Mr. Booth in London successfully accomplished, but it is not necessary. We can obtain as good results as we please by sampling, and very often quite small samples are enough; the only difficulty is to ensure that every person or thing has the same chance of inclusion in the investigation. (553)

Later on, Bowley brought this idea of random sampling into the field. In the fall of 1912, Bowley initiated a household survey in Reading to investigate the living conditions of working-class families (Bowley, 1913b). The Reading survey was Bowley's first attempt to put his 'inexpensive' idea of random sampling technique into practice, and the purpose of this study was to extend the empirical basis of the economic conditions in Britain:

[35] It should be noted that Edgeworth was not the creator of the law but Jacob Bernoulli and S. D. Poisson. See discussions by Stigler (1986, 182–6).
[36] Despite Bowley was not the first person who used probabilistic sampling. However, he was credited as the first person who produced confidence interval calculations using random samples. For a history of representative sampling, see Desrosières (1991).

> The results are of much more than local interest, since they prove that an inquiry adequate for many purposes can be made rapidly and inexpensively by a proper method of samples … sufficient number of people will be interested to carry out investigations in other towns and in rural districts, till we have general knowledge of the economic conditions of the *households* of Great Britain. (Bowley, 1913b, 672)

During the first phase, 1,950 households in Reading were selected according to the alphabetical order of streets; among them, 1,350 working-class houses were identified. Next, after around half of the working-class houses were interviewed, one out of every 21 houses was selected. In sum, information from 622 representative working-class households was gathered, including occupation, housing rent values, number of family members, working hours, wage income, and non-wage income. However, as the Reading survey concentrated on the poverty situation, only five observations on detailed family expenditure were collected.

Although the sampling method was improved, Bowley's statistical analysis of the Reading survey was proto-econometric. The inferences were based on the distributions of economic and demographic variables and the approximations of the total income of each household from certain assumptions (683). Income was then used to compare his criteria for the poverty line with Rowntree's.[37] Under this new standard, Bowley inferred that 29% of people in Reading were living in poverty (690).

With the same technique, Bowley conducted another three household surveys between 1912–3, including Northampton (891 samples, one in 23 houses), Stanley (204 samples, one in 17 houses), and Warrington (640 samples, one in 19 houses). Another survey in Bolton (3,650 samples) was conducted in 1914. Combined with the four groups of samples, the analysis was published as *Livelihood and Poverty* (Bowley & Burnett-Hurst, 1915), followed by a supplementary chapter on Bolton in 1920. The surveys were repeated later in 1923–4, and the results were published as *Has Poverty Diminished* (Bowley & Hogg, 1925).

Bowley's expertise in poverty surveys and statistical analysis led him to become one of the advisory board members of government statistics. He sat on the Select Committee on Income Tax in 1906 (BPP, HC 365) and the Departmental Committee on Trade Records in 1908 (BPP, Cd 4346). During World War I, Bowley engaged with the 1918 expenditure survey conducted by the Working Classes Cost of Living Committee (BPP, Cd 8980). In the 1920s, he was highly regarded as the authority of statistics for government officials.[38]

[37] A comparison of different concepts of poverty line from Booth, Rowntree to Bowley, see Hennock (1991).
[38] In the House of Commons sitting of Friday, 5th March 1926, Bowley's estimate on the cost of an unemployment bill was mentioned, 'I noticed that the Prime Minister a few days ago quoted with great approval the estimates which had been prepared by a very distinguished authority. Professor A. L. Bowley. I presume the Ministry of Labour will not contest the accuracy of that estimate' (BPP 1926).

In 1928, the New Survey of London Life and Labour was initiated by the LSE under the direction of H. Llewellyn-Smith. Bowley was one of the consulting committee members.[39] *The New Survey*, published in nine volumes between 1930–5, extended Booth's 'epoch-making work' (Llewellyn-Smith, 1929, 531) to investigate the living conditions of the working class in London. Bowley was responsible for the random sampling enquiry of 30,000 working-class households within the boroughs of London (17 Eastern and 20 Western).[40] Two substantial analyses by Bowley, both titled 'The House Sample Analysis', appeared in Volume III on the Eastern Area and Volume IV on the Western Area (Llewellyn-Smith, 1932; 1934).[41] For the sampling method, as Aldrich (2008, 45) has remarked, 'the scale was new but the method was not'. It was akin to what Bowley had done in the Reading survey. He outlined two methods of sampling. The first was to number all the household in a district then draw the samples using exogenous numbers. The second method was to select an initial number from one or two randomly, and then the next 50th household was chosen as the next sample. While Bowley had previously applied both methods,[42] he adopted the second so that no typical district would be oversampled:

> If the population is fairly homogenous in a district, but varies from one district to another, as is usually the case, this method has a slight advantage in precision over the first; it also gives a guarantee that no exceptional area is excluded, as might happen in a pure sample. (Bowley in Llewellyn-Smith, 1932, 32)

Again, Bowley's statistical analysis in the *New Survey* was a proto-econometric analysis of the proportion of poverty in London and an investigation of the causes behind it. Although the primary analytical framework remained identical, Bowley reported the partial correlations of rent on wages and numbers of people in the household in a small paragraph (56, cf. Dale & Kotz, 2011, 52).

Bowley usage of a correlation analysis was fairly rare in his poverty study,[43] but it was seen in his writings elsewhere. Back in the early 1900s, Bowley had already shown his grasp of the theory of correlation in the first edition of the *Elements of Statistics* (1901), where he not only provided a standard mathematical account of correlation analysis but two illustrative examples (Bowley, 1901, 316–27). He calculated the correlation coefficient of (1) marriage-rate and

[39] There were nine committee members including Bowley, William Beveridge and Lionel Robbins.
[40] As Llewellyn-Smith (1929, 542) noted, 'The supervision of this section [sample enquiry] of the New Survey is, I am glad to say, in Professor Bowley's hands, and the knowledge of this fact, and of the basis on which the sampling method is founded, will be a sufficient guarantee to Fellows of this [Royal Statistical] Society that the work is being carried out on sound and scientific lines'.
[41] For an exhaustive summary of Bowley's contribution to the *New Survey*, see Dale and Kotz (2011, 48–60).
[42] The first method was published in Bowley (1906), and a mixture of both was published in Bowley (1913b).
[43] It was also seen in Bowley and Hogg (1925, 177 and 180).

price of wheat between 1845–64 and 1875–94 and the regression coefficient of (2) daily maxima and minima of temperature in 1898. A causal condition of the correlation coefficient was proposed,[44] and a 'biological language' (325) of regression was highlighted. He thus argued that the apparent application of the theory of correlation would be in biology:

> There is an intimate relation between the law of error and biological theory … The law of heredity can be only tested numerically by the theory of correlation; the effect of natural selection is easily considered with the help of the coefficient of regression. (Bowley 1901, 325)

In his 1906 presidential address, he argued that the theory could be used to reveal causal relations corresponding to his causal condition:

> In most cases of cause and effect, and in general in testing the independence of phenomena, we have to use the mathematical measure of correlation, a subject whose importance demands much more than the brief mention here given. (Bowley, 1906, 554)

Despite this confidence, however, in the first three versions of the *Elements* (Bowley 1901; 1902; 1907), Bowley did not address the possibility of applying correlation analysis and regression technique to economic reasoning. The regression analysis was not even mentioned in the earlier versions of his introductory textbook, *An Elementary Manual of Statistics* (Bowley, 1910; 1915).[45] The reason behind this reluctance was that the theory of statistical error at the time was too underdeveloped to account for human actions:

> The great difficulty which the student of economics encounters when dealing with the theory of error is apparent slightness of relation between this theory and the facts with which he deals. This slightness is only apparent; it is because the theory has not, in the form he meets it, been carried far enough to fit it to the very complex facts of human affairs that we do not get that exact correspondence we might desire. (Bowley, 1901, 325–6)

Bowley's position towards statistical economic reasoning was clear. Despite the potential power of the correlation analysis, credible statistical inferences in economics required explanatory economic theories that well characterised statistical errors. In other words, without a cautiously identified economic hypothesis, many statistical relations were simply accidental, and the regression analysis would not capture those complexities. This scepticism was also reflected in his empirical writings before the 1930s. For instance, Bowley (1913a, 523) asserted that his findings on the statistical relationship between the wholesale and retailed prices of food could be unstable:

[44] Bowley (1901, 302) proposed that the causal relation existed as the correlation coefficient was six times more than its probable error. For further elaboration on his concept, see Morgan (1990, 137).

[45] In *An Elementary Manual of Statistics*, the method of least squares was firstly added in the sixth edition (1945, 46–51) that Bowley commented in the preface, 'statisticians will notice that regression equations are developed without any reference to the correlation coefficient or to the method of least squares, which are dangerous weapons except in the hands of the expert' (v).

> There is, of course, no guarantee that this relation between wholesale prices and the retail prices … existed prior to 1896, nor that it will continue … The details behind the averages are so variable that this apparent constancy may be partly accidental.

Furthermore, Bowley (1922, 198) stated the following on the statistical relationship between the cost-of-living and wholesale indexes:

> This formula [of the relations] does not express a law of nature or of economics, but is merely an empirical equation whose numerical constituents will be gradually modified, and it is liable to fail whenever there is any temporary disturbance in the retail price of a seasonal commodity.

Such empirical interpretations were not uncommon during the early 1920s, especially since Bowley's analyses of macro-level indices did not usually call for a fully fledged theoretical basis.[46] But when it came to micro-surveys, the theoretical requirement of demand analysis was the opposite. Just as empirical economists interpreted the price-quantity relation as a testable scientific hypothesis, the microdata of household budget on income and expenditure required the justification of a hypothesis of income-expenditure relations. Therefore, when Bowley started to apply the regression analysis to microdata, not only were the budget materials, but there was no coherent theoretical treatment of household behaviour for analysing the expenditure-income curve and its statistical error. For instance, in the fourth edition of the *Element of Statistics* (1920, 400–1), although Bowley added a regression exercise exploring the relationship between family expenditure and its size from the 1918 working-class cost-of-living survey, he did not comment on the results or link them to any economic interpretation. Furthermore, Bowley (1933) used the weekly wage data from the *New Survey* to test the normality of income distribution, but he did not end up fitting any budget data to any economic model.

3. Bowley as a Microeconometrician

Bowley's statistical analysis of economic microdata remained proto-econometric until later on when the proper theory was finally proposed. In 1934, Bowley's LSE colleagues, R. G. D. Allen and J. R. Hicks, published two articles on the theoretical foundations of the individual income-expenditure curve (Hicks & Allen, 1934a; 1934b). The Hicks-Allen thesis was the crucial turning point that provided Bowley with a solid rationale to econometrically analyse microdata. In 1935, he published an econometric study on the Engel curve with R. G. D. Allen

[46] Documented in Morgan (1990, 83–100), the idea of connecting a logical theory to time-series analysis was uncommon before Ragnar Frisch's work in the mid-1920s.

(Allen & Bowley, 1935). The study was one of the most representative interwar exemplars of microeconometrics and best demonstrated Bowley's transition from a Booth-inspired proto-econometrician to a modern econometrician.

In 1934, Bowley's colleagues, John Hicks and Roy Allen, who joined the LSE in 1926 and 1928, respectively, published two seminal papers titled 'A Reconsideration of the Theory of Value' (Hicks & Allen, 1934a; 1934b). The Hicks-Allen thesis revived Pareto's concept of ordinal utility and formalised consumer demand theory within the framework of diminishing marginal utility (Moscati, 2007a, 139–40; 2018, 97–9). In the first part, Hicks deconstructed the price elasticity of demand into income and substitution elasticity; in the second part, Allen supplemented the mathematical derivations.[47] As such, one of the most critical implications of the Hicks-Allen thesis was that under the assumption of diminishing marginal utility (convex preference), *ceteris paribus*, the increments in income would increase the expenditure on a normal commodity. In other words, the income-expenditure curve under such a framework would be upward-sloping.

The Hicks-Allen explanation of the income-expenditure curve was not entirely new. In the mid-19th century, Ernst Engel (1857) studied the budget survey of 153 Belgian working-class families collected by Édouard Ducpétiaux. After comparing the percentage of food consumption to the living conditions of families,[48] Engel posited a law of consumption, which would eventually be named after him: 'The poorer a family, the greater the proportion of its total expenditure that must be devoted to the provision of food' (Engel, 1857, 28–9; cf. Stigler, 1954, 98). Once social scientists had recognised Engel's law, the empirical income-expenditure relation was gradually problematised as the Engel curve.

Until the 1930s, Engel's law was still understood as an inductive-statistical law that did not engage income theory.[49] For instance, as one of the earliest interwar practices, W. F. Ogburn (1919, 36) claimed that his Engel-curve estimation 'generalizes from the sample in such a form as to permit a large number of comparisons' without mentioning any theoretical economic relations.[50] In this sense, the Hicks-Allen thesis provided Engel's law with economic meaning

[47] William Baumol (1972, 505) sees the thesis as a crucial rediscovery that founded the neoclassical demand analysis after Eugen Slutsky.
[48] The living conditions were categorised into 'on relief', 'poor but independent', and 'comfortable' (Engel, 1857, 27).
[49] For historical explanations of this delay of income theory, see Stigler (1954, 102–3).
[50] Ogburn (1919) estimated the Engel curve from a budget survey of 200 households in Washington, D.C. under the model,

$$e_i = a + by + f$$

where e_i was the expenditure on item i, y was income, and f was family size. He reported the regression coefficients on every commodity, as well as their partial correlations.

and showed that individual expenditure could be explained through preferences and income. Once buttressed by economic theories and deductive logic, the law was no longer inductive-statistical but deductive-nomological, meaning that the relationship between income and expenditure was causally reliable and statistically measurable.

Working closely with Allen, around 1934, Bowley discovered the possibility of using regression analysis and household expenditure surveys to explore Engel's law within the Hicks-Allen framework.[51] The outcome of their project was collected in the book *Family Expenditure: A Study of its Variation* (Allen & Bowley, 1935). This 'econometrical' study, as Bowley defined, 'in the sense that it attempts to apply measurement to economic actions' (3), was a neoclassical synthesis of family budget materials in three parts: the first two chapters studied the empirical income-expenditure curve at the average level (Chapter I) and at the individual level (Chapter II) using multiple budget surveys. The last chapter (Chapter III) was the Hicks-Allen thesis that justified the previous chapters' econometric analysis. Chapter I adopted a simple model for studying the relationship between individual income and expenditure. Denoting the household income as y and its expenditure as e, the first approximation of the income-expenditure curve was written in two rectilinear forms:

$$y = ke + c$$

$$y = ke + bn + c$$

where k, b and c were constants, and n was an index of the family size measured by the numbers of equivalent adults. As Bowley noted, this relationship expressed Engel's law but repositioned in a neoclassical framework:

> In a homogeneous group of families differing only in respect of income, the excess over (or defect from) the average of expenditures on any budget item bears a constant proportion to the excess over (or defect from) the average income. In the case of some goods, which may be described as *necessaries*, this rule results in a diminishing proportion of expenditure as income rises. In the case of other goods, which may be described as *luxuries*, the proportion of expenditure rises as income rises. (Allen & Bowley, 1935, 7)

The definition showed that the law was no long Engel's statistical observations but a carefully defined theoretical object. To test the formulas, Bowley calculated the average expenditure of different income groups on commodities using 21 budget materials, some from the UK and some from other European countries collected in Staehle (1935). The point estimates of selective materials were plotted in an XY graph. Figure 3 shows an illustrative

[51] It is still unclear when Bowley picked up Engel's work. The only thing can be shown here is, prior to 1935, Bowley never mentioned Engel in his writings.

example of a survey of 1,944 workers conducted by the U.K. Board of Trade in 1904 (BPP, Cd 2337).

In Figure 3, the x-axis is average weekly income, and the y-axis is expenditure. Three lines for food, non-food, and bread were fitted. After showing the results from multiple budget studies, Bowley argued that his empirical findings portrayed a positive linear relationship between expenditure and income across countries, even after dividing the expenditure by the number of equivalent adults. He thus summarised his findings with the 'linear expenditure law',

> The averaged budget collections illustrated by these diagrams have been selected from a much larger number of collections, all of which tend to show that the hypothesis of the straight line expenditure relation is not unjustified. (Allen & Bowley 1935, 21)

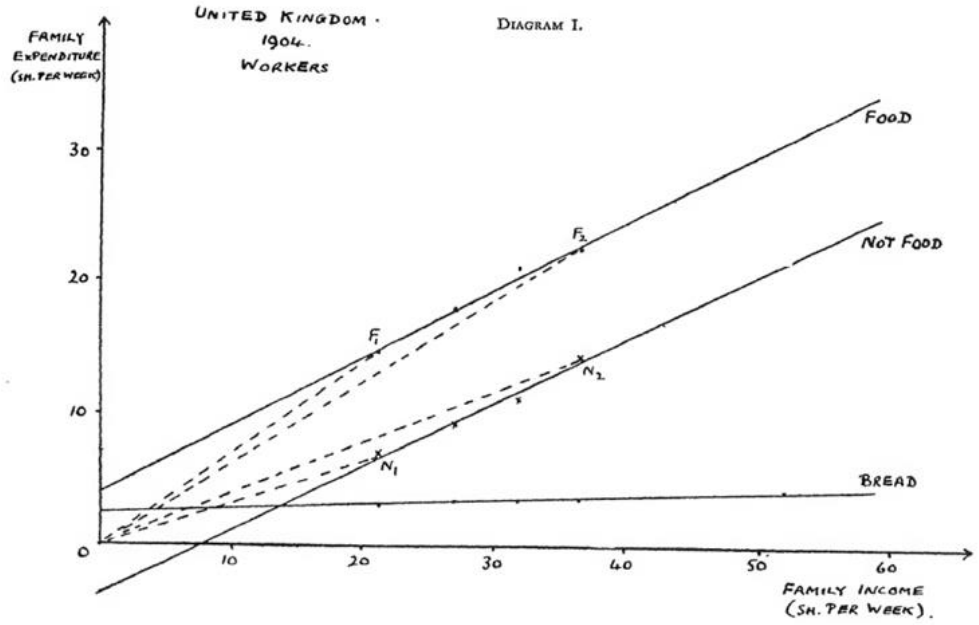


Figure 3 The Engel curve with family income on the x-axis with four point estimates (Allen & Bowley 1935, 6)

Chapter II of Allen and Bowley (1935) proceeded to examine the linear expenditure law in specific budget collections with full details on income and expenditure. Instead of plotting the group averages, Bowley fit the budget data into a classic least-square equation,

$$y_t = ke_t + c + v_t$$

where e_t was the total expenditure, y_t was the expenditure on item y, and v_t was the residual of expenditure for the tth family. The income elasticity of y could be determined after estimating k and c, and v_t presented other variations explained by tastes and habits.

The main issue was whether v_t was normally distributed, or, in other words, whether the variations caused by other factors were accidentally observed. Therefore, Pearson's chi-square test was applied to test the normality of residuals. The budget survey of 154 working-class families in Liverpool (Jones, 1934) was used as an illustrative example, where e_t was defined as the total food expenditure and y_t the vegetable expenditure, both divided by the number of equivalent adults. The regression results yielded $k = 0.07$ and $c = 0.185$. After showing the confidence interval of v_t (Figure 4) and confirming the normality of its frequency distribution, Bowley concluded that he had provided 'a fairly complete account of the phenomena of the variation of tastes for the vegetable food group in this collection of budgets' (Bowley & Allen, 1935, 76). The same procedure was subsequently applied to analyse the expenditure variations of the budget materials of English towns in 1926 (194 families in Jones ,1928), of LSE in 1932 (123 families, collected by William Beveridge and the B.B.C.), of Hamburg and Bremen in 1927–8 (104 families, collected by German officials), and of U.S. farmers (269 families, collected by Helen Canon at Cornell University). The hypothesis of the normality of tastes was again confirmed.

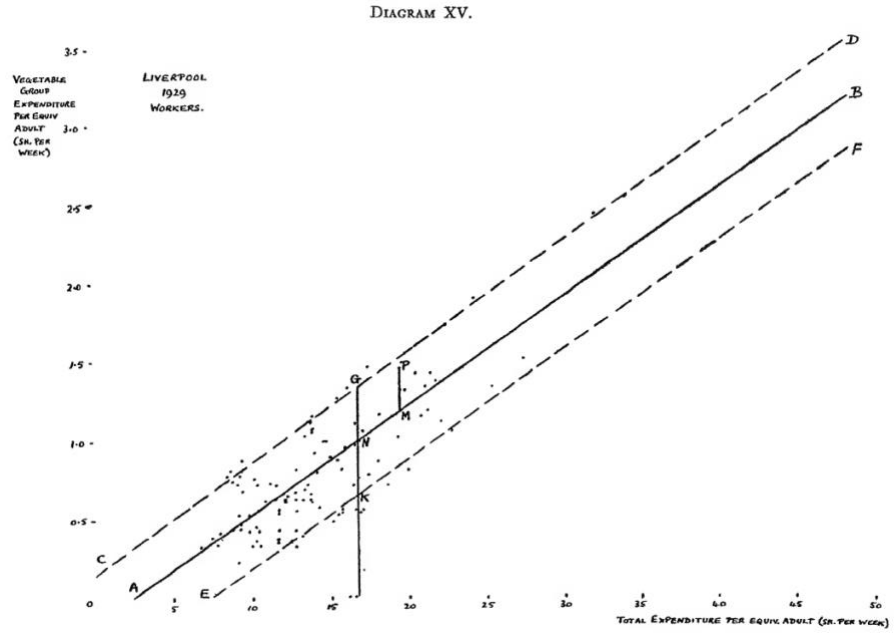


Figure 4 The relationship between total food expenditure (x-axis) and vegetable expenditure (y-axis) per equivalent adult in the Liverpool survey (Bowley & Allen, 1935, 75)

In the last part of Chapter II, Bowley examined the correlation between the expenditures on different commodities to determine whether two commodities were substitutes or complements. The model was written as a partial correlation framework. The expenditure on a commodity

was explained by the total expenditure and the expenditure on another commodity. Estimates of the correlation coefficients on certain budget materials were reported.

The Bowley-Allen study was the first exemplar in the history of microeconometrics in terms of the theory tested, data fitted, and method used. Although not all data from all surveys were regressed, its style of analysis was similar to modern econometrics, from the use of economic theory and econometric model to the statistical analysis of microdata. One of the main purposes of the study was 'to relate any rules that are found to the postulates of economic theory' (Bowley & Allen, 1935, 1). Their study was based on the Hicks-Allen neoclassical theory, which rationalised Engel's law from the assumption of stable preferences and the equilibrium between budget allocation and indifference curve. There was not another statistical analysis of microdata before the mid-1930s embedded in such microeconomic theory.

Second, the scale of microdata used by the Allen-Bowley study was large compared to the contemporary standards. Since the 1920s, many different governments have started to collect their budget data. While most of the data he used were either from government officials or academics worldwide, Bowley's impact as a statistics authority in the UK might have accelerated the data collection process. In fact, he had been using the budget data listed in Staehle (1935) before the latter appeared in *Econometrica* (Allen & Bowley, 1935, v).

Third, the Hicks-Allen thesis helped to justify Bowley's empirical reduced-form model. Their rationale implied that, if the individual preference was stable, Bowley's regression framework would be the first to capture the behaviour of expenditures. Therefore, the purpose of Bowley's econometric analysis was to test the statistical stability of individual preference that was mainly captured by the residual in each regression or translate to the modern econometric language, to test the error term's normality. What was different to modern econometrics was that Bowley did not attribute the residual to unobserved or omitted variables, but only to, without extensive discussion, the fundamental difference in household decisions.[52] This strategy of model specification perfectly corresponded to the Hicks-Allen framework, as well as their quote at the beginning of the introduction, 'There is no accounting for tastes./Different people have different opinions./Some like apples and some like onions' (Allen & Bowley, 1935, 1).[53]

Finally, Bowley adopted the scatter plot and confidence interval to visualise residuals. He also attempted to adjust his data by family size to see whether the estimates could be improved

[52] Using the Gaussian law of errors to explain human variations could be traced back to the work of Adolphe Quetelet in the 1840s. For this history, see Porter (1986, 100–9).
[53] Allen and Bowley did not identify the source of this quote.

enough to perform the chi-square test when checking the normality of residuals. Furthermore, he was aware that some of the budget data was imperfect. For instance, he commented that the budget data collected by the LSE might suffer from a sample selection bias,

> The budgets [from the LSE] were, however, submitted by a self-selected group of families in response to broadcast appeals. The families are thus not homogeneous in social class and they come from various parts of the country. The collection can be taken as largely middle-class and professional but a certain number of more definitely working-class families are also included. (Bowley & Allen 1935, 80–1)

These elements made the Bowley-Allen study a pioneering effort of microeconometric practices. Their linear expenditure model was based on the postulate from the economic theory of individual behaviours. The budget data was rigorously examined in the sense of error-term normality, and justifications of data sources were carefully discussed. The contribution of this study was soon recognised; it was reviewed by Faith Williams (1936) and then Henry Schultz (1936). The editor noted that the two reviews were arranged because of 'the nature and importance of this book' (Williams, 1936, 610n). Econometricians took its empirical framework as an exemplar for estimating the Engel curve. For instance, it was referenced as a starting point in both Kaplan's (1938) analysis of the consumer expenditure of urban households in Chicago and Denver from the 1935–6 Survey of Consumer Purchases,[54] and another canonical study by Prais and Houthakker (1955), which will be discussed in Chapter III.

4. Interwar Budgetary Demand Analysis

In the first two decades of the 20th century, statistical demand analysis appeared on the empirical economist's research agenda. At the first stage, most practices focused on the time-series analysis of agricultural goods (Moore, 1914; Schultz, 1925a; 1925b). These macroanalyses on individual commodities were soon recognised and brought into agricultural economics. Meanwhile, while these economic time series suffered their own problems, corresponding econometric techniques were developed to address the gap between the static theory of demand and the dynamic characteristics of time series (Morgan, 1990, Chapter 5). Although early examples of demand analysis were many, it was not until the 1930s that empirical economists started to consider applying family budget materials to demand analysis. This 'considerable revival' (Morgan, 1990, 152) of cross-sectional studies of demand

[54] Other examples included Leser (1941) and Nicholson (1949).

distinguished the interwar microeconometric practices as a separate subject from macroeconometrics.

A. C. Pigou (1910) created the first empirical framework for using budget data to estimate the household demand elasticity of a single commodity. He derived that, under an economic equilibrium, the ratio of the elasticity for commodity x (noted as e_x) to the elasticity for commodity y (noted as e_y) could be presented as the mathematical relationship below,

$$e_y = e_x * \frac{x_1}{x_2 - x_1} * \frac{y_2 - y_1}{y_1}$$

From the equation, x_i and y_i denoted the annual consumption of the *i*th group for that commodity. The crucial assumption that guaranteed the relationship was that the preferences between two groups were 'approximately the same' (637) such that among them, the marginal utility of money remained a constant. Pigou thus argued,

> But since a small change in the consumption of any ordinary commodity on which a small proportion of a man's total income is spent cannot involve any appreciable change in the marginal utility of money to him, the elasticity of the utility curve in respect of any consumption x_1 is equal to the elasticity of the demand curve in respect to that consumption. (637–8)

Based on this reasoning, Pigou calculated the elasticity ratios for clothing consumption relative to food for five wage groups using the aggregate figures from the second *Fiscal Blue-book* (BPP, Cd 1761). However, apart from a review by Milton Friedman (1935), no similar practices were produced over the next two decades after the paper was reprinted in Pigou's *Economics of Welfare* (1920). As George Stigler (1954, 108) noted, the method was 'never employed except in Pigou's own illustrative calculations'.

Until the interwar period, Pigou's method regained econometricians' attention when Jacob Marschak (1931) outlined another empirical framework for estimating price elasticities of commodities using microdata. Marschak's study assumed that the quantity of commodity consumption q could be written as a function of household income ρ divided by its price p,[55]

$$q = f(\frac{\rho}{p})$$

Two assumptions were at work here. First, the ratio of income to price (ρ/p) was a constant, which indicated that any income variation would only comove with the price of commodities and vice versa. Second, p remained unchanged under the fixed period of the budget survey. Assuming that $p = 1$, the individual demand for a commodity was simplified as:

[55] Marschak's book was written in German. The analysis here relies on two reviews by Gilboy (1931) and Schultz (1938, 117–9) to reconstruct Marschak's method.

$$q = f(\rho)$$

Three auxiliary assumptions were added to ensure that the equation above represented a true household demand. First, like Pigou's method, the consumption of that commodity was proportionately small to the total expenditure. Second, all prices of other commodities moved together. Finally, there were no significant substitutes for that commodity. As such, with the function of the income distribution denoted as $n(\rho)$, the market demand Q for a commodity could be written as,

$$Q = \int n(\rho) f(\rho) d\rho$$

Marschak argued that, although what Q actually measured was the aggregate income-expenditure curve, based on three additional assumptions above, this curve could be seen as the mirror image (*Spiegelbild*) of its demand curve. In other words, the price elasticity of a commodity could be obtained by taking the negative sign of its income elasticity. Accordingly, Marschak calculated the average values of the aggregate income elasticities for seven commodities using the 1907–8 German family budget surveys and then took their negative sign to derive the price elasticities.

While Pigou's method only calculated the relative ratio of the price elasticities of two commodities, Marschak's method was carried out based on the assumption of a functional relationship between the quantity consumed and household income. Although their underlying methodologies were different, econometricians in the 1930s tended to group them together. For example, in a footnote, Gilboy (1932, 376n) stated that the works of Pigou and Marschak were 'most noteworthy in this group'. Two reasons can explain this grouping. First, their methods were rare methodological alternatives that adopted budget materials for measuring price elasticities. On this point, Marschak believed that that the method of *Spiegelbild* had an advantage over the Moore-Schultz approach in time-series econometrics, which failed to eliminate disturbances from the annual time series (Schultz, 1938, 119). Second, they relied on strong assumptions about individual preference and commodities to validate their arguments. Due to the nature of budget data, the data-theory gap that Pigou and Marschak addressed was not time trends like Moore and Schultz, but how to transform income-expenditure relations into price-quantity relations. In Pigou's case, the assumption that the individual's marginal utility of money was a constant warranted that his ratio of household price elasticity needed only consumption data.[56] In Marschak's case, the price-quantity curve could be interpreted as

[56] This inference was dubious to Schultz and Friedman, see Friedman (1935) for his detailed criticism.

the mirror of its income-expenditure curve after ruling out other potential threats through idealisations of the commodities.

One year after Marschak, in his *New Methods of Measuring Utility* (1932), Ragnar Frisch proposed a new empirical framework called 'the translation method', which measured the relationship between the marginal utility of money and real income. Frisch's model considered two crucial price variations: the price of the commodity (p) and the price of living (P). Defining $u(x)$ and $w(r)$ as degrees of utility obtained from the expenditure of the commodity x and the real income r, Frisch derived the equation under equilibrium,

$$w(r) = \frac{P}{p}u(x)$$

Assuming that the utility functional forms of $u(x)$ and $w(r)$ were identical, the quantity of the utility measures could thus be standardised. Under such a framework, as family budget data contained the information of nominal income and total expenditures on each commodity, combining with price data was sufficient for calculating the quantity consumed and the real income. Using the 1918–9 U.S. Budget Study of 92 cities,[57] he then plotted the relationship between r and x of eight cities, all of which was adjusted by the magnitude of average family size. He then plotted the marginal utility of money ($\frac{d\ log\ w(r)}{d\ log\ r}$) and r. The pattern suggested a decreasing marginal utility of money with respect to real income.

While both Pigou and Marschak assumed the price factor away, Frisch's treatment of budget data based on the prices of single commodities was rare. However, Frisch measured household income-expenditure curves, and he did not try to transform them into price-quantity curves. As a result, since Frisch's method was only a by-product of measuring utilities, it was not generally perceived as a potential method for acquiring budgetary price-quantity curves. The person who noticed the potential of Frisch's inquiry was Henry Schultz (1938, 116), who stated, '… this [demand] curve can be obtained by fixing the income of the individual at a particular level and observing how his consumption of the commodity in question varies as its price is changed'.

During the early 1930s, heated debate continued over Marschak's method, which offered an approach other than time-series econometrics.[58] In its first review in the English-speaking world, Harvard economist Elizabeth Gilboy (1931, 667) cast doubt on his 'very dubious'

[57] This survey, however, did not record income directly, but it provided the total expenditure of households for different commodities as well as family deficits that allowed Frisch to estimate family income (Schultz, 1933, 105).
[58] The reviews were mostly in German. One in French was written by Frisch (Moret & Frisch, 1932).

assumptions about the constant price elasticity of demand and the independence of price.[59] Regardless, she lauded the book as 'a valuable contribution to demand analysis, especially since it comes at a time when many investigators are skeptical as to the further possibilities of market data'. Gilboy suggested that econometricians were becoming interested in using budget materials as an alternative to the aggregate time series. In line with this scepticism, Gilboy (1932) initiated a cross-sectional study that collected the income and expenditure data of 24 households sampled by herself at Harvard. The questionnaire was designed to understand the consequences of hypothetical changes in price and income. The conditions were stipulated as follows:

a. If income increased 10, 25, 50, and 100 per cent; and decreased 5, 10, 25, and 50 per cent.
b. If income remained the same, and the price of each commodity or group of commodities increased 10, 25, 50, and 100 per cent, and decreased 5, 10, 25, and 50 per cent.

(Gilboy, 1932, 377)

Gilboy distinguished two types of curves in her research design: the price-quantity 'orthodox' demand curve and the income-quantity 'Marschak' curve. After tabulating information about the 24 household budgets in detail, eight random samples were plotted to show how quantity consumed changed when its income or price changed in double logarithmic scales, as presented in Figure 5. Based on the results, Gilboy's interpretation was threefold. First, she cast doubts on the constant-demand-elasticity hypothesis uncritically accepted by Schultz, Marschak, and Wassily Leontief.[60] Second, Marschak's mirror-image argument was not empirically verified. Conversely, her samples responded to identical changes of income and price quite differently. Third, the income-quantity curve was generally more elastic than the price-quantity curve.

Gilboy's critiques of Marschak's method showed that the income-expenditure curve from budgetary data could not be applied as a mirror of the price-quantity curve if the price factor was absent. Afterwards, this gap of budget materials was recognised by Hans Staehle (1934, 355) in his survey article on family budgets,

> It seems clear, therefore, that owing to price variation and irregular flow of income in time and to price differences in space and between qualities of the same good, essential

[59] Elizabeth Waterman Gilboy (1907–1973) obtained her AB degree from Barnard College in 1924 and PhD from Radcliffe College in 1929. Initially working on economic history, Gilboy shifted her research toward personal income, expenditure, and consumption in the 1930s. She was secretary of the Harvard Committee on Economic Research and was affiliated with the Office of Strategic Services during the war. She was associate director of the Harvard Economic Research Project, a research project on input-output analysis founded by Wassily Leontief in 1948. From 1957 to 1965, Gilboy was acting director of the project.

[60] See Gilboy (1931b) for her evaluation of Schultz's and Leontief's methods of demand analysis.

conditions for the attainment of consumers' equilibrium are not fulfilled by the ordinary budget material.

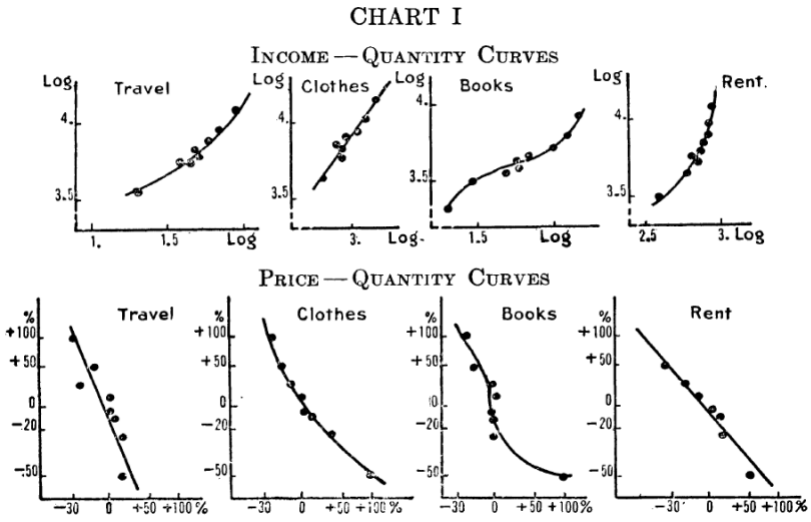


Figure 5 The income-quantity curve (up) and price-quantity curve (down) measured by the percentage variations (Gilboy, 1932, 381)

Despite these shortcomings, however, Staehle did not abandon his research on budgetary analysis. Instead, he clarified the estimation of income elasticity as 'Marschak's problem' and argued that the problem 'would yield indications of some importance' after the price variation was satisfactorily explained (Staehle, 1934, 357). On this point, he held that investigating the expenditure variation of households had merit in testing the homogeneity of individual decisions. Thus, he urged econometricians to explore this topic further, with the hope that Bowley's 'first step', which resulted in Allen and Bowley (1935), could provide an informative start,

> At any rate, however, it would greatly facilitate the work of the econometrician, and certainly improve the quality of the materials, if Bowley's first step in the direction of supplying some measure of dispersion together with each average could be adopted generally. (Steahle, 1934, 358)

5. Interwar Budgetary Consumption Function

In Keynes's *General Theory* (1936), the consumption function described the positive relationship between national consumption and income. The function's parameter, the national marginal propensity to consume (MPC), was one of the crucial assumptions of Keynesian macroeconomics when calculating the spending multiplier. Since the publication of *General*

Theory, economists have been curious about the theoretical structures of the consumption function and empirical estimates of the MPC. While most of the theories, including those of Duesenberry (1949), Modigliani and Brumberg (1954), and Friedman (1957), were finished in the post-war period, studies on the empirical consumption function proliferated from 1937 to 1950, in which using cross-sectional budget surveys characterised the first stream of this literature in 1937–40 (Thomas 1989, 134).

The very first article on empirical consumption function in Keynesian terms using budget data was written by Cambridge graduates Richard and Winifred Stone,[61] and their intention could not be more explicit from the title 'The Marginal Propensity to Consume and the Multiplier: A Statistical Investigation' (Stone & Stone, 1938). The Stones adopted three empirical methods to explore Keynes's 'general psychological law' elaborated by Keynes (1937, 219–20):

> … when aggregate income increases, consumption-expenditure will also increase but to a somewhat lesser extent. This is a very obvious conclusion. It simply amounts to saying that an increase in income will be divided in some proportion or another between spending and saving, and that when our income is increased it is extremely unlikely that this will have the effect of making us either spend less or save less than before.

Keynes suggested that the proportional relation between national consumption and income existed, which was theorised as the MPC. To explore the MPC empirically, Stone and Stone's (1938) 'budget method' fitted the average income $\bar{y}$ and expenditure $\bar{c}$ of different income groups from three budget surveys (the US in 1929, Germany in 1926–7, and Japan in 1926–7) into two regression equations,[62]

$$log\bar{c} = a + blog\bar{y}$$

$$log\bar{c} = a + blog\bar{y} + clog^2\bar{y}$$

where the individual propensity to consume k and its aggregated term K (i.e., the MPC) were calculated through,

$$k = \frac{d\bar{c}}{d\bar{y}} = log^{-1}b\bar{y}^{b-1}, \text{ and } K = \frac{\Sigma[y\frac{d\bar{c}}{d\bar{y}}]}{\Sigma y}$$

The budget data fitted was the household income and expenditure of all surveyed groups that relied on only a few point observations (US: 28; Japan: 9; and Germany: 7) according to different income levels. They estimated that the household MPCs ranged between 0.5 and 0.73, indicating that the Keynesian multiplier $1/(1-K)$ was between 2.0 and 3.7. They proceeded

[61] The only study left out from Thomas's (1989) list was Kaplan (1938), which was excluded since its goal was clearly not the Keynesian consumption function but Engel's law.
[62] For discussions on the other methods of this article, see Chapter III.

to analyse U.S. savings and found that the lowest-income class accounted for 4% in total saving with around 15% of the savings rate, suggesting the working-class saving was not a negligible term for the future analysis.

The log-log econometric framework adopted by the Stones was used in Elizabeth Gilboy's (1938) study of the income-expenditure curve examined through a wide range of budget evidence in the United States and her focused study on the 1935–6 Survey of Consumer Purchases (Gilboy, 1941). Unlike her previous article on the budgetary demand curve (Gilboy, 1932), she reframed her earlier empirical findings on group studies under the Keynesian theory of the MPC. She then plotted the household income-expenditure curves on different commodities for urban and farm families and occupational groups. How she related her findings to Keynes's inquiry was, again, through the analysis of saving behaviour. She found a negative relationship between household income and its elasticity of saving and constant between income and total expenditure among the farm families. While the statistical evidence suggested heterogeneous responses to income changes from different income groups, Gilboy (1938, 140) concluded that 'the relationship between income, consumption and saving is neither as simple nor as stable as Mr. Keynes assumes in his statement of the propensity to consume'.

Gilboy's estimates demonstrated that it is impossible to regard the household MPC based on budget studies as the Keynesian aggregate MPC since the budget data provided only partial information about the whole population. Therefore, Gilboy shifted her methodological focus to the impact of income distribution,[63] echoing Staehle's (1937, 142) earlier findings through his time-series analysis of the quarterly data of Germany in 1928–34, stating 'the more income is concentrated in the hands of receivers of large incomes, the smaller … will be the proportion of total income spent on consumption goods'. Both Staehle and Gilboy pointed out that Keynes's assumption of the national MPC without addressing the income distribution was oversimplified and unrealistic. As recorded in Thomas (1992, 158–65), Keynes entered the dispute later and insisted that the empirical estimates of the MPC were not the essential concern of his fundamental psychological law. The debate had no clear conclusion in the end.

This methodological add-on was addressed in the later literature in two ways. First, the theoretical framework was mathematicised in Marschak (1939b, 166), where he argued that the aggregate MPC was 'the weighted average of the personal marginal propensities' with 'the weight being the marginal shares of the various incomes'. Second, the empirical concern was

[63] The early arguments on income distribution are explored in detail by Thomas (1992).

further explored by Horst Mendershausen (1939; 1940). With the same data from the Survey of Consumer Purchases, Mendershausen experimented with the quantitative relationship between income X and savings s, measured by a family's net change in assets and liabilities from eight metropolitan areas in a hyperbola form,

$$s/X = a + bX + c/X$$

Instead of linking to Keynesian psychological law, Mendershausen's analysis concentrated on income elasticity and its distribution, echoing a previous income-saving study conducted by the Brookings Institution (Leven, Moulton, & Warburton, 1934). The econometric analysis showed that the saving behaviour and consumption pattern were similar to the results of Brookings but significantly different between the white and African-American communities.

After Gilboy's (1940) summary of this literature, this first line of research on the budgetary consumption function declined in the late 1930s. After 1940, the literature almost disappeared and was replaced by a significant outpouring of aggregate time-series analyses. As summarised in Thomas (1989, 134), practices of household consumption function occupied almost 50% of the studies (5 out 11) from 1937 to 1940 and only 16.6% (6 out of 30) between 1941 and 1950. Thomas (1989, 136–7) attributed the early boom of budgetary studies to a consequence of the time when the national income data was not prevalently accessible. Furthermore, this literature was a continuation of previous budgetary studies on household expenditure; as Thomas (1992, 157) commented, 'the response in the early studies was not a macroeconomic analysis of aggregate consumption behavior, but rather a microeconomic analysis of Keyne's theory of household behavior'.

There are some points to be added to Thomas's explanations. First, the key to the emergence of budgetary consumption function before 1940 was the 1935–6 Survey of Consumer Purchases. This budget material appeared at the time when Keynes raised the issue of the MPC, following his publication of *General Theory* in 1936. Thus, it was an intuitive move for U.S. economists to utilise the newest dataset in testing the latest theory across the Atlantic. Second, Stone and Stone's study (1938) was the earliest example that applied various methods and sources for obtaining the MPC. The Stones study might indicate that Richard Stone's early career as a managing editor of the *Trends* allowed him more access to relevant data.[64] Finally, the reappearance of Staehle, Gilboy, and Marshak in this section reflected that, despite the variations in the theoretical scenes and datasets, interwar microeconometricians were still few in number. This point will be again exemplified in the following section.

[64] See further histories of Stone in Chapter III.

6. Marshak's Pooling Method

In the late 1930s, microeconometricians had learned three methodological lessons from studying budget materials. First, from the Hicks-Allen thesis, the price effect could be arguably decomposed into income and substitution effects, and from the Allen-Bowley study, testing Engel's law was equivalent to measuring the income effect. Second, using the income-expenditure curve in approaches to the demand curve seemed increasingly implausible after Hicks and Allen distinguished their theoretical contents and Gilboy tested the empirical differences. Finally, Gilboy's study confirmed that the household MPC suffered from the aggregation problem when the actual income distribution was unknown. Once the evidence and theory were both progressed, the intellectual demand for a new empirical framework emerged.

Another development in the data supply changed the situation. In the early 1940s, Simon Kuznets (1942) published the U.S. national income time-series estimates, which had a significant impact on econometricians. On the one hand, the practical issues of budgetary MPC were no longer a concern once Kuznet's long-run statistics were able to directly obtain the aggregate MPC. This fact is verified in Thomas's (1989) survey of the boom in the 1940s of empirical studies on the aggregate MPC, most of which used the Kuznet series. On the other hand, national income estimates made demand analysts reconsider their methods and the new possibilities of their applications.[65] Against this background, econometricians had gradually abandoned the idea of solely applying budget data to derive demand equations. Instead, they turned to a synthetic framework of budget and market data.

Prior to the appearance of long-run national income data, Marschak (1939a) had proposed a theoretical framework that could arguably combine the micro- and macro-level information in a demand analysis. Assuming the linear family budget function is defined by the household expenditure x and its income r,

$$x = \alpha + \beta r$$

where α and β could be estimated linearly by the price p under a fixed period:

$$\alpha = a + kp$$

$$\beta = b + lp$$

The family budget function was rewritten as,

[65] In the United Kingdom, Richard Stone (1945) produced the first econometric analysis of market demand using his national accounting estimates with James Meade. See the discussion in Chapter III.

$$x = a + kp + br + lp + lpr$$

Therefore, the national consumption (X) could be integrated through the function of income distribution $f(r)$,

$$X = \int_0^\infty (a + kp + br + lp + lpr) f(r) dr$$

Since $\int_0^\infty f(r)dr = 1$, the national income per family was defined as $R = \int_0^\infty rf(r)dr$. Plugging in the distribution assumption into X will get,

$$X = a + kp + bR + lp + lpR$$

The equation above aggregated the budget equation from a household to a national level in a certain time. After obtaining the 'hypothetical' national consumption X under each period, the market demand X_m (taken from the national time series) could be fitted by a function of X_t and market price P_t under time *t*,

$$X_m = g(X_t, P_t)$$

Under such a framework, Marschak (1939a) suggested that it would be possible to derive the market demand function if the budget and price data, aggregated income, and its distribution were accessible. This agenda signalled that Marschak had abandoned the idea of transforming the income-expenditure curve into the price-quantity curve; instead, he treated the former as a part of the latter.

Marschak (1939b) further defined the x as 'personal' budget functions and X as 'collective' budget functions that referred to the household and national consumption, respectively. However, Marschak's terms are slightly different to the household-aggregate dichotomy used in this chapter. On the one hand, despite his primary usage of 'personal', he also accepted 'family' and 'individual' as synonyms for 'person'. Thus, this chapter selected the term household to distinguish between the household-behavioural and individual-behavioural data. On the other hand, as Marschak noted, while 'collective' might also be called 'national', the former was a more general term 'to cover cases where the aggregate income and consumption are not those of the whole nation but only of a section of it' (161n). In this sense, this chapter chose the term aggregate since it was the most general concept, which involved collective, national, and market.

Another paper alongside Marschak (1939b) was De Wolff (1941), which focused on the empirical issue of income distribution. De Wolff (1941, 140) first distinguished micro income elasticity, which demonstrated the relationship between income and expenditure – 'for a single person or family' – from macro income elasticity – 'for a large group of persons or families'.

He derived the theoretical properties of the income elasticity of demand under the micro and macro perspectives and argued that, through a known income distribution, macro income elasticity could be determined by the micro elasticity. After examining his theory through Pareto's distribution and Staehle's (1937) study, De Wolff (1941, 144) concluded, 'As a general theory of income distribution is still lacking, there are no theoretical grounds to prefer a particular definition, and it seems hardly possible to avoid ambiguity'.

In the early 1940s, the availability of national income data finally made Marschak's (1939b) framework practicable, and Marschak (1943) elaborated its empirical recipe as the 'pooling' method. The pooling method was based on the methodological premise that the expenditure variations were modelled with income and price variations; the former were captured through the interpolation of budget materials, and the latter could therefore be estimated by the residuals. In this sense, a combination of the U.S. Survey of Consumer Purchases study in 1935–6 and the national income time series in 1920–40 was taken as an illustrative example. The method first used the 1935–6 budget surveys to construct the collective income variations as the 'aggregated hypothetical demand', weighted according to the income distribution and interpolations from the national economic time series. Next, the time series of 'absolute excess consumption' (Marschak 1943, 43) was derived by computing the difference between the aggregate demand (from the time series) and the hypothetical demand. Lastly, the variations between various price factors were explained, such as the price of the commodity, the price of its substitutes or complements, and the price index of the whole commodity basket, to derive the price and income elasticities. The entire procedure was applied to measure the price and income elasticities of meat between 1920 and 1940. The results were used to predict the level of meat expenditures in 1941.

The pooling method was the first methodological recipe that synthesised the economic data at different levels in demand studies. In the 1930s, use of household- and market-level data were considered two distinctive practices. For instance, Henry Schultz (1938) spent two chapters reviewing the derivations of demand curves from family budgets and time series, respectively, and he saw some works on budgetary demands as 'attacks' (64) on the market demand analysis. Schultz's interpretation indicated that time-series demand analysis was competing with its cross-sectional alternative during the 1930s. This macro/market-micro distinction is also shown in the exemplars of Table 1. Before Marschak (1943), there was not

an empirical framework combining both levels of evidence to measure the demand elasticities of commodities.[66]

The pooling method concluded Marschak's journey of budgetary demand analysis from his *Spiegelbild* method (1931). Intellectual progress in the theory and evidence of household income since the 1930s led him to disaggregate the total demand effect into price and income components. The new national income time series eventually allowed him to estimate variations at the national level. He did not have to rely on unrealistic idealisations to translate the income-expenditure curve to the demand curve as in his 1931 study. To this end, budget materials played a crucial role in making market demand estimates more credible. Marschak confidently asserted that the method solved the controversy between micro- and macro/market-level data:

> 'Pooling' is the answer to the discussion as to whether 'budget' or 'market' methods of demand analysis are preferable … with the material now at hand, we can venture the statement that a reduction of money incomes (especially those of non-farmers) is likely to have a somewhat stronger effect on the real expenditure on meat than would a proportionate rise in the price of meat, accompanied by a proportionate rise in the price of other foods and/or in other living costs. The use of budget data for one year in addition to market time series does improve the reliability of a demand forecast and, to a lesser degree, of the estimated elasticities. (Marschak, 1943, 48)

Marschak's synthetic framework inspired several practices in the demand analysis of commodities. Among these practices, Staehle (1945) adopted Marschak's (1939b) theoretical assumptions to analyse the U.S. meat demand by comparing the empirical results from budget materials and time-series data. However, until the late 1940s, the method was often found to be difficult to apply due to its vast data requirement. Not only did it require annual time series on the price-of-living index, commodity prices, and national income statistics, but it also needed cross-sectional data on budget surveys and income distribution. This difficulty was assessed by Stone (1945), who, in his analysis of market demand, claimed that the lack of information prevented further applications of the method.[67]

Despite its practical constraints, the pooling method was still an influential exemplar among econometricians who were eager to apply budget materials to market demand analysis. The method was eventually reapplied by Tobin (1950) in his food demand study and Stone and Rowe (1954) in their analysis of consumer behaviours,[68] especially Tobin (1950, 8n) claimed

[66] Stone and Stone (1938) used different empirical methods and data to estimate the MPC without combining the empirical evidence.
[67] See discussions in Section 3 of Chapter III.
[68] See discussions in Section 6 of Chapter III.

that his study 'owes much' to Marschak (1943) and Staehle (1945).[69] Afterwards, the idea of the pooling method was introduced by Wold and Jureen (1951) and Klein (1953), but only limited practices were produced between 1950 and 1970. When Chetty (1968) reviewed the method, he still used Tobin's 1950 paper as its illustrative example.

7. Concluding Remarks

It was during the interwar period that econometricians started to experiment with the various uses of family budget data. These attempts at interwar microeconometrics were a series of trial and error during the discipline's initial stages of development. Among these studies, Bowley's analysis of family expenditure offered a classic econometric exercise of the Engel curve by analysing the variations, which is identical to the error term in modern econometrics. Although different practices boomed in the 1930s, the epistemic boundary between research topics was not always clear. While the aspect of information and computing power was limited, microeconometricians utilised their income-expenditure estimates to approach the empirical price elasticity and the national MPC. This fact demonstrates that interwar histories of the Engel curve are embedded in other accounts of demand macroanalysis and the Keynesian consumption function. However, the consequences of these two areas of research are rather different. Marschak's pooling method indicated that the microdata could only be used as a complement to time series in demand studies, and the budgetary consumption was substituted by the outpouring of time series in the 1940s.

[69] As argued by Dimand (2011, 167–8), Tobin was influenced by Staehle's course on statistical demand analysis when Staehle was a visiting scholar at Harvard.

Chapter III. Richard Stone's 'Cambridge Crew' and the Consolidation of Postwar Microeconometrics

1. Introduction

This chapter explores how Richard Stone directed his 'Cambridge Crew' of econometricians at the Department of Applied Economics (DAE) and initiated the first collective effort of post-war microeconometrics during the early 1950s. This group of econometricians mainly used new data from household budget surveys to obtain empirical knowledge through advanced computing power. By examining Stone's work and econometric collaborations with DAE affiliates, the chapter shows that the history of modern microeconometrics is best understood by starting with DAE's teamwork on econometrics under Stone's directorship.

Many scholars have studied Stone and the history of DAE. As a Nobel laureate, Stone's intellectual journey is well-documented – for instance, Johansen (1985), Deaton (1993; 2008b), Pesaran and Harcourt (2000), and Barker (2017) have provided general accounts; Gilbert (1991) has looked at his demand analysis; Comim (2001) has studied his idea of national accounting; and Marangoni and Rossignoli (2016) have examined his input-output models. Historians of econometrics have also studied the contributions of the DAE. For instance, Epstein (1987, 141–52) has studied its time-series econometrics, and Smith (1998) has examined its econometric methods. However, the literature has not paid enough attention to Stone's and the DAE's contributions to the microeconometrics of household behaviours.

Stone's econometrics can be dated back to his empirical curiosity in economic theory and its measurement, inspired by his teacher Colin Clark. He was concerned with two practical issues throughout his career: the construction of a reliable dataset and its statistical applications to supplement economic theories. During the interwar period, Stone finished an empirical study on the Keynesian multiplier using ranges of budget and aggregate data (Stone & Stone, 1938). Later, he was involved in creating the first standardised national accounting estimate (Meade & Stone, 1944), and he then applied these time-series estimates to measuring market demand (Stone, 1945). These works led him to become the first director of the DAE in 1946. During

his directorship in 1946–55, Stone engaged with multiple projects and DAE affiliates to explore econometric problems of time series and family budget data. The former led to two crucial instruments in time-series econometrics: the Cochrane-Orcutt transformation procedure and the Durbin-Watson test (Cochrane & Orcutt, 1949; Durbin & Watson, 1950). For the latter, the focus of this chapter, the final products were three departmental monographs, published in the mid-1950s, that contributed to the microeconometrics of demand analysis (Stone & Rowe, 1954), the Engel curve (Prais & Houthakker, 1955), and income distribution (Aitchison & Brown, 1957). At the DAE, Stone's position was analogous to the captain of the rowing club: he was managing several boats by allocating his crew members to different boats for the purpose of advancing econometrics. Those microeconometric contributions were not only in line with Stone's empirical spirit but also consequences of the effective cooperation between DAE econometricians under his guidance and coordination.

2. Stone's Early Years

John Richard Nicholas Stone (1913–1991) was born to an upper-middle-class family of London, the only child of Gilbert Stone and Elsie Lawton Scott.[70] Stone's early passion in childhood was building wooden models of trains and boats, but when he attended Westminster School from 1926 to 1930, his father 'destined' him for studying law (NobelPrize.org, 1984). After spending a year in India with his family, Stone followed his father by attending Caius College at the University of Cambridge to pursue a law degree in 1931. However, two years of studying law proved untenable. Instead of law books, what interested Stone were social sciences, including the works of Irving Fisher, Karl Marx, Sigmund Freud, and Vladimir Lenin. During the 1932–3 academic year, despite passing part one in the Law Tripos, Stone was determined to switch to economics. Unsurprisingly, what brought him to economics was the Great Depression.[71] As he recalled,

> At that time the world was in the depth of the great depression and my motive for wanting to change subject was the belief, bred of youthful ignorance and optimism, that if only economics were better understood, the world would be a better place. (NobelPrize.org, 1984)

[70] Sir Gilbert Stone (1886–1967, knighted in 1936) studied law at the University of Cambridge and became a barrister in 1911. His political career ended after losing general elections representing the National Liberal Party in 1922 and the Liberal Party in 1923. Between 1930 and 1943, he stayed in India and served as Judge of High Court in Madras (1930–35) and Chief Justice in Nagpur (1936–43).

[71] The Great Depression also brought Jan Tinbergen to economics and Keynes to his general theory.

The following academic year, Stone's journey of economics started with Richard Kahn, who was his first supervisor at King's College,[72] and two assigned readings over the holidays: Alfred Marshall's *Principle of Economics* and Joan Robinson's *Economics of Imperfect Competition*. During his undergraduate training, Stone identified that Kahn and Robinson were 'the two best on the theoretical side' (Pesaran, 1991, 88). He also encountered Keynes, who had invited him to speak at the Political Economy Club on the subject of 'effective demand versus production frictions', but at that point, they were only acquaintances (Pesaran, 1991, 89; Deaton 1993, 477). The closest Cambridge economist to Stone was Colin Clark, who was his teacher in statistics. Clark, whom Stone credited as 'the greatest influence on me' (Pesaran, 1991, 87–88), was spearheading the first attempt to estimate the UK national income and expenditure (Clark, 1932). Attracted by Clark's work, Stone gradually located his interest in economics to applying quantitative facts when addressing empirically ill-founded Marshallian economic theories:

> My interest in economics was from the beginning in its applications … the economics I was taught was insufficiently quantitative and that theory and facts were too widely separated … the development of a science requires attention to both facts and theories and I agree with Marshall that economic theory is as mischievous an imposter when it claims to be economics proper as is mere crude unanalysed history … The real difficulty is to combine the two so that theory can be used to interpret facts and facts can show what has to be interpreted. (Pesaran, 1991, 89)

Unlike other economists of the 'Cambridge Circus',[73] Stone's empirical mind was more sympathetic to econometricians, a new label that a group of statistical economists had started adopting since the 1930s following the foundation of the Econometric Society. Unlike some of his contemporaries, Stone was particularly concerned with applying real data to economic theories.[74] In his last undergraduate year, Stone spent a summer conducting his first econometric exercise at home using a Monroe desktop calculator.[75] He estimated the Cobb-Douglas function by fitting British time-series indexes of output, labour, and capital with time trends. However, such laborious calculations and exciting results did not impress Pigou, who had shown respectful but little interest.[76] In 1935, Stone obtained a double first BA degree in

[72] Stone was sent there while 'there was no economist at Caius' (Pesaran, 1991, 88).
[73] The Cambridge Circus was an affinity group of Keynes at Cambridge during the 1930s. Members of the group included Richard Kahn, Austin and Joan Robinson, James Meade, and Piero Sraffa (Black 2013). Meade aside, other members of the Circus worked more on economic theories rather than econometric studies.
[74] This attitude was slightly different to Clark, whose work was 'a lack of theory' (Deaton, 1993, 477).
[75] It is not clear in which year Stone did it, as he recalled it was in 1934 or 1935. The Monroe calculator was his twenty-first birthday (which should be August 30, 1934) present from his parents.
[76] In the 1930s, index numbers of British manufacturing had been published in the *Board of Trade Journal*, but a regression of three logarithm variables still contained unquestionably burdensome calculations even using a

law and economics.[77] However, he was not confident enough to do a graduate degree since he had only studied economics for two years. After his father managed to find him a job at the C. E. Heath and Co., Lloyd's firm of insurance brokers, he moved to London in 1936 and became a businessperson in the city. In the same year, Stone married his fellow economics peer, Winifred Mary Jenkins.[78]

Soon after, Stone published his first co-authored paper in *Econometrica*, titled 'A Study of Costs' (Tweddle & Stone, 1936). The study explored the British empirical labour-cost function, that is, the statistical relationships between aggregated output per-capita index (as an explained variable) and employment and production indices (as explanatory variables, respectively) between 1928 and 1933. Tweddle and Stone estimated each elasticity of output with respect to employment and production for eight industries by adding a time variable. This article served as one of the earliest attempts to obtain a labour-cost curve in econometrics.[79] What drove them to conduct this study was that an empirical concern in economics needed legitimation. Just as engineers were also dealing with compact theories and imperfect data,

> The fact that the [labour-cost] curve theoretically, could change in shape through time had to be neglected. But this … was a small point inhibiting the plunge compared with the fear that the theoretical economist would have no tolerance of the results obtained from data so crude. He defines an industry very narrowly and obtains interesting results thereby. But if the statistician is ever to obtain results at all he must be content with what data there are and lump together such things as ships and electrical apparatus and call his industry engineering. Small wonder that his results are not so neat as those of the theorist. (Tweddle & Stone, 1936, 226)

Stone prompted the idea that economics was analogous to other sciences that required empirical content to progress. This attitude was the earliest evidence of Stone's position toward the relationship between economic theories and econometrics and could be consistently found in his published works in the 1930s.

Between 1936 and 1939, working in the City, Stone spent most of his spare time conducting economic research, ranging from general studies to academic writings. In June 1937, he took over Colin Clark's position as the managing editor of the *Trends*, a supplemental series in the

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desktop calculator. As a result, twenty years before Robert Solow, Stone had found 0.75 for the labour share, 0.25 for the capital share, and the 2.25% per year for the residual (technological progress). On such finding Pigou's reaction was 'doubtless it is all very interesting but still I don't understand' (Pesaran, 1991, 89).

[77] His examiners of the economics Tripos were Pigou, C. W. Guillebaud, Austin Robinson, and John Hicks. Only five students were awarded first, including David Champernowne and David Bensusan-Butt (Pesaran, and Harcourt 2000, F147).

[78] Winifred Mary Jenkins (1913–2000) went to Newnham College (BA in Economics, 1935). She was one of the earliest co-authors of Stone (as W. M. Stone). After they divorced in 1940, she married Sir Patrick George Hamilton, 2nd Baronet of Ilford, Essex, the only son of Conservative Party politician, Sir George Clement Hamilton.

[79] Another one was a study by Joel Dean (1936). See Schumpeter (1954, 962).

monthly *Industry Illustrated*. Until he resigned in May 1939, the Stones produced monthly reviews on the European economies from Clark's compilation of economic time-series statistics, such as output, consumption, investment, foreign trade, and prices (Stone, 1988, 21). Those economic writings for general audiences contributed to Stone's rising reputation during this period (Baranzini & Marangoni, 2015, 3). He once commented the *Trends* would be a 'modest forerunner of the British official monthly *Economic Trends* which began to appear after the war' (Pesaran, 1991, 90).

Stone also published two academic journal articles with his wife. Their first article estimated Keynesian MPC and calculated the multiplier through three different methods: the 'budget method', the 'historical method', and 'Mr. Kahn's method of leakages' (Stone & Stone, 1938). First, the 'budget method' applied three household cross-sectional surveys of the US (1929), Germany (1926–7), and Japan (1925–6) into a logarithm or log-squared regression.[80] Second, the 'historical method' followed a simple Keynesian framework that the aggregate consumption was a function of the value of the gross national income and time trends. They then estimated nine consumption functions from the time-series statistics of seven countries. Lastly, Kahn's method argued that the propensity to consume was defined as the total increase in 'home' income relative to the original expenditure in a country that excluded savings and foreign-good consumptions. Four aggregated datasets from Australia, Denmark, Great Britain, and Queensland (Australia) were applied for the calculations.

The purpose of this study was to offer an empirical buttress to Keynes's 'general psychological law',[81] which the Stones felt needed more empirical content rather than theoretical arguments:

> We do not need a learned article to tell us that consumption for the whole community may be affected by factors other than the community's income. What is not obvious is whether by and large Mr. Keynes' assumptions are justified and, if they are, with what reservations. (Stone & Stone, 1938, 1)

For the Stones, their evidence from multiple sources and empirical methods was 'an appeal to facts' to determine the extent to which economists should trust their theories. In other words, the empirical content should be prioritised in judging the validity of economic theories rather than the theoretical one, which offered indecisive remarks. Although this article was not the first attempt to obtain the income-expenditure relationship, it was the first empirical paper

[80] For the empirical models, see discussions in Section 5 of Chapter II.
[81] For the definition of the law, see discussions in Section 5 of Chapter II.

before 1950 that fit the budget data into a consumption function framework and constructed cross-country comparisons through a combination of multiple sources and methods.[82]

Their second article compared the industrial output indices produced by three institutions and created a new measurement for output of the manufacturing and mining sectors, adjusted according to census and employment indices (Stone & Stone, 1939).[83] This article suggested that Stone's interest from his undergraduate years in estimating the production function was still present. The main indices revised were the mining and manufacturing quarterly index (1924, 1927–38) collected by the Board of Trade and the employment figures from over the same period. Both output and employment were standardised using the census data of output and regressed with a time trend. The equations could therefore be used to predict future indices of production and employment and to obtain the sectorial labour share. Based on the Cobb-Douglas framework, they conjectured that 'the state of physical returns to labour' could be identified by including more data on the working hours and employment numbers, and their experiments on the U.S. series signalled some progress.[84] However, similar to Bowley's early econometric study,[85] the Stones realised that the evidence was too patchy to make this claim, especially in the absence of a corresponding theory. Even though these two adjusted indices comoved quite consistently, he noted, 'it is most important that no such economic meaning should be attached to this coefficient' (Stone & Stone, 1939, 484).

Apart from the two articles, the Stones reviewed (under the name of R. W. S.) 16 books that appeared in the *Journal of the Royal Statistical Society*, which covered a wide range of topics in economics and statistics.[86] They particularly admired the contemporary econometric practices of the Cowles Commission and the National Bureau of Economic Research. For instance, they regarded the Cowles monograph by Charles Roos (1937) as 'extremely satisfying to the mind' (S. 1938c, 467) and that by Alfred Cowles (1938) as 'of the highest value to whose engaged in the field of econometric research' (S. 1938a, 769). They also praised Kuznets (1937) as 'impossible to withhold the most whole-hearted admiration' (S. 1938d, 624). Those reviews

[82] See a survey of empirical consumption function by Thomas (1989, 146–49) and discussions in Chapter II.
[83] Stone probably collected those indices when he was writing for the *Trends*.
[84] 'Working with American data, we have found that the inclusion of hours in our employment series not only greatly increases the correlation between output and employment, trend influences eliminated, but also completely alters the partial coefficient confined to a single, reasonably homogeneous industry' (Stone & Stone, 1939, 484n). Nevertheless, no bibliographic evidence shows that any of their findings on this were published later.
[85] See discussions in Section 2 of Chapter II.
[86] Stone's archive still has some of the drafts. JRNS/1/1, Papers of John Richard Nicholas Stone, King's College Archive Centre, University of Cambridge.

showed that young Stone was rigorously engaged with the contemporary literature of econometrics.

Other than econometrics, the Stones still kept track of theoretical economics in their reviews. They held Joan Robinson's (1937) interpretation of the Keynesian theory of employment in high regard, even if in the end they eventually added,

> This does no harm provided a critical attitude is adopted to the author's estimates – in common with Mr. Keynes – of the quantitative significance of the various factors involved. Mrs. Robinson would no doubt be the first to agree that an important next step is the quantitative estimation of the significance of the various determining variables. (S., 1938b, 770)

The quote again captured Stone's early empirical curiosity in applied economics around the late 1930s. From his early works, this curiosity could be categorised into two concerns. The first was the *construction of a reliable dataset* from his revision of the official production and employment indices, and the second was the *statistical application of the dataset to supplement economic theories* from his econometric estimations of the labour-cost function and the Keynesian multiplier. As the next section will show, these two concerns would persist not only throughout the interwar period but Stone's entire academic career.

3. Towards a Macroanalysis of Market Demand

Becoming a government statistician in 1939 was a turning point in Stone's career. He stayed in London until the war ended, working on his interest in applied economics. He and James Meade developed the first British national accounts that initiated Stone's Nobel-winning contributions to economic statistics. In addition, with the new aggregate-level estimates, he produced a macroeconometric analysis of market demand for a selective set of commodities (Stone, 1945). Despite the unavailability of contemporary budget data being, he did not give up the idea from Marschak's pooling method to incorporate microdata into his market demand estimates.

In mid-1939, stepping down from the managership of the *Trends*, Stone finally persuaded his father that he 'was not and never should have become a businessman' (Pesaran, 1991, 90). Some solid publication records helped him locate another job. Following the outbreak of the war in September, at the request of Noel Hall,[87] Stone joined the economic staff of the Ministry

[87] Noel Frederick Hall (1902–1983) was an English economist. He was Professor at the University College London (1927–38) and the founding director of National Institute of Economic and Social Research (1938–43). During the war, he was director of Neutral Countries Intelligence at the Ministry of Economic Warfare.

of Economic Warfare as a statistician to keep the records of shipping imports of neutral countries.[88] In the summer of 1940, referred by Austin Robinson, Stone joined the Central Economic Information Service at the Offices of the War Cabinet, headed by Lionel Robbins. Stone worked as an assistant in statistical work for James Meade,[89] who was trying to obtain national accounting statistics to tackle the problems of war finance.[90] Stone and Meade developed a strong friendship. At the end of 1940, they produced three accounts of 1938 and 1940, including the estimates of national income and expenditure; personal income, expenditure, and saving; and government expenditure, tax, and country's net investment.

These estimates immediately interested Keynes at the Treasury, who had expressed his concerns about the inadequacy of national accounting statistics in *How to Pay for the War* (Keynes, 1940).[91] Under Keynes's keen 'selling' (Stone in Patinkin, 1976, 1115n), those numbers were circulated as a part of White Paper on Budget Day of 1941 (BPP, Cmd 6261). An article describing their methodology was subsequently published in the *Economic Journal*, edited by Keynes (Meade & Stone, 1941). After the Economic Information Service was split into the Economic Section and the Central Statistical Service, Stone was sent to the latter, and from there, Keynes hired him as his assistant to continue the work on national accounting.[92] Until 1945, Stone met Keynes frequently and continued to produce the national income statistics for the years of 1938–44. The results, along with a set of U.S. estimates, were published in Meade and Stone (1944).

Meanwhile, working on the British national accounts, Stone continued working on his second interest in econometrics. In 1941, Stone married Feodora Leontinoff, Secretary at the National Institute of Economic and Social Research (NIESR).[93] Founded earlier in 1938, the institute faced a crisis after the outbreak of the war in September 1939. The bulk of its staff members, including Director Noel Hall, were reallocated to Whitehall, the majority to the Ministry of Economic Warfare. Feodora Leontinoff became the 'caretaker' of the wartime Institute, and she shifted the managing power to its Executive Committee while the director

[88] By tracing abnormal shipping records, Stone claimed that he had successfully predicted Italy's declaration of war. However, he was still too junior to have any intellectual impact (Pesaran, 1991, 91–2).
[89] Meade described how Stone 'joined him in his tiny room in Richmond Terrace of Whitehall, a room furnished with a single desk, on a corner of which Dick established himself with a quill pen and a Monroe hand calculator' (Deaton, 1993, 479).
[90] Stone recalled that it was under the request of the Survey of Financial and Economic Plans (Stone, 1980, 69).
[91] Cf. Stone (1980, 67–68).
[92] Stone was initially assigned the job of editing oil statistics. On this, Keynes said, 'I'll soon stop that. I shall arrange for you to be appointed my assistant and you will take your orders from me' (Stone, 1980, 70).
[93] The NIESR was founded in 1938 by Sir Josiah Stamp and initially funded by the Rockefeller Foundation to support the development of British social sciences (Jones, 1988).

was absent (Robinson, 1988, 63). Only one mile away from Whitehall, the institute managed to survive by adopting relatively flexible research policies. The primary reason for its success was its efficient communication between the officials under Leontinoff. Some affiliates, for instance, Austin Robinson, John Hicks, and Richard Stone, travelled between Whitehall and the Institute and used their daily official estimates to conduct evening research. Austin Robinson later emphasised the 'data' advantage in the early stage of the NIESR:

> … our [the NIESR affiliates] first year's work in Whitehall came two essential tools of all subsequent economic work: first, the monthly *Economic Digest*, first edited by Eli Devons and Harry Campion but in which we all had a hand; second, the national income calculations designed and carried out by James Meade and Richard Stone. Together these made possible not only the rational conduct of the war but also a permanent change in the character of all applied economic research. (Robinson, 1988, 64)

With a specific focus on applied economics, the NIESR enjoyed manpower and data sources derived from the Civil Services, and the Stones, without a doubt, were pivotal among those practices. Robinson recollected how the NIESR conducted research works:

> The planning of that research was almost done at the only time of day that Stone and I were free – over wartime rationed dinners in a subterranean Kensington restaurant, where we might or might not suffer a noisy night from Hitler's night bombers. (Robinson, 1988, 65)

In an example illustrated by Austin Robinson, inspired by the Meade-Stone national accounts, Robinson came to question whether the same method could be applied to the British colonies. After the Stones showed their interest, they hired Arthur Lewis,[94] a young Jamaican lecturer at the LSE, as one of the advisers for the project and Phyllis Deane,[95] a graduate from Glasgow University, to do the work. As Deane recalled,

> I was very fortunate in having Austin Robinson as a supervisor [of the project]. Many well-known economists were working in Whitehall for the war effort, including Austin Robinson, Richard Stone, James Meade, and Arthur Lewis … They used regularly to come over to the National Institute to have a sandwich lunch with me and advise me on my work. (Deane in Crafts, 2008, 134)

The second reason for NIESR's survival was that it provided another scholarly entity that resembled the universities at peacetime. Its research projects focused on long-run shots rather than the research in Whitehall usually expected for fast and instantly applicable outcomes (Robinson, 1988, 64). As such, academics at the Institute were given more freedom and flexibility to conduct research on public policy.

With the advantages of research and administration, Stone was put in an ideal situation conducting his econometric works with the small group there. His project on the analysis of

[94] Lewis won the Nobel prize in 1979 for his research on development economics.
[95] For the history of Deane's work in the early 1940s, see Morgan (2011).

consumer behaviour was initiated at the Institute in 1941. As he recalled, the project originated from a simple empirical curiosity in economic theories:

> As for my analysis of consumers' behaviour, I suppose you could say that my initial motivation was that of the child who takes a watch to pieces to see how it works. (Stone in Pesaran, 1991, 102)

After three years of development, the results were published under the title 'The Analysis of Market Demand' at the end of the war (Stone, 1945). Based on the national accounting data of expenditure and consumption of the UK (1920–38) and the US (1929–41), the study estimated their market demand functions for individual commodities by fitting the data into a logarithmic single equation,[96]

$$logq_i = \alpha_i + \beta_{i0} logx + \beta_{i1} logp_i + \beta_{i2} log\pi + r_i t$$

where q_i was consumption of the ith commodity, x was total expenditure, p_i was price of the ith commodity, π was the retail price index, and t was time trend. The commodities investigated included beer, spirits, tobacco, soap, and telegram consumptions in the UK; and food, tobacco, and certain durable goods consumptions in the US. This 97-page article included detailed model specifications following Henry Schultz's empirical framework of demand function (1938). The econometric analysis showed Stone's belief in the structural-form models that the market demands could present as an economic system with multiple single equations. As he wrote,

> … we are primarily interested in the structural relation of the variates, and not simply in obtaining a relationship that will enable us to forecast the dependent variate. (Stone, 1945, 296)

After showing the graphic representations of regression estimates, Ragnar Frisch's (1934) bunch-map analysis was used to select which regression model could provide the best fit.[97] What justified Stone's use of bunch-map analysis was the data availability. Since this study contained fewer than 20 annual observations, the macro-variations explained by the regressions were very limited, as he noted:

> Not only are the samples usually small on account of restrictions on the available data and the fact that large-scale empirical research designed to test hypotheses is still something of a novelty in economics, but at the same time almost nothing can be done by means of experiments to increase the variance of the series or control the variation in part of the field while a limited amount of covariation is being studied. (Stone, 1945, 299)

Thus, the bunch-map analysis acted as a 'safeguard' for the small samples:

> Bunch maps figured largely in my early works as a safeguard against the appearance of more than one relationship in my small samples. I do not know how widely they were used

[96] Identical form and notations were used in Gilbert (1991, 298).
[97] Gilbert (1991, 290–1) showed the procedure of model selection.

even in the early days. I suppose I was persuaded that they were not worth the considerable amount of work involved as I gave them up after a time. (Stone, in Pesaran, 1991, 103)

The application of time-series data in this study limited Stone's interpretation of the demand for individual commodities at the aggregate level. However, he also realised that the synthesis of both individual and aggregate information, as Marschak (1943) had suggested, could improve his market demand estimates.[98] However, the idea of utilising both levels of information could not be applied at the time, as most of the relevant budget data and income distribution were still inaccessible:

> Professor Marschak has suggested an interesting [pooling] method which involves the combination of material in the form of time series with budget data on family expenditure and the distribution of family incomes. This method cannot be applied in the present instance, since the required budget data are not available. (Stone, 1945, 288)

Stone then proceeded to argue that, once these empirical contents were progressed, the pooling method could therefore be applied to his inquiry,

> If an exact form could be given to the individual budget functions and if an exact description were available of the way in which the population and the distribution of income varied, then it would be possible to deduce the form of the market demand equation. Unfortunately we do not possess this knowledge. (291)

4. The Department of Applied Economics at Cambridge

The war was a productive period for Stone. He established himself as a national accountant as well as a macroeconometrician. Both areas of expertise were consistent with his early research ideas about constructing a reliable dataset and its statistical application to existing economic theories. Thanks to the advancement of Keynesian macroeconomic models and the Stone-Meade national accounting framework,[99] such an idea of applied economics became prevalent in the 1940s. Keynes played a crucial role in advancing these theories and promoting the relevant practices. He not only championed the importance of applying economic statistics to making policy decisions in Keynes (1940), but back to 1938, he realised that those applications deserved a focused research institution. In a letter to Colin Clark, Keynes urged Clark to return to England from Australia for a new 'statistical realistic department':

> Come back here [Cambridge] in the first instance anyhow. You will be able to get back to Australia at any subsequent moment you may choose. The problem of doing anything here might be more difficult—indeed it is—but it may be more important. It is very necessary to

[98] Marschak's method is explored in Section 6, Chapter II.

[99] As argued by De Vroey (2000), the evolution from Keynes's general theory to the Keynesian IS-LM model contained two stages of development. The first was John Hicks's (1937) general IS-LM framework and then Franco Modigliani's (1944) revision of Hicks's model.

> lay the foundations for a proper department of statistical realistic economics at Cambridge.[100]

At the time, the idea of this new department was still up in the air. Keynes never offered a clear definition of the department, and he even once used another phrase, 'Department of Realistic Economics'.[101] Such uncertainty when defining the institutional aims was recalled by Austin Robinson, who was in close contact with Keynes about the foundation of the department:

> When the National Institute [NIESR] was born a little belatedly into this family of struggling infants … neither Maynard Keynes nor I, concerned with planning and negotiating what ultimately became the Cambridge DAE, knew the answer in those early days. (Robinson, 1988, 63)

Although everything seemed uncertain, the only sure thing was, with a huge admiration for Clark's work,[102] Keynes would love an empirical minded person to lead or at least serve essential tasks for the new department. Eventually, in November 1939, the Faculty of Economics and Politics at the University of Cambridge approved Keynes's proposal and named the new institution the 'Department of Applied Economics'. However, the proposal was delayed once most of the Cambridge economists were sent to London during the war.

Once the war reached the end, the proposal was finally put in action. In 1944, the DAE's Committee of Management made Stone an offer as the first acting director in May 1945.[103] The DAE acquired funding from the Rockefeller Foundation, the Nuffield Foundation, and the NIESR and would be officially launched in April 1946. Initial employments of the DAE included one director, five Nuffield research fellows, three senior research workers, five junior research workers, and seven computing and administrative staff (DAE, 1948, 25–6).

As director, Stone's guideline from the Departmental Committee was 'extremely liberal and congenial', which enabled him to 'set up an econometric program which would embrace work on facts, work on theories, and work on econometric and statistical methods needed to analyze the facts in the light of the theories' (Pesaran, 1991, 99–100). In his proposal submitted to the Nuffield Foundation, he addressed the three research aims of the DAE:

> The Department will concentrate simultaneously on the work of observations, i.e. the discovery and preparation of data; the theoretical appraisal of problems, i.e. the framing of hypotheses in a form suitable for quantitative testing; and the development of statistical methods appropriate to the special problems of economic information. The special character of the Department's approach to the problems of real world [sic] will lie in this attempt at systematic synthesis. (Stone, cf. Pesaran and Harcourt, 2000, F149–F150)

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[100] Letter from Keynes to Clark, 5 March 1938, in Keynes (1978, 800–1).
[101] Letter from Keynes to Geoffrey Crowther, 28 March 1940, in Keynes (1978, 813).
[102] See Maddison (2004, 11).
[103] Members included Keynes (Chair), David Champernowne, Austin and Joan Robinson, Gerald Shove, Piero Sraffa, and Dennis Robertson (Pesaran, 1991, 97).

These goals called for improved data, methods, and empirical estimates. In the DAE's first report, Stone categorised the three research goals for the department: national and social accounting, time-series econometrics, and applied econometrics.[104] First, as Stone realised, 'the most pressing problems of applied economics today turn on the inadequacy of suitable data and methods of analysis' (DAE, 1948, 4). Hence, the department's priority was collecting and producing raw data, that is, British national accounting and other statistics. Second, to turn newly acquired or existing data into a 'more efficient use' (DAE, 1948, 5) required hiring a group of econometricians to provide relevant statistical analyses of annual economic time series. The first group of econometricians included Roy C. Geary, Guy H. Orcutt, Gerald Tintner, James Durbin, Geoffrey Watson, and graduate students Stanley F. James and Donald Cochrane.[105] Third, with statistical methods, applied econometrics used aggregated and disaggregated data to either test economic hypotheses or evaluate the impact of economic variables. During 1946–48, the group of applied econometricians included Gerhard Stuvel and Alan R. Prest, who worked on the demand analysis, and Orcutt and Andrew D. Roy, who worked on the estimation of the consumption function. Those arrangements were continuations of Stone's previous works: the former responded to Stone's (1945) earlier study of demand analysis and the latter to Stone and Stone's (1938) multinational estimations of the MPC.

During its first three years, the department mainly concentrated on the national accounting and time-series econometrics; applied econometrics at the DAE occupied very few research activities and showed limited contributions to microeconometrics. Prest's experiments with different sets of equations on the demand of commodities were based on new-coded national accounting macro-level data between 1870 and 1914 (Prest, 1948; 1949). The only microeconometric study during the time was conducted by Gerhard Stuvel,[106] whose project investigated the impact of household incomes on food expenditures in Holland by using the 1935–6 family budget survey of 598 Dutch households.[107] While Stuvel did not complete the study during his one-year stay, the task was finished around 1949 with the aid of Stanley James and published a year later (Stuvel & James, 1950). The empirical model assumed that the total

[104] This categorisation was condensed from DAE (1948), which classified three groups of the research activities: (a) National Income, Product and Social Accounting Projects, (b) Statistical Methods in Economics, and (c) Verification and Estimation of Economics Relationships.
[105] As Durbin recalled, 'Although he [Cochrane] was not a member of the department's staff, he was very often in there working with Orcutt, using the library, turning up at seminars and so on. So he was almost a part of the team' (Durbin in Phillips, 1988, 129–30).
[106] Stuvel was Jan Tinbergen's pupil and a staff member of the Netherlands Central Planning Bureau. By then, he was a short-time visitor of the DAE during 1946–47.
[107] The survey was collected by the Netherlands Central Statistical Bureau.

food expenditure y was determined by the total disposable income x_1 and family size x_2 in two forms:

$$y = constant + \alpha_1 x_1 + \alpha_2 x_2$$

$$\log y = constant + \beta_1 \log x_1 + \beta_2 \log x_2$$

The estimates in both equations were calculated through detailed tables of sums of squares and cross-products. They subsequently performed an analysis of covariance following the F-test and investigated between-group differences according to districts and occupations. The results showed that the income elasticity of food expenditure (α_1 and β_1) for low-income households (from 0.246 to 0.582) showed greater variability than middle-income households, excluding farmers (from 0.231 to 0.299). In contrast, the family size elasticity (α_2 and β_2) of middle-income households, excluding farmers, ranged from 0.318 to 0.772, and the low-income ranged from 0.546 to 0.680.

The unavailability of contemporary UK household budget surveys limited the development of microeconometric practices at the DAE. As the analysis by Stuvel and James was 'made for a comparison of demand analysis from time series and budget material with reference to the United States' (DAE, 1948, 16), there were no suitable U. K. family budget surveys available between 1946 and 1948. Although there were some budget surveys before 1920,[108] such microdata seemed too early to be applied to the DAE's relatively recent scope of research agenda.

The DAE's early attempts to measure consumption function proposed only the 'historical method' from Stone and Stone (1938), which plugged annual time series into a simple regression framework. However, no empirical work was published. In the summer of 1948, James Duesenberry visited the department for two months and worked with Orcutt on estimating the U.S. aggregate consumption function by using new annual data on national income statistics. Another study proposed by Orcutt and Andrew Roy applied monthly data from 1935 to 1941 to investigate short-term changes in U.S. consumption. Both projects seemed to stall out while Roy shifted his attention to Prest's work on U.K. national accounting, and Orcutt primarily focused on the statistical properties of annual economic series with Cochrane. Apart from an unpublished mimeograph on a bibliography of the consumption function (Orcutt & Roy, 1949), no evidence showed that Orcutt's and Roy's projects were

[108] For instance, the 1904 working-class expenditure survey by the Board of Trade and the 1918 food expenditure survey by the U.K. Working Class Cost of Living Committee (Staehle, 1935; Gazeley & Newell, 2013).

completed. After Orcutt left for the US in 1948, empirical research on the consumption function disappeared from the DAE's research reports.

Although the empirical works at the DAE did not see a vast outflow of publications, empirical instruments in solving the autocorrelation problem were progressed by time-series econometricians during the first three years. Returning to his earlier demand study, Stone (1945, 333) noticed the problem of serial correlation, that is, when 'the extent to which observations separated by different time periods are correlated'. However, back then, a valid solution remained unknown. Afterwards, two instruments were developed by the DAE members in the late 1940s to tackle this concern. The first was the statistical test of serial correlation in the least-square regressions that was later named the Durbin-Watson test (Durbin & Watson, 1950; 1951), from which they derived the distribution theory of regression residuals and its statistical bounds under the situation that the time series were generated from an autoregressive process. The second was the Cochrane-Orcutt autoregressive transformation, which adjusted the serial-correlated time series with first differences (Cochrane & Orcutt, 1949; Orcutt & Cochrane, 1949).[109] As Stone was 'very highly' interested in applying both (Pesaran, 1991, 101), these two instruments reshaped his view on time-series econometrics such that he came to question the limits of aggregate data as well as the Haavelmo-Cowles programme. He once commented on these practical influences on his monograph of demand analysis (Stone & Rowe, 1954), which will be discussed later in section 6, writing,

> The statistical analysis [of Stone & Rowe, 1954] owed much to my colleagues and particularly to Durbin and Orcutt. It is perhaps surprising that I did not discuss Haavelmo's simultaneous equation system. In principle, I fully agreed with it but in practice I thought that, with the many other difficulties in time series regression analysis, this one could perhaps be left over for the time being. (Pesaran, 1991, 103)

5. Building Materials for Microeconometrics

Around the late 1940s, the DAE had established its reputation in econometrics due to the popularisation of its empirical tools for analysing time series. Likewise, for Stone, the practical problem of autocorrelation in time series was seemingly solved by the Durbin-Watson diagnosis and the Cochrane-Orcutt treatment. Thus, the DAE began to shift its central research toward the applications of household microdata.

[109] For the formal procedure, see Section 2, Chapter IV.

Since 1949, a new group of econometricians arrived at the DAE and started to work on the econometric analysis of household behaviour. This group contained short-term visitors and long-term fellows, including James Tobin (active 1949–50), Hendrik S. Houthakker (1949–52), Michael J. Farrell (1949–75), Sigbert J. Prais (1950–57), J. Alan C. Brown (1952–65), and J. Aitchison (1952–56). Among the group, Tobin and Houthakker were early-stage academics. Tobin was a Junior Harvard Research Fellow who had just defended his doctoral dissertation and almost finished a food demand study (Tobin, 1950) before coming to the UK.[110] Houthakker, a doctorandus from Amsterdam, joined as a research officer. Before that, he had already published a paper on price elasticity in the Dutch electricity sector in the Netherlands (Houthakker, 1949). Farrell and Prais started their graduate degrees under Stone, and both were hired as research assistants. Farrell's project investigated the demand for durable goods, and Prais began to work with Houthakker on estimations of the Engel curve.[111] Brown was a staff member who had experience with food budget surveys in the statistical division of the Ministry of Agriculture, Fisheries, and Food (Stone, 1985, 191). Aitchison was a Cambridge graduate who had just started his academic career as a statistician.

Unlike the earlier years, the emergence of new materials provided Stone sufficient reason to encourage his staff members to conduct econometric investigations of microdata. Those materials encompassed (1) the outflow of official budget data, (2) the construction of sampling surveys, and (3) the improvement of computational efficiency.

First, since the late 1940s, multiple official budget surveys in the US and UK were made available over this period. All of these micro surveys became the novel inputs for demand analysis at the time. Some examples included the 1937–8 working-class household expenditure survey conducted by the Ministry of Labour (10,762 families) and the 1938–9 middle-class survey by the Civil Service Statistical Bureau (1,361 families), both of which were interwar surveys first coded and released for academic purposes in 1947. Two household surveys by the UK Ministry of Food were conducted in 1950 (1,143 families) and 1951 (6,000 families). Similar developments were also seen across the Atlantic. The 1941–2 wartime expenditure

[110] Tobin's (1947) dissertation studied the U.S. consumption function through the combination of annual economic time series and family budget surveys. For his early empirical work, see Tobin's (1997) recollection and Dimand (2011).

[111] Farrell and Prais's focus on demand analysis might be a consequence of Stone's direction, as recalled by Wilfred Beckerman, 'Pretty well everybody there under his [Stone] control had to work on one or other of two things. Either you worked on national accounting, which was where my measuring the capital stock fitted in his grand scheme; or you worked on demand analysis. As I had given up the capital stock topic I became a demand analyst' (Beckerman in Tribe, 2002, 164).

survey (3,060 families) by the U.S. BLS was finalised and published in 1945. The first SCF (3,058 spending units) by the University of Michigan was also launched in 1946.

Second, given that collecting national and social accounting data still occupied a significant part of the DAE's research activities, Stone initiated a new project on the social accounts of Cambridgeshire. The idea of the project was to 'try out the feasibility of collecting economic information about transactions on a designed basis' (DAE, 1951, 13). Properly sampled households could be used as a reliable unit when aggregating regional accounting estimates. In early 1949, J. E. G. Utting was hired to oversee the project assisted by James Durbin, a supervisee of Stone who joined as a departmental staff in 1948. They soon finished an article describing the research idea, in which they argued that such social accounts could be used to construct the Leontief-style input-output tables (Stone, Utting, & Durbin, 1950). In 1953, they launched a new sample survey of Cambridgeshire, covering around 4,000 addresses after three pilot studies (DAE, 1954, 9). As recalled by Durbin, Stone played a crucial role in determining this development:

> He [Stone] was rather excited at that time with the idea of using sample surveys for collecting economic data. He suggested that, as part of the project that I had to do in my applied field for the diploma, I might do some work on sampling business enterprises. That was the origin of my interest in sample surveys. (Durbin in Phillips, 1988, 128)

The Cambridge survey was one of the earliest examples in the UK that collected income-saving data rather than expenditure. This area of concern was similar to the concurrent attempt at Michigan's Survey Research Center and the one from slightly later at the Oxford Institute of Statistics in 1952.[112] Although empirical results from the Cambridge survey were not widely applied and published, Stone's regional sampling still attracted attention from the professional community. His reputation was demonstrated in an invitation letter from Lawrence Klein, who was then at Michigan, to Stone:[113]

> Various persons at the Survey Research Center would be extremely anxious to talk with you, especially to learn about your experience with sampling surveys in Cambridge … you may want to learn at first hand about some of the survey techniques used here.

Finally, the introduction of computer-based calculation around the late 1940s was pivotal in facilitating the development of microeconometrics at Cambridge. In 1949, the University of Cambridge Mathematical Laboratory put the Electronic Delay Storage Automatic Calculator (EDSAC) into operation and ran its first punch-card programme to compute a table for squared

[112] Reported in Lydall (1951), the pilot study of the Oxford Saving Survey covering around 600 households was conducted in 1951. Afterwards, there were other five Oxford income-saving surveys between 1951 and 1960 (Hill, 1960).

[113] Letter from Klein to Stone, 29 October 1952, JRNS/3/1/77, Papers of John Richard Nicholas Stone, King's College Archive Centre, University of Cambridge.

numbers (Ahmed, 2013, 49–53). The DAE's econometricians were the first group of scientists to use the machine to perform matrix calculations. As Lucy J. Slater, the main programmer of the EDSAC, recalled,

> Economists were among the earliest users of the programmable electronic computer, when it was made available for general use in the early 1950s. (Slater, 2004, 119)

Among DAE's applied econometricians, Houthakker, Prais, Brown and Farrell were typically interested in computer programming. The first three wrote the first econometric program in the UK to calculate sum of squares and cross-products (Brown, Houthakker, and Prais 1953). Farrell later on, together with Slater, produced a general-purpose program to do regression analysis that 'quickly became very popular among other research units in the University' (Slater, 2004, 121).

The EDSAC offered substantial computational efficiencies for Cambridge econometricians. Before the new computer was applied, the DAE's computation relied on a 'regression analyser' (Orcutt, 1948a) and computing staffs who used desk calculators. The former was the analogue machine invented by Orcutt to calculate regression coefficients,[114] and the latter, as Durbin described, the DAE's early human computors:

> … one of the assets of the DAE was that we had a room there with perhaps eight or ten young ladies operating desk calculators, supervised by an older lady of forbidding demeanor. They did the computing. (Durbin in Phillips, 1988, 131)

EDSAC achieved another magnitude of technological progress in carrying out burdensome calculating works. For instance, Orcutt's machine needed four to five minutes to punch a card and about 30 seconds to obtain every sum of the cross-products (Orcutt, 1948a, 68–9). The DAE's first EDSAC programme computed over 400 cross-products in about seven minutes, indicating that the EDSAC was at least 50 times more efficient than Orcutt's machine and 600 times more than the human computors.[115]

6. The Consolidation of Microeconometrics

With significant improvements to the microdata sources and technology, econometric practices by the DAE affiliates boomed in the early 1950s, constituting the first series of collective

[114] The history of Orcutt's analyser is documented in Section 2, Chapter IV.

[115] 'It takes about 7 minutes on the Edsac to compute all the 55 weighted sums of squares and cross-products of 10 variables with 40 observations in addition to about 4 hours for punching and checking the number tape and verifying the results by a sum-check. A human computer with an electric desk machine would probably need about 75 hours for this job, so that 71 hours of labor are replaced by 7 minutes of machine time' (Brown, Houthakker, & Prais, 1953, 423).

contributions to post-war microeconometrics. The dataset used by these works were primarily based on the 1937–8 working-class expenditure survey and 1938–9 middle-class expenditure survey, the former of which was truncated to 2,225 households after omitting agriculture and rural households to exclusively represent urban occupations. These contributions included a series of journal articles covering issues that included *demand analysis*, *Engel curve*, and *income distribution*, culminating in three departmental monographs: *Measurement of Consumers' Expenditure and Behaviour in the United Kingdom* by Stone and Derek Rowe (1954), *the Analysis of Family Budgets* by Prais and Houthakker (1955), and *the Lognormal Distribution* by Aitchison and Brown (1957).

Stone and Rowe (1954) was a summary of Stone's econometrics of consumer behaviour, dating back to his interwar demand analysis (Stone, 1945), which estimated the demand for selective commodities using national income time series. The monograph extended his previous study to include extra time-series data on the foodstuffs and new microdata from the two budget surveys to provide comprehensive estimates for market demands in the UK between 1920–38. Chapters II–XVI of the study analysed, in detail, the price elasticities from time-series regression and the income elasticities from budget regression for every single and subgroup of commodities. The estimates from both were reported, in which some of the substitutes were taken as another regressor. Both results were visualised as bunch maps and juxtaposed at the end. In the study, the baseline econometric model was changed from his original 1945 model,[116]

$$log q_i = \alpha_i + \beta_{i0} log x + \beta_{i1} log p_i + \beta_{i2} log \pi + r_i t$$

to a framework with some variables adjusted,

$$log(q_i/n) = \alpha_i + \beta_{i0} log(x/n) + \beta_{i1} log(p_i/\pi) + r_i t$$

The method of estimation was built under the Cochrane-Orcutt transformation,

$$\Delta[log(q_i/n) - \beta_{i0} log(x/n)] = \alpha_i + \beta_{i1} \Delta log(p_i/\pi)$$

where commodity expenditure q_i and total expenditure x were adjusted according to the number of equivalent adults n in the population, the commodity price p_i was deflated by the retail price index π, and the Cochrane-Orcutt transformation was adopted to eliminate the time trend t.

The most significant difference between Stone's 1945 and 1954 models was the empirical content of income elasticity β_{i0}. While in the 1945 model, β_{i0} was estimated from annual time

[116] All forms and notations are taken from Gilbert (1991, 298).

series, in the 1954 model, β_{i0} was obtained from budget materials based on the dummy variable regression below,[117]

$$\log\left(\frac{v_{iR}}{x_{1R}}\right) = \log A_i + B_i \log\left(\frac{v_R}{x_{1R}}\right) + C_i x_{2R} + D_i \log x_{1R} + \varepsilon_i$$

where v_{iR} was the expenditure on the *i*th commodity in the *R*th group, and v_R was the total family expenditure; both were divided by the family size x_{1R}. B_i measured the total expenditure elasticity of a single commodity. x_{2R} was a dummy variable distinguished by the 1937–8 working-class survey and 1938–9 middle-class survey, and C_i was the occupational difference in total expenditure. D_i explained the contribution of family sizes, and ε_i was the error term.

Since B_i measured the total expenditure elasticity, this concept was different to the income elasticity required in the demand equation. Stone adopted a 10% reduction of B_i to approximate the income elasticity. The reduction was based on Houthakker's (1952, 20) estimation of MPC using the middle-class surveys, in which around 1,100 families reported their income information. That led to

$$\widehat{\beta_{i0}} = 0.9B_i$$

Stone elaborated the standard procedure in Chapter XX. The empirical framework decomposed the expenditure variations into price and income effects, which could be separately measured by time series (β_{i1}) and budget materials (β_{i0}). After the income elasticity was captured by the reduced B_i and the income variations were obtained, the residuals would be explained by the price variations and individual differences:

> … the variation in consumption per equivalent adult thus attributable to changes in income per equivalent adult is removed from consumption per equivalent adult in the time series, and the residue is related to changes in relative prices and to time as an indicator of the slowly changing effects of tastes and habits. (Stone & Rowe, 1954, 310)

Stone's 1954 model demonstrated considerable practical progress in estimating the market demand equation in both aspects of time series and cross-sectional data. The time-series problem of autocorrelation was tackled by the Cochrane-Orcutt transformation to remove the time trend. In their article, Cochrane and Orcutt (1949) had shown that Stone's (1945) time-series data could be corrected by taking first differences. Apparently, Stone drew upon the Cochrane-Orcutt inquiry and then applied it in his 1954 analysis, producing 'satisfactory results' (Stone & Rowe, 1954, 290). Moreover, during the interwar period, Jacob Marschak had experimented with the synthesis of levels of data in demand studies (Section 6, Chapter II).

[117] In this equation, Stone's original notation is adopted.

Stone's method, which utilised both time series and budget data, brought Marschak's (1943) pooling method back to the practical domain. Stone's and Marschak's methods were similar in principle. They both explained the total quantity variations between price and income and used the micro-level total expenditure and macro-level national income to extrapolate the hypothetical income variations for other years.[118] The differences between them were that Stone had included a dummy variable to account for the class differences and relied on Houthakker's estimation to correct the total expenditure elasticity.

Based on his 1954 study, Stone managed to aggregate his demand estimates at the upper level. Stone (1954) presented a model of linear expenditure systems (LES), through which he tried to synthesise those market demand equations into a compact system. The LES assumed that any expenditure on a commodity was only dependent on all commodity prices and total expenditure. A linear applicable expenditure system could be derived under three other auxiliary assumptions: adding-up, homogeneity, and symmetry. Stone stated,

> … that the expenditure on all the commodities be equal to total expenditure, that each demand be homogenous of degree zero in income and all the prices, and that the matrix of elasticities of substitution be symmetric. (Pesaran, 1991, 104)

Therefore, the LES approached a demand system with few variables and evaded the problem of utility measurement. Stone then applied his market demand data from between 1920–38 to derive an LES and extrapolate it to the 1920 and 1952 levels. Stone's LES paper was an important step forward by making estimates from a *group of single estimates* and to a *price-interdependent system*. This paper was the first compact demand system in the literature and pioneered the entire subfield, culminating in Deaton and Muellbauer (1980). As Deaton evaluated, Stone's LES was 'a major landmark along the route that leads to where we are now' (Deaton, 2008b, 5) and 'a major breakthrough, not only in demand analysis, but also in applied econometrics in general' (Deaton, 2008a, 17).

Using the same interwar budget surveys, Prais and Houthakker (1955) estimated the U.K. Engel curves. The study responded to three kinds of literature in empirical economics: the U.K. poverty survey tradition, the measurement of the cost-of-living index, and the determinants of consumer expenditure. On the last point, they elaborated that the theoretical targets of this study were the Engel curve and Keynesian consumption function:

> The possibility of examining income variation in this way in order to derive estimates of income elasticities and of the marginal propensity to consume is no doubt one of the main

[118] In this sense, Gilbert's (1991, 298) perception that Stone and Rowe's study (1954) was 'the first example of mixed time series cross section estimation' may not be true.

reasons for the increased interest in family-budget investigations since Keynes directed attention to these concepts. (Prais & Houthakker, 1955, 4)

This quote shows that studies on the Engel curve and Keynesian consumption function at the micro level were sometimes mutually embedded, as discussed in Chapter II. However, while the bulk of the microdata was still limited to expenditure but not income,[119] Prais and Houthakker framed the study as 'attempts to add a brick' to the Allen-Bowley (1935) analysis of family expenditure:

> … it has taken the now classic work on *Family Expenditure* by Allen and Bowley (1935). The reader should not estimate its influence on our work merely by the number of explicit references to it in the following pages; there is hardly a chapter of this book which would have had the present form but for the work of these authors. (Prais & Houthakker, 1955, 6)

At the first stage, the study applied five specifications to choose a proper econometric model for 'testing between alternative hypothesis about the income elasticity or the marginal propensity to consume',

Double-log:	$\log v_i = \alpha + \beta \log v_0$
Log inverse:	$\log v_i = \alpha - \beta / v_0$
Semi-log:	$v_i = \alpha + \beta \log v_0$
Linear:	$v_i = \alpha + \beta v_0$
Hyperbola:	$v_i = \alpha - \beta / v_0$

where v_i was the expenditure per head on item i, and v_0 was the total expenditure per head approximating total income.[120] The computations of these models were based on the EDSAC, and their practical experiences of using machine were recorded in Chapter 6.[121] Unlike Bowley's and Stone's regressions, the empirical model of Prais and Houthakker (1955) omitted the family size as an additional explanatory variable. This setup was because their study targeted the homogeneity hypothesis as a working hypothesis that '*consumption per person depends only on the level of income per person*' (Prais & Houthakker, 1955, 88). Accordingly, Chapter 7 obtained the income elasticities under all specifications from the two budget surveys for six food categories: farinaceous, dairy, vegetables, fruit, fish, and meat. They found that the double-log form gave 'a fairly satisfactory description' for most commodities and semi-log form for most foodstuffs in terms of goodness of fit (Prais & Houthakker, 1955, 103). Thus, they reported estimates of the total expenditure elasticities of all food items under the semi-log form and of all non-food items under the double-log form. In Chapters 8–10, they proceeded to examine divergent topics on consumer behaviour, including quality variation, family

[119] They only reported the marginal propensity to consume in a small section (Prais & Houthakker, 1955, 100–2), which was basically the same result from Houthakker (1952, 20) applied in Stone and Rowe (1954).
[120] Allen and Bowley (1935) used the linear form, and Stone and Rowe (1954) used the double-log form.
[121] For more details, see Brown, Houthakker, and Prais (1953).

composition, and economics of scale, with some observations that allowed them to calculate the commodity price. Chapter 11 provided the dummy regression estimates on the difference in income elasticity between the two social classes in the budget surveys.

Finally, empirical research on the income distribution at the DAE started with A. D. Roy's (1950) study on the distribution of hourly outputs through his self-sampled individual data from various factories. Roy's study confirmed the lognormality of hourly earnings, which were approximated by individual outputs. Aitchison and Brown (1957) applied Roy's idea from studying the mathematical features of lognormal distribution to its empirical applications in economics. The mathematical exercise in Chapters 4–6 was based on the Monte Carlo method that generated 64 'artificial' random samples using the EDSAC for testing the lognormality through different methods of estimation.[122] Chapters 11 and 12 explored the uses of lognormal distribution to describe the actual income from scattered data of individual earnings (Chapter 11) and surveys of household budgets (Chapter 12). For the latter, they showed that the macro-models of household behaviour could be aggregated from Prais and Houthakker's (1955) micro-model by assuming the lognormality of total expenditure. They thus argued,

> … the lognormal hypothesis for the distribution of some variable may be used to decide the manner in which aggregation modifies the initial micro-equation; and they may perhaps serve as an introduction to the more general problem of discovering the econometric laws which are applicable to statistical populations rather than to individual entities. (Brown & Aitchison, 1957, 123)

The 1937–8 working-class survey was used as an illustrative example for validating this argument. The results showed that a lognormal curve could fit the expenditure distributions of most commodities (food, rent, clothing, and fuel). Based on that, they formalised the procedure of consolidation and then computed the aggregated Engel curves of selective commodities.[123]

These departmental monographs on consumer behaviour made the DAE the first-rank institution in econometric research during the mid-1950s. Apart from Stone and Rowe (1954), which was applauded as a classic study in demand analysis,[124] Prais and Houthakker (1955) and Aitchison and Brown (1957) received wide attention from contemporary economic literature.[125] Many articles as side products of these three monographs were published in leading journals, such as the *Journal of the Royal Statistical Society*, *Economic Journal*, and

[122] The technical issue while using the EDSAC was addressed in Chapter 13. At the DAE, the use of the Monte-Carlo approach could be traced back to Orcutt and James (1948). See the discussions in Section 2, Chapter IV.
[123] Aitchison and Brown (1957, 129) termed the consolidation and aggregation differently, while the former was 'in distinction from aggregation as we wish to hold the latter term in reserve for the process of averaging consumption data over the distribution of incomes'.
[124] For instance, Allen (1954, 124) praised the study as 'beyond adequate praise'.
[125] Before 1960, the Prais-Houthakker study had 16 review articles, and Aitchison and Brown (1957) had 13.

Econometrica.[126] Later, the DAE's econometric framework of household surveys was widely recognised by their contemporaries, for instance, in James Tobin's (1957b, 493) assessment:

> In socialist Britain, the government turned over the basic data for analysis by a private research organization, the Department of Applied Economics at the University of Cambridge. The results speak for private enterprise in research. Britain could learn much from American techniques of data collection, and America has much to learn about the analytic exploitation of survey data.

The DAE's booming contributions to microeconometrics was due to the teamwork started under Stone's coordination and the cooperation with econometricians. To this end, the DAE was like a rowing club, where Stone was the captain and econometricians were crew members of different boats. Stone's duties as the captain were to track every boat's progress and ensure that every crew would be rightly assigned to each boat. Even though rowers came and left, Stone was in the best position placing a suitable person on the roster. What explained the productiveness of this rowing-club was that in the 1950s, econometrics, as an applied science, had developed into a stage focused on collective effort. As Roy Allen (1954, 124–5) commented on the Stone-Rowe study,

> The moving spirit in this vast enterprise [Stone & Rowe, 1954] is Richard Stone himself, but he lays great emphasis, quite rightly, on the team work involved … except for an occasional and brilliant pioneer, no single individual can any longer expect to produce basic results in applied economics.

Stone and Rowe (1954, xxvi) elaborated that two of the reasons behind this shift to teamwork were the fact that collecting new data was becoming laborious and solving econometric problems was becoming specialised:

> First, in many cases the number of facts that have to be assembled for a piece of economic analysis are numerous and those required for different pieces of analysis are frequently related … It is obviously better that whole sets of facts, such as the data for social accounting in its widest sense, should be compiled on a single plan directed from one centre … Secondly, the coordination of economic facts and theories involves essentially more than just a knowledge of economics; in addition, there are mathematical problems of formulation, statistical problems of estimation and the testing of hypothesis and problems of computation.

The DAE experience is one of the most successful examples of econometric collaborations in the early years of computer-based calculation. Focusing on their individual but complementary tasks, Stone and the DAE crews cooperated to solve different pieces of puzzles in econometrics. Such an extensive collaboration and sophisticated division of labour were almost unseen in the interwar period: Stone's demand analysis not only benefited from the autocorrelation test developed by Durbin and Watson and the transformation by Cochrane and

[126] See Chapter V for further analysis of these materials.

Orcutt but also Houthakker's evidence of income elasticity. Prais and Houthakker expanded Stone's regression and offered a complete set of estimations of the Engel curve. The results of Aitchison and Brown buttressed the Prais-Houthakker framework by exploring the distribution of household income. In addition, to handle these large-scale computations of microdata, Brown, Houthakker, and Prais acted as the first generation of programmers of EDSAC who offered expertise in arranging punch-card programmes.

In the 1950s, when data, theory, and computation reached higher complexities, there was hardly only one hero, but a caption coxing different boats could make more efficient progress in producing empirical knowledge. Although Stone was not always directly involved in most econometric collaborations, he was pivotal as the coordinator in facilitating innovations between crew members. His unique role has been confirmed by individual numbers, such as Orcutt (see Section 2, Chapter IV), the bibliometric analysis of the acknowledgement network (see Chapter V), and comments from Angus Deaton (2008b, 5–6), who stated that,

> … his [Stone's] personal influence has been extraordinarily strong, partly because of the compelling lucidity of his writings, but also by the example he set to the stream of economists and statisticians who spent time in the DAE with him … Not only did all of this work owe much to Stone's presence and to the existence of the DAE, but the joint output of all of these people represents an explosion of econometric and economic knowledge that has never been exceeded in the history of the subject and has perhaps been equalled only by the work of the Cowles Commission.

7. Concluding Remarks

In 1952, Stone was offered the P. D. Leake Chair of Finance and Accounting at Cambridge, a research professorship without teaching and administrative duties. He was happy to accept the offer and keep the directorship of the DAE. However, the Faculty of Economics made him choose one of them by changing the employment rules so that the director could not hold a professorship. This consequence was due to, as Barker (2017, 839) observed, the distinct separation of the DAE from the Faculty of Economics, as verified by Geoffrey Watson's impression:

> There were no departmental boundaries where I was concerned, but the joke used to be that this was an applied economics group. We weren't allowed in the door of Economics. All the economists were anti-mathematical. They believed you had to do it with words, which was bloody hard. You have to be very clever to say all these things, for example marginal utilities – quite hard to define in words but mathematically trivial. In fact, the economists [at the Faculty] thought that Richard Stone was so subversive they made this little extra department to keep him out of theirs. (Beran, Fisher, & Watson, 1998, 77, cf. Barker, 2017, 839n)

Although Stone was unwilling to take sides in this controversy, the hostility from some economists of the Cambridge Circus ultimately forced him to step down from the directorship in 1955. Under the new directorship of Brian Reddaway, the DAE's research gradually moved away from econometrics (Smith, 1998, 99–101). Afterwards, Stone shifted his research to modelling the national economy and continued to work on various projects with the DAE, including the analysis of market demand for durable goods (Stone & Rowe, 1957; 1960), the Cambridge Growth Project, which was initiated in 1960 with Alan Brown (Pesaran & Harcourt, 2000, F158–F161; Barker, 2017, 847–9), and input-output modelling (Marangoni & Rossignoli, 2014). Although the boats he managed were long gone, Stone stayed connected with the DAE until his retirement in 1980.

Chapter IV. Guy Orcutt and the Creation of Microanalytic Simulation in Empirical Economics

1. Introduction

This chapter traces the creation of microanalytic simulation (henceforth, microsimulation), an empirical tool in microeconometric modelling created by Guy Orcutt in the late 1950s. Nowadays, microsimulation is widely applied by different government agencies and academic institutions to understand the consequences of demographic, tax, welfare, health, and redistributive policies.[127] Examples encompass the Policy Simulation Model owned and used by the British Department for Work and Pensions, the EUROMOD, which analyses tax-benefit policies of the European Union, and the Urban-Brookings Microsimulation Model, constructed by the US Tax Policy Center. The Urban Institute played a crucial role in developing this technique for U.S. policy evaluations. As a think tank founded in 1968 in the wake of Lyndon B. Johnson's War on Poverty, the institute's research centred on urban problems and the efficacy of federal welfare programmes, for which it built simulation programmes in cooperation with other agencies. During the early 1990s, microsimulation has been an essential policymaking tool for the federal government (Citro & Hanushek, 1991, 1–2).[128] In 2019, the Institute used six microsimulation programmes, spanning different policy areas from health insurance, transfer income, and social security benefit to social mobility.

Despite its wide application in policymaking, microsimulation as an empirical technique and its history in economics have still not been sufficiently explored. Following Morgan's (2004; 2012, Chapter 8) rediscovery, a key name in the story was Guy Orcutt, who first conceptualised economic microsimulation and implemented it back in the 1950s (Orcutt, 1957; 1960; Orcutt et al., 1961). Orcutt also played a pivotal role in bringing microsimulation to the

[127] Even though they started off from different pasts, microsimulation is now often connected with 'agent-based modelling', a more recent project in the economic toolbox. Developers of agent-based modelling also see microsimulation as their precursor – see, for instance, Gallegati, Palestrini, and Russo (2017, 15–7).
[128] Others are large-scale macroeconomic models, single-equation time-series models, cell-based models of population groups, econometric models of individual behavior, and large-scale microsimulation models.

Urban Institute in the 1970s and served as the chief coordinator in developing one of its simulation programmes, the Dynamic Simulation of Income Model (DYNASIM; Orcutt, Caldwell, & Wertheimer, 1976). However, while Orcutt is more often remembered as an econometrician for his co-creation of the Cochrane-Orcutt estimator (Cochrane & Orcutt, 1949; Orcutt & Cochrane, 1949), his role in the development of microsimulation has received little attention.

The story of Orcutt's microsimulation begins with his background in engineering and physics and his fascination with Jan Tinbergen's macrodynamic model. With these in mind, two senses of economic engineering were interconnected with Orcutt's career. First, based on his early attempt to design an electric calculation machine called the 'regression analyser' (Orcutt, 1948a), he developed the idea that viewing the national economy as an engineering feedback-loop could potentially solve the methodological pitfalls of the Haavelmo-Cowles programme. Orcutt's first demographic microsimulation, which he finished in 1961, was a rigorous exemplar that applied an engineering perspective to a microeconometric analysis of the socioeconomic system. Second, after a failed attempt at the Wisconsin Social Systems Research Institute (SSRI), Orcutt's microsimulation regained government attention in the late 1960s, when an intellectual demand for real-world programme evaluation emerged. The 1976 DYNASIM was designed at the Urban Institute with the expectation that policymakers could use the simulation programme to obtain more credible evidence in policymaking, such as deciding which wage policies would efficiently reduce gender-wage gaps or estimating the future caseload of welfare programmes. Eventually, microsimulation developed out of Orcutt's dream to build a Tinbergen-style model into a tool of microeconometric modelling used by policymakers to reorient society.

2. Orcutt's Early Years

Guy Henderson Orcutt (1917–2006) was born in Wyandotte, a suburb area near Detroit in Michigan. As the son of a superintendent of electric engineering at the Michigan Alkali Company, he became interested in designing experiments with electronic circuits during his teenage years. After first enrolling as an engineering major at the University of Michigan, he switched to physics and mathematics and a philosophy minor. Orcutt graduated in 1939 and then stayed at Michigan to pursue a postgraduate degree in economics under the supervision of Arthur Smithies. Lacking an economics degree, Orcutt was required by the economics

department to take five undergraduate economics courses in his first semester. In these courses, Orcutt met James Duesenberry, one of his teachers and an advisee of Smithies. They soon developed a close friendship, along with fellow student Daniel Suits. Orcutt (1990, 7) recalled these stimulating friendships as 'an enormous help to me in developing a commitment to my new field and in broadening and deepening my rather limited understanding of economic theory'.

Orcutt obtained his PhD in 1944 with a doctoral dissertation entitled, *Statistical Methods and Tools for Finding Natural Laws in the Field of Economics* (Orcutt, 1944). The first part of his dissertation provided a theoretical bridge between the induction and deduction methods in economic inferences with time-series non-experimental data.[129] In the second part, building on his interest in engineering and his training in physics, Orcutt developed the idea of building an analogue computational machine, which he called the 'regression analyser' (RA) to calculate series of multiplication and least-square estimates.[130] The mechanical structure of the RA was transplanted from what had already been developed for solving differential equations.[131] The RA contained a combination of electrical circuits and resistances, in which both input and output were measured by voltmeters. Thus, when inputs varied, outputs could be calculated through its specific electrical properties. Initially, the prototype of the RA was the result of Orcutt's attempt to obtain numerical solutions in duopoly and spatial market models inspired by Smithies's work with L. Jimmie Savage.[132] In 1940, Orcutt had already designed two analogue-mechanical devices, 'one machine had to do with spatial location problems, taking into account transportation costs; the other dealt with duopoly problems involving a spatially distributed market and freedom of location along a line' (Orcutt, 1990, 8). The original design of the machine is untraceable, but Smithies (1941b, 424n) provided several technical details:

> G. H. Orcutt of the University of Michigan has constructed a mechanical model for solving this problem with a greater degree of generality than is possible by analytic methods. The principle of the machine is to represent, for each competitor, price, quantity per unit distance, and distance by voltage drops along linear resistance wires. These resistance wires are included in an electric circuit such that the product of these three voltages, i.e., total profits, can be read off a voltmeter. The machine is operated by varying price and distance for each

[129] He presented this topic at the 1944 Cleveland econometric society meeting (*Econometrica*, 1945, 82).
[130] Several years later, a similar idea was implemented by Walter Newlyn and Bill Philips in the UK for the Newlyn-Phillips machine, an analogue computer used to model the national economy under the logic of hydraulics (Morgan, 2012, 176–84; Backhouse & Cherrier, 2017, 106).
[131] In the early 1930s, Vannevar Bush and Harold Hazen at the MIT built a working version of a differential analogue analyser (Bush, 1931).
[132] Savage was a graduate student in mathematics at the time. Their works were published as Smithies and Savage (1940) and Smithies (1941).

competitor, in accordance with the assumptions of the problem, until a simultaneous maximum is achieved.

These analogue machines were, in Orcutt's view, the 'first use of a simulation approach to generate specific solutions of economic models' (Orcutt, 1990, 8). In other words, he saw these devices not only as machines undertaking numerical calculations, but also as prototype models that could emulate real-world phenomena. The structures of electric circuits represented different mechanisms and assumptions of economic models, and changing voltage inputs within the model allowed him to understand the consequences of certain manipulations in reality.

During his doctoral years, Orcutt gradually shifted his attention from economic theory to econometrics. As he was 'impressed with the scarcity of efforts to estimate relationships between various available time series of data',[133] he started to think about the extent to which empirical data could actually measure the macroeconomy. Orcutt began to engage with econometrics under the guidance of Smithies, who introduced him to the works of Eugen Slutsky, Trygve Haavelmo, and Jan Tinbergen.

Among these canonical works, two direct influences of Orcutt were the summation of random series by Slutsky (1937) and the macroeconometric modelling by Tinbergen (1939).[134] The problem was pinned down mainly on the examination of the validity of statistical estimates from economic time series. On the one hand, since Slutsky had shown that the aggregation of random series led to regular business cycles, Orcutt realised that autocorrelated economic time series should be distinguished as another problematic object. He recalled, 'I also became aware of Slutsky's paper on random summation. It was quite clear that anything which did in autocorrelation would be different from those done in random series. That was the key thing'.[135] As such, Orcutt's takeaway from Slutsky's experiments was that economic time series, whether aggregated from periodic or random causes, were generally serial correlated.

[133] Duo Qin, interview with Guy Orcutt, 1988, interview note. The current author thanks Professor Duo Qin for sharing her unpublished interview note through email and making the note available on request. This interview was part of Professor Qin's PhD field work conducted at Yale University in 1988. The note was cited once in her book *The Formation of Econometrics* (Qin 1993). Orcutt's quotes in the rest of this chapter are direct quotes from the note.

[134] Slutsky (1937) conducted multiple statistical experiments to test whether different summation processes of the random series led to cyclical fluctuations. His results confirmed this claim and showed that the cyclical pattern was similar to economic business cycles. The article evidenced that economic cycles were not necessarily generated from the aggregation of known causes. For a history of Slutsky's work, see Morgan (1990, Chapter 3.1.2). Tinbergen's (1939) model was the first large-scale macroeconometric model of the US. The model contained 71 variables from 1919 to 1932 and 48 equations to explore the statistical properties of business cycles. For a history of Tinbergen's macrodynamic models, see Morgan (1990, Chapter 4).

[135] Duo Qin, interview with Guy Orcutt, 1988, interview note.

On the other hand, Orcutt was most fascinated by Tinbergen's (1939) macrodynamic model of the U.S. national economy and was convinced that the model of the macroeconomy could be constructed from a system of equations:

> The monumental efforts of Jan Tinbergen [1939] seemed to offer a good start in providing the empirical postulates upon which business cycle theory might profitably be built … it appeared that a useful objective might be to develop improved tools for deducing the mutual consequences of large numbers of simultaneous difference equations. (Orcutt, 1990, 8)

As with Orcutt's early designs of analogue machines to solve spatial location problems, Tinbergen's macroeconometric modelling was a useful tool for Orcutt to mathematically describe the complexities of the real world. Furthermore, once suitable statistical data were plugged into the model, it was possible to uncover the empirically measured version of the unknown structure. The power of Tinbergen's work triggered young Orcutt's ambition of grand model building, which would last throughout his career. Orcutt never lost this 'Tinbergen dream'; later in life, he reflected, 'I always had the dream of doing something like Tinbergen did, to build a model which could cover all the system. I still have it'.[136]

Thus, in his dissertation, Orcutt delved into the analysis of the autocorrelated nature of economic time series and designed the RA to deal with it. This research idea was apparently inspired by Tinbergen's macroeconometric model but was limited to testing the empirical validity of his data; Orcutt recalled, 'Tinbergen's work really excited me. I was quite anxious to pursue his work. I thought maybe I was able to do something about test of significance on the series. So I started designing machines that would do the kind of calculation'.[137] However, despite proposing the preliminary design, his dissertation did not build a workable version of the RA (Orcutt, 1990, 9).

After he was turned down for a position at the Cowles Commission,[138] Orcutt's first job was an instructor of economics at the Massachusetts Institute of Technology (1944–46), where his main duty was lecturing on introductory economics based on the finalised version of Samuelson's textbook. During the rest of his time, he worked on constructing the RA. The building works were facilitated by the mathematics professor George Wadsworth and his laboratory as part of a weather forecasting project for the US Army Air Forces (Orcutt, 1990, 9). The project applied regression techniques and assumptions of distributed lags in building a rainfall forecasting model (Wadsworth, 1948), and Wadsworth offered Orcutt machinists and a laboratory for building a workable machine that could perform multiple regressions. The

[136] Duo Qin, interview with Guy Orcutt, 1988, interview note.
[137] Duo Qin, interview with Guy Orcutt, 1988, interview note.
[138] Orcutt's main competitor was Lawrence Klein.

result was indeed exciting. The size of the RA was about 12 by 18 by 24 inches, with an oscilloscope for plotting scatter diagrams that could visualise the data when different voltages changed between horizons (Figure 6). The RA accommodated up to three time series, with each up to 30 observations (Orcutt, 1990, 10), requiring 40 to 50 seconds to calculate one simple regression.[139] The demonstration of this new machine took place at the 1945 annual Christmas meeting of the Econometric Society.[140]

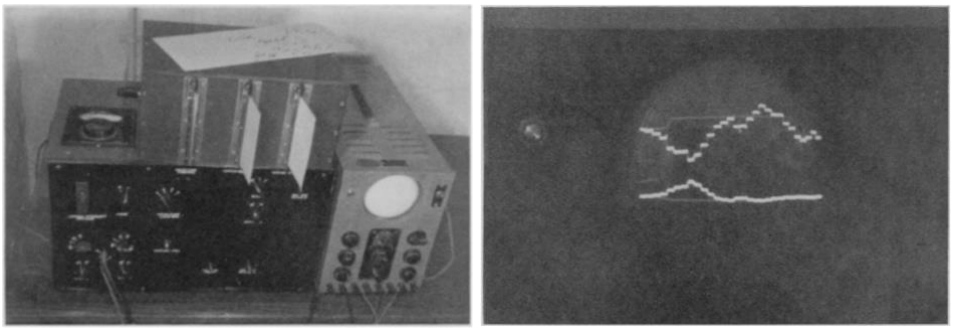

Figure 6 The regression analyzer and its cathode ray oscilloscope (Orcutt, 1948a, 64)

During the summer of 1945, after Smithies's introduction, Orcutt submitted a research statement to Richard Stone, who was looking for new faculty members to join the DAE at the University of Cambridge.[141] The letter enclosed two proposals, one for his regression machine and its potential in applied economics and one for a 'sampling experiment' to find a satisfactory test of significance for economic data. The idea of the latter was whether 'what to expect by chance on the frequency distribution of correlations obtained between non-related series' could 'exhibit the properties of continuity which we believe our real series to have'.[142] Coming from the statistical tradition of Cambridge, Stone was sympathetic to Orcutt's proposal, which fit the 'English' sampling experiment.[143] He was also convinced that the RA's emphasis on better

[139] 'After the cards are punched, it takes about forty to fifty seconds to do a simple correlation. Since each card is used several hundred times in this current problem, the punching time per correlation is negligible'. Letter from Orcutt to Stone, 15 May 1946, JRNS/3/1/96, Papers of John Richard Nicholas Stone, King's College Archive Centre, University of Cambridge.
[140] The paper was titled 'A Machine for Determination of Correlation and Regression Coefficients' *(Journal of the American Statistical Association*, 1946, 78).
[141] Letter from Orcutt to Stone, 22 August 1945, JRNS/3/1/96, Papers of John Richard Nicholas Stone, King's College Archive Centre, University of Cambridge.
[142] Chapter 3 of Orcutt's 1944 thesis tackled this idea without any empirical investigation, where he cited the Cambridge statistician G. U. Yule's (1926) 'spurious correlation' as the primary reference for serial correlation (Orcutt, 1944, 52).
[143] This sampling-experiment tradition could be traced back to Yule (1927). His successors also inherited the notion and used random sampling technique to capture the behaviour of time-series data. For a history of Yule's work, see Morgan (1990, Chapter 3.1.1).

computational efficiency would ultimately bring 'immense value' to the DAE.[144] Therefore, when Stone went to visit Princeton the following academic year, he stopped by MIT to meet Orcutt and shortly afterward made him an offer to work at the DAE. In two weeks, Orcutt accepted this offer as a senior research worker with a 715-pound annual salary and became the DAE's first employee from the US.[145] On November 1946, with his family and machine, Orcutt arrived at the University of Cambridge.

These years in the UK indeed proved worthwhile for Orcutt (1990, 10), who later remembered this period as 'two of the most intellectually stimulating and productive years'. At the Marshall library and Cavendish Laboratory, he worked on the two original proposals along with statisticians and econometricians, including Stone, Roy C. Geary, Maurice G. Kendall, Maurice H. Quenouille, Herman Wold, and a graduate student named Donald Cochrane. Stone played a significant role in supporting Orcutt's work and facilitating his communications with other members in the community. As Orcutt reflected,

> He [Stone] was a truly great director of a group. He put everyone of us in touch with whoever we should be in touch with. I got to know Kendall, Quenouille and Geary.[146]
>
> Our [Cochrane and Orcutt's] debt to Richard Stone should be noted for he not only provided support, suggestions and encouragement but also ensured my personal interaction with Tinbergen and with Herman Wold. (Orcutt, 1987, 133)

The first series of Orcutt's work at the DAE was on the empirical consumption function, by then still a rather small research agenda of the department (see the discussion in Section 4, Chapter III). With James Duesenberry, Orcutt's first research report at the DAE tested the relative income hypothesis with the U.S. monthly data from 1935 to 1946, and with A. D. Roy, he compiled an unpublished bibliography of the consumption function (Orcutt & Roy, 1949). The second series included four journal articles on time-series econometrics, for which the improved RA (Orcutt, 1948a) had performed most of the calculations.[147] As Orcutt 'was thinking very much of Tinbergen',[148] his first published paper examined the autoregressive nature of the economic time series used in Tinbergen (1939, Appendix C, 205–7) with the sampling experiment technique (Orcutt, 1948b). The paper, which he saw as 'the best paper I

[144] When he tried to import the RA to the UK, Stone wrote to the custom officer F. W. Lawfield, 'It is a small piece of apparatus about the size of a large table wireless set and was invented by Mr. Orcutt when he was working at the Massachusetts Institute of Technology … You will understand that we are very anxious to have the use of this machine, which will be of immense value to our research'. Letter from Stone to F. W. Lawfield, 11 November 1946, JRNS/3/1/96, Papers of John Richard Nicholas Stone, King's College Archive Centre, University of Cambridge.
[145] Letter from Orcutt to Stone, 14 January 1946, JRNS/3/1/96, Papers of John Richard Nicholas Stone, King's College Archive Centre, University of Cambridge.
[146] Duo Qin, interview with Guy Orcutt, 1988, interview note.
[147] The main improvement was that he modified the method that allowed the simplification of repunching cards.
[148] Duo Qin, interview with Guy Orcutt, 1988, interview note.

have written' (Orcutt, 1990, 11), argued that Tinbergen's series y_t under time t could be approximated by an AR(2) model with random error term ε_t:

$$y_{(t+1)} = ay_t + b\left(y_t - y_{(t-1)}\right) + \varepsilon_{(t+1)}$$

Supposing that Tinbergen's series were randomly drawn from an AR(2) procedure, the statistical property of that series should have been similar to random numbers. Therefore, after taking first differences, Orcutt generated the correlograms of the series and compared them with the correlograms produced from random numbers (Kendall & Smith, 1939) in order to test which parameters of the latter could provide the best fit.[149] Orcutt concluded that if Tinbergen's series were randomly sampled, $a = 1$ and $b = 0.3$ would provide the best statistical model for describing the autocorrelated nature of the series. Then, using the AR(2) model, Orcutt and James (1948) proceeded to construct a quasi-Tinbergen series with random numbers to test their autocorrelations by calculating the von Neumann ratio.[150] However, the results showed that the quasi-Tinbergen series still had the problem of autocorrelation. Orcutt and James argued,

> If economic time series are analogous to the constructed series used in this paper then, except in the cases where the sample autocorrelations happen to be low, such high correlations between economic time series may be expected by chance that we are unlikely to detect real relations. (Orcutt & James, 1948, 412)

The Orcutt-James sampling experiment offered evidence that searching for a true auto-regressive structure of the economic series was not a suitable framework when the sampling procedure was non-random. In other words, it was more reasonable to place assumptions on the error term rather than the population when the former was more contaminated by other influences. The paper thus conjectured that a small sample and non-experimental data like Tinbergen's series needed adjustments to eliminate the non-randomness of the error term. With that concern in mind, Orcutt started to work with Donald Cochrane, shifting the methodological focus to the autoregressive nature of the error term. The result was known as the Cochrane-Orcutt (CORC) transformation. They first examined the economic series used by Klein (1947; 1950), Girshick and Haavelmo (1947), and Stone (1945) with the von Neumann ratio and the idea of the Orcutt-James experiment, but they still found no satisfactory evidence supporting the randomness of the error term. Therefore, to eliminate bias from the error term, a proper

[149] A correlogram is a visualised way for presenting the autocorrelation in time-series analysis by computing correlation coefficient of each observed series with different numbers of lags in order to decompose their commonalities. In the literature, this could be traced back to G.U. Yule's (1927) correlation diagram. Kendall (1945) thought the term 'correlogram' first appeared in Wold (1938). Orcutt (1948b) followed this approach but used four lags as the x-axis.

[150] The ratio was proposed by Von Neumann (1941) and was reframed as the Durbin-Watson statistics (Durbin & Watson, 1950; 1951).

way was to adjust the time series using an 'autoregressive transformation' (Cochrane & Orcutt, 1949). The key assumption of the CORC transformation was that the error term was serial-correlated with one lag. That is, in an OLS model $Y_t = \beta_0 + \beta_1 X_t + \varepsilon_t$, the error term ε_t followed a stationary AR(1) structure,

$$\varepsilon_t = \rho\varepsilon_{t-1} + \mu_t, |\rho| < 1, \mu_t \sim N(0, \sigma^2)$$

After estimating ρ, the CORC transformation proposed that the OLS estimator could be adjusted by taking the quasi-difference, in which the iteration procedure was shown as:

$$Y_t - \hat{\rho}Y_{t-1} = \beta_0(1-\hat{\rho}) + \beta_1(X_t - \hat{\rho}X_{t-1}) + \varepsilon_t(1-\hat{\rho})$$

The CORC transformation successfully transformed Stone's (1945) economic series into one that did not reject the error-term independence (Cochrane & Orcutt, 1949, Table VIII) and its merits were experimented under different structural-form models (Orcutt & Cochrane, 1949). Despite the seemingly positive results, Orcutt had started to question the validity of using annual economic time series in structural estimations, writing,

> … a certain amount of scepticism is justified concerning the possibility of estimating structural parameters from aggregative time series of only twenty observations when generated by systems analogous to those examined in this paper. This scepticism will be considerably increased if it is also attempted to make a choice of variables and time lags from the same data. (Orcutt & Cochrane, 1949, 371–2)

3. The Birth of Microsimulation in Economics

After one year at the International Monetary Fund, Orcutt joined Harvard in the fall of 1949, where he spent nine years, mostly as an assistant professor. He started to work with Wassily Leontief for his Harvard Economic Research Project and organise an econometric seminar.[151] From that point forward, his econometric work in the UK, especially the CORC transformation, became one of the 'classics' among econometricians.[152] Based on his previous discovery regarding Tinbergen's data series, he turned increasingly critical of the Haavelmo-Cowles approach, which was mainly characterised by the simultaneous equation modelling (SEM) technique by Koopmans (1950) and its empirical exploration by Klein (1950). Between 1950–1955, Orcutt wrote seven book reviews, two of which critically assessed the works of

[151] Leontief initiated the Harvard Economic Research Project on the structure of the American Economy in 1948 and directed the project until 1972. Funded by private and public institutions, the project devoted to Leontief's input-output analysis of the U.S. economy. He received the 1973 Nobel prize for his contribution to input-output models in economics. For a short history of the project, see Carter and Petri (1989, 14–6).
[152] Duo Qin, interview with Guy Orcutt, 1988, interview note.

Koopmans and Klein (Orcutt, 1951; 1952), who were hugely inspired by Tinbergen's work.[153] With little empirical work but abundant reflective writings, the young Harvard econometrician began to question the usefulness of the SEM framework and aggregate-level information for policy evaluation. He found that a new empirical method was needed for modelling the national economy to solve those methodological pitfalls, thereby reframing his 'Tinbergen dream'.

Orcutt elaborated his criticism of the Haavelmo-Cowles programme by working on problems of *data*, *experiment*, and *recursivity*. First, Orcutt (1951) developed his criticism of the uncorrelated and unlagged assumptions of the SEM framework, in which he asserted that the strong independence assumption would bias the OLS estimator when some variables were omitted:

> The estimation procedures developed by staff members of the Cowles Commission and used by Klein are aimed at dealing with situations in which correlations between the omitted variables and the included explanatory variables arise because of the existence of additional relations. It should be recognized that there are other likely ways, such as the existence of errors of observation, in which correlation between omitted and included explanatory variables are likely to arise, and that the resulting biases may be substantial and are not eliminated by these methods of estimation. (Orcutt, 1951, 262)

In addition, based on his work with Cochrane, Orcutt was also sceptical of Klein's uncritical acceptance that the omitted variables were not autocorrelated with the error term. He felt that aggregate economic time series suffered from insufficient information to adequately expose economic dynamics. He thus endorsed the need to search for an alternative and credible data source:

> It is to be hoped that more econometricians will have the foresight to do as Klein is now actually doing and attempt to use data that may be expected to be more revealing than aggregative annual time series. (Orcutt, 1951, 263)

Second, as predictions from the Haavelmo-Cowles approach were based on multiple series of passive observation, it prevented model builders and policymakers from performing real experiments. By taking an analogy of experiments in electrical engineering, Orcutt (1952, 166) pessimistically elaborated this point,

> … we wish to determine from non-experimental data the values of the parameters of the structure in order to provide guidance to policy makers by predicting the consequences of changes in the structure. The problem thus is somewhat analogous to that of giving a radio to a physicist and asking him to determine the operating characteristics of each component

[153] Klein (1950, 1) wrote that the monograph was 'written in the spirit of Tinbergen's investigation and is intended as an improvement and extension of his results'.

part merely by observing the radio as it plays but without being able in any way to take it apart or rearrange its circuits or perform experiments on it.[154]

With this view, the proper model of a national economy should have opened the possibility for socioeconomic engineers to design an actual experiment that would unveil the structure of an economic system. He then proceeded,

> If given a wiring diagram and a specification of what are to be treated as the component parts, our physicist could determine the operating characteristics of each of the specified components; then a radio engineer could form some judgment as to the consequences of rearranging the components in any desired manner in much the same way that an economic policy maker might use information about the operating characteristics of components of the economic system, such as consumers, investors, etc., to predict the consequences of a modification or rearrangement of the components.

Orcutt's point was associated with the problem of identification. Without the information of its inner mechanism, the structural estimations of non-experimental data were unable to disclose causal relations like real experiments. In that regard, the radio analogy signified Orcutt's view of the national economy as an engineering machine; he believed that econometricians should have paid more attention to the design of a tractable economic system.[155]

Finally, as the SEM was not a recursive framework capable of capturing the co-movement of each variable, influenced by Tinbergen and Wold, Orcutt believed that a model of the national economy was recursive, so that the output was determined sequentially but not simultaneously (Morgan, 2012, 318).

> I always believed in Tinbergen's notion about recursive systems and Wold's idea about recursiveness rather than the standard American view of simultaneous-equations. I think the real thing are recursive. I am sure that's what Tinbergen and Wold thought.[156]

Those justifications characterised Orcutt's unease with the Haavelmo-Cowles methodology, which had failed to offer an experientable and recursive system with more revealing datasets. In the mid-1950s, he gradually shifted his research concern toward an alternative methodology of microdata:

> … I got the understanding that if one was doing planned experiments, one would certainly want to have as many of any kinds of entities as possible. The whole idea was that one would have experimentation. This was an idea I used to teach my students. If you didn't

[154] It seemed that Orcutt advocated this idea eagerly at Harvard. Harvard graduate Vernon Smith once recalled how 'over twenty-five years ago, Guy Orcutt characterized the econometrician as being in the same predicament as that of an electrical engineer who has been charged with the task of deducing the laws of electricity by listening to a radio play. To a limited extent, econometric ingenuity has provided some techniques for conditional solutions to inference problems of this type' (Smith, 1982, 929)

[155] This idea implied that a properly designed machine could be isomorphic to an economic system fits into cybernetics literature of that time, although Orcutt did not use the phrase explicitly.

[156] Duo Qin, interview with Guy Orcutt, 1988, interview note. For Wold's idea on recursiveness, see Morgan (1991).

> have planned ones, you'd like to have natural ones which somehow met most of the conditions of the planned ones. If you wanted to test hypotheses and you started in a background thinking about experiments, you certainly wouldn't want just to have one of a kind. That made me recognize the importance of micro data. This was how one got more observations and so more information. Time series of the aggregates had thrown away almost all the information.[157]

Orcutt's attitude toward aggregate-level data might be a consequence of Leontief's influence. At the time, they worked together on the Harvard Economic Research Project, and Leontief was critical of aggregate time series and the Haavelmo-Cowles methodology.[158] Like Orcutt, Leontief (1953, 5–6) was worried that the time series oversimplified the actual information:

> Both theoretical formulation and factual description must be reoriented if they are to be brought closer to each other. Much of contemporary abstract analysis is couched in aggregative terms. At worst, this robs it of any operational meaning; at best, it separates artificially the essentially analytical task of defining the aggregates in terms of the directly observed 'real' variables from the rest of the theoretical argument and shifts it onto the shoulders of the empirical investigator, who often is even unaware of its true import.

Another clue was the engineering analogy used by Leontief (1954, 228–9). Akin to Orcutt's radio analogy, Leontief argued that the probability approach to econometrics failed to disclose the relationships 'under the hood':

> It is as if we were asked to reproduce the blueprint of a complicated motor on the basis of our knowledge of the general principles of operation of internal combustion engines and no other specific information but that conveyed by the few dials located on the dashboard and possibly the noise coming from under the closed hood. And as if that were not difficult enough, the structural characteristics of the engine the economist is studying are known to change under the impact of its continual operation.
>
> The task as presented can hardly be accomplished. It certainly becomes much easier if we are allowed to look under the hood. It would, of course, be even more convenient if it were possible to stop the motor, take it apart and subject each of its components to any desired tests and measurements. That is what experimental scientists can do and economists cannot.

It is not clear whether Leontief took his motor analogy from Orcutt or vice versa. Nevertheless, the resemblance between the criticisms of Leontief and Orcutt demonstrated that they probably had a positive influence on each other. In Orcutt's later writings (Orcutt 1960; 1962), Leontief's input-output model was frequently mentioned as the second approach to modelling the national economy after Tinbergen's macrodynamic approach.

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[157] Duo Qin, interview with Guy Orcutt, 1988, interview note.

[158] On this point, the current author benefited from an unpublished paper by Professor Marcel Boumans on Leontief's criticism of statistical econometricians. See Boumans (2016b, 419–20) for a summary of Leontief's view on the Haavelmo-Cowles programme.

Orcutt's next step was to reconcile 'the large gap between microanalytic research and the application of the results of research to policy problems at the national level'. However, until 1955, Orcutt struggled with formulating an individual founded analysis of national economy, since 'there simply was no known way of satisfactorily aggregating relations about micro-components into macroeconomic relations' (Orcutt, 1990, 15).[159]

In 1956, Orcutt finally found the solution at his alma mater when he spent a summer as a fellow at the Survey Research Center (SRC) of the University of Michigan. Benefiting from interactions with members such as James Morgan and another frequent visitor, James Tobin,[160] he came to understand the SRC's Survey of Consumer Finance as an excellent source of micro-level data that could be used to represent the real population:

> The key idea, a direct result of my stay with the Survey Research Center, was the realization that, not only were the attributes of real populations relatable in a known way to the attributes of probabilistic samples from such populations, but also, in precisely the same way, probabilistic samples could be used to represent evolving populations implied by the theories embedded in a model. (Orcutt, 1990, 16)

Furthermore, with representative samples, the Monte Carlo method could be applied to aggregate those micro-relationships:[161]

> I thought, "If you could represent a real population with a real sample, why couldn't we represent a theoretical population with a synthetic sample? Why couldn't we have a real sample representation of the real population at the start, and then move forward in time according to behavioural relationships applied to micro entities?" The sample no longer was real, once I started moving it. It was a synthetic one. So I represented a theoretical population with my sample. By using sample representation, we could say things about aggregates. We could aggregate the outputs generated from micro-relationships. What we couldn't aggregate was those micro-relationships, because there was no way of aggregating micro-relationships to give you macro-relationships between macro-variables. So the idea was to use Monte Carlo technique.[162]

After his summer fellowship, Orcutt returned to Harvard and tried to construct a computer programme dealing with national-wide random samples; he wrote to James Morgan, claiming

[159] Orcutt referred to this as 'a serious aggregation problem', since he was more concerned about the *empirical* validity when summing micro-level information into a representative macro scale. He was not, however, involved in the concurrent discussions on microfoundations in macroeconomics, which questioned the micro-macro relationship of Keynesian models and its theoretical root in economic agents, such as Klein's aggregation program elaborated by Hoover (2012).

[160] Both Morgan and Tobin had been working on the SRC data since the early 1950s.

[161] The Monte Carlo method has two roots: first, the 1920s English sampling experiments and second, the 1940s post-war Thermonuclear Weaponry 'H-bomb' project. While Orcutt was immersed in the former, he probably took the term Monte Carlo from the first academic paper on the Monte Carlo method by Los Alamos Laboratory (Metropolis & Ulam, 1949), published in the same *Journal of the American Statistical Association* issue as Orcutt and Cochrane (1949).

[162] Duo Qin, interview with Guy Orcutt, 1988, interview note.

that some progresses had been made.[163] This conceptualised methodological framework became realisable as a result of the progress made in high-speed digital computers. Starting in 1956, Orcutt served as Harvard's representative to the Computational Center for the New England College and Universities (or New England Regional Computing Center, NERCC) (Orcutt, 1990, 15–6). After he had a chance to programme the newest IBM 704 in this IBM-financed centre, he knew that increasing computer capability would be the solution to the enormous computational burden. As he recalled, 'the concept of micro entities came to me much earlier, but it was until then I felt it was [sic] computationally feasible'.[164]

In 1957, Orcutt embarked on his first effort in microanalytic modelling with his Harvard PhD students Martin Greenberger, John Korbel, and Alice Rivlin. The new method – microsimulation – was introduced following two general accounts (Orcutt, 1957; 1960), in which he elaborated how his simulation approach solved the pitfalls of the Haavelmo-Cowles methodology. First, Orcutt (1957, 116) argued that microsimulation was an inference strategy based on emerging microdata of decision-making units, while the old macroeconometric models 'only predict aggregates and fail to predict distributions of individuals, households, or firms in single or multi-variate classifications'. Second, and most importantly, microsimulation was an empirical tool that allowed experimental manipulations and replications:

> An individual simulation run may be thought of as an experiment performed upon a model. A given experiment involves operating a model after first completely specifying a set of initial conditions appropriate to the model, a set of values of the parameters used in specifying relations contained in the model, and the time paths of those variables used in the model and treated as exogenous. Additional experiments would involve operating the model after respecifying the initial conditions, the parameters, and/or the exogenous variables. (Orcutt, 1960, 893)

The idea of microsimulation was illustrated in the final product: *Microanalysis of Socio-economic Systems: A Simulation Study* (Orcutt et al., 1961). The simulation took about 150 hours on an IBM 704; the computing hours were guaranteed mostly by the NERCC at MIT and the Littauer Statistical Laboratory at Harvard. Using the 1950 U.S. population census and SRC's 1955 household surveys, this study formalised 4,580 U.S. households (10,358 individuals) as the initial population and then simulated their aggregate trajectories of births, deaths, marriages, and divorces during 1950–60. Each individual, called a 'decision-making unit', would make a demographic decision based on different exogenous 'status variables' (e.g., sex, race, age) subject to a probability measure that was also specified based on the population

[163] Minutes of Executive Committee, 9 October 1956, Box 27, Institute of Social Research Records, Bentley Historical Library, University of Michigan.
[164] Duo Qin, interview with Guy Orcutt, 1988, interview note.

census. The relationships between the probability measure and status variables were called the 'operating characteristics' (OC). According to the OC, each individual with status variables would be mapped onto a probability measure presenting the likelihood of actual behaviour. As an example, Orcutt explained this concept through a simple case of mortality rate. Suppose a specified probability of death next month of a white man aged 34 years and 7 months was $P(male, white, 34y7m) = 0.0002$.

In this case, as the simulation started, this white man was drawn randomly to die with a chance of 0.02%. Otherwise, he had a 99.98% chance of survival to the next month and reacted again to another mortality measure $P(male, white, 34y7m)$. The process would repeat until the simulation ended. This study then aggregated the outcomes of all such synthesised individuals to obtain the 'national' level.

Thus, the study consisted of three steps: *the OC*, *initial population*, and *the Monte Carlo method*. Through linear regressions, the U.S. population census was used to specify the OCs, that is, to calculate the expected probability of death, birth, marriage, and divorce every month for different groups based on age, sex, and race. After a statistically representative population was constructed for the period between April 1950 and April 1960, a monthly demographic 'event' was designated to every individual through the Markov process. During the event, the computer would generate a uniform-distributed pseudo-random number from 0 to 1 for every individual. The event was determined to happen if the generated number was smaller than the expected probability derived from the OC. This entire procedure of Monte Carlo method was designed and assembled by Greenberger, who also wrote the 'random number generator' programme to serve the task of the Markov process.

Based on the simple recipe above, a microsimulation framework made the crucial assumption that the national economy could be disaggregated into micro-level behaviours and vice versa. Orcutt illustrated this idea by adopting an electric engineering analogy, as seen in Figure 7. The socioeconomic system could be presented as a closed feedback-loop that was 'wired' by those inputs and outputs of markets and decision-making units. A decision-making unit under predetermined status variables would flow into a market and then be processed by the OC. The processed outcome would flow out as the input of that decision-making unit in the next period. Orcutt emphasised that a microsimulation model was recursive: 'There is no simultaneous interaction between units, and hence there are no simultaneous equations involving more than one unit at a time to be solved' (Orcutt et al., 1961, 26).

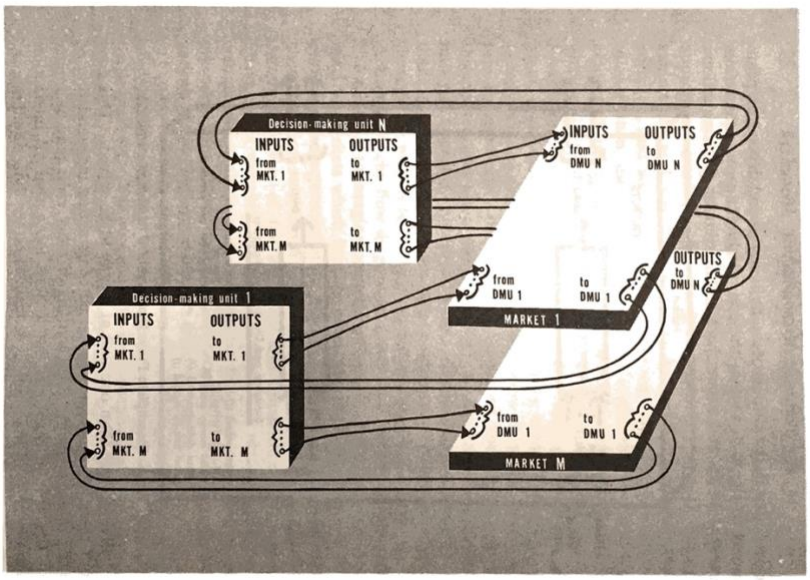

Figure 4. Incomplete Flow Diagram of a Model of an Economic System

Figure 7 The incomplete flow diagram of an economic system in microsimulation (Orcutt et al., 1961, 28)

4. Struggle at Wisconsin: Social Systems Research Institute

In 1958, one year after his initiation into microsimulation, Orcutt became more 'restless' due his rather slow promotion at Harvard and his desire for more resources for his grand project.[165] This was when Edwin Young, chairman of the economics department at the University of Wisconsin, offered Orcutt an excellent opportunity. By then, the junior Wisconsin institutionalist was concerned that his home institution might deteriorate into a 'third-rate department' due to the retirements of senior professors.[166] In response to this expected shortage, the recruitment committee decided to hire an 'imaginative theorist-econometrician' who could bring the department a 'renaissance' (Johnson, 1993, 142). Orcutt landed on the top of the list after several discussions between Young and Peter Steiner, a Harvard alumni and young assistant professor recruited in 1957.[167] Young acquired a five-year grant of 100,000 dollars from the Wisconsin Alumni Research Foundation (WARF) to persuade Orcutt to come to Wisconsin, with an appealing offer that allowed him to establish a research centre focusing on

[165] Laura Smail, interview with Edwin Young, 1978, 1981, transcript, Oral History Program, University Archives and Records Management Services, University of Wisconsin.
[166] Smail, interview with Young, 1978, transcript, Oral History Program, University Archives and Records Management Services, University of Wisconsin.
[167] Another key person who suggested Orcutt to Young was Sumner Slichter, who also knew Orcutt from Harvard.

microsimulation.[168] After Young's visit to Harvard, Orcutt gave up his tenured professorship and moved to Madison with a doubled salary.[169]

The arrival of the Harvard econometrician sparked hope for departmental revival from the new recruitments: not only did a Harvard econometrician, Jack Johnston, and Orcutt's co-author Korbel follow him to Wisconsin, but his reputation also attracted many talented academics over the next few years, including Arthur Goldberger, Arnold Zellner, Martin David, Charles Holt, Jan Kmenta, and Harold Watts. Outside the economics department, Orcutt helped to establish the statistics department by hiring its first chair, George Box,[170] in 1960 and by developing the new computer science department, where he served as an early faculty member (Orcutt, 1990, 18). As Robert Lampman (1993, 145) lauded, 'Bringing Guy Orcutt from Harvard – clearly the signal event in rebuilding the department – was parallel to bringing Richard T. Ely from Hopkins [in 1892]; in both cases the university announced an intention to invest heavily in the social studies. In each case the announcement was followed by a great burst of energy and new ideas'.

In the fall of 1959, with the launch of the SSRI at Wisconsin, Orcutt became the founding director. The aim of this new centre was twofold. Its first goal was to build a compact microsimulation of the US, an extension of Orcutt's dream. The second was to emulate the institutional model of the Institute of Social Research at Michigan;[171] Orcutt wanted the SSRI to act as a 'holding company' of quantitative social sciences as well as an 'umbrella institute' for facilitating interdisciplinary research.[172] Therefore, he tried to incorporate members from diverse backgrounds outside of economics, such as sociology, anthropology, political science, regional and urban planning, and statistics. This goal was also reflected in its institutional structure. Initially, the institute started with a three-workshop system that turned into three research centres in 1962: the Systems Formulation and Methodology Center (led by Zellner),

[168] At first, the WARF money was guaranteed mostly for natural science research. After Fred Harrington, a historian and a 'rigorous proponent of social research', became assistant to President E. B. Fred in 1956, the WARF eventually began to allocate part of its budget to social science research (Solovey, 1993).
[169] Seymour Harris (1958), head of the economics department at Harvard, wrote to the New York Times about Orcutt's departure, 'Many of the public universities are doing a splendid job and gaining in quality and prestige relative to private institutions. Last [academic] year one diverted a first-class economist from Harvard at double his Harvard salary'. Young was typically impressed by Harris's complaint, 'a place, no, kind of place like Wisconsin can take somebody away from Harvard … They were really upset … They didn't want to lose him'. Laura Smail, interview with Young, 1981, transcript, Oral History Program, University Archives and Records Management Services, University of Wisconsin.
[170] George E. P. Box (1919–2013) was an eminent British statistician. His work focused on experimental design, time-series analysis, and Bayesian inference.
[171] Laura Smail, interview with Guy Orcutt, 1988, digital audio file, two tapes, Oral History Program, University Archives and Records Management Services, University of Wisconsin.
[172] Laura Smail, interview with Martin David, 1981, digital audio file, two tapes, Oral History Program, University Archives and Records Management Services, University of Wisconsin.

the Center for Household and Labor Market Research (led by Goldberger and later David), and the Center for Research on the Firms and Market (led by Steiner). Meanwhile, four new centres were also established: Research Policy and Operations (led by Holt); Financial and Fiscal Research; Social Behavior Research; and Demography and Ecology (SSRI, 1963). Depending on their expertise, members of the SSRI were free to affiliate their memberships with those research centres that focused on complementary tasks. Each centre also enjoyed its autonomy to organise research seminars and working paper series.

Since constructing a microanalytic model and doing quantitative social sciences required enormous computer power to handle the data work, Orcutt emphasised the importance of advancing the research infrastructure accordingly. As he put it, 'Electronic computers are of tremendous importance in modern research in the social sciences … The SSRI seeks to bridge the rather large gap between social science and computer technology by maintaining a sizeable staff of professional and student programmers' (SSRI, 1963, 5). Between 1962–63, over 40% of the budget was used to hire programmers and update contemporary computational devices,[173] such as the IBM 1460 and the CDC 3600. In the spring of 1963, the Data Library was established to maintain all data on magnet tapes, which guaranteed its readability and accessibility.[174] Furthermore, inspired by Michigan's example, the Wisconsin Survey Research Laboratory was established to conduct regional surveys through state-wide interviews.

With capable econometricians, advanced computers, and a data and survey centre, Orcutt's situation seemed ideal. Following the publication of his 1961 book on microsimulation, he moved to extend the microanalytic model to household behaviour, that is, to identify the OC of household spending and consumption. During 1963–64, the research group contained two subgroups, one on the labour force and earning behaviour, led by Korbel and David,[175] and one on the consumer and portfolio behaviour, led by Goldberger and Zellner (SSRI, 1963, 8). To model the household behaviour, the second subgroup was of the utmost importance. Goldberger worked on the statistical nature of household expenditure on durable goods by analysing Michigan's SCF.[176] In the progress report presented at the 1961 annual AEA meeting, Goldberger and Lee (1962) found a stable pattern of household durable goods consumption using SCF data for 1951–60. They concluded that the reinterview of the SCF and a new

[173] Author's calculation from SSRI (1963, 97).
[174] See SSRI (1963, 7).
[175] Korbel's work on a microanalytic model of small business was published later (Korbel, 1965).
[176] The SCF was analysed earlier by the DAE affiliate Michael Farrell (1954) on the demand for automobiles, but then the demand for durable goods was not the DAE's central research focus (Section 5, Chapter III).

consumer survey conducted by the Wisconsin Survey Research Laboratory would provide useful references to specify the behavioural model of the household sector.

Orcutt (1962) presented a paper in the same 1961 AEA session, in which he argued for the need for microsimulation in economics. While reiterating its potential, he voiced a pragmatic concern that, at most, 10 million dollars from the governmental budget per year should be allocated to his approach, while he predicted that the return to the country would 'easily' be 1 billion dollars (240). Such emphasis reflected Orcutt's pressing need for external funding: the first-round WARF money would end in 1963, and therefore, he had to seek other soft money to keep his project alive and to preserve the institute's autonomy.[177] From 1960 onward, the SSRI also received research grants from the National Science Foundation, the Ford Foundation, and the Brookings Institution, in addition to the WARF money. Among those grants, the three-year fund (1961–64) of $400,000 from the Ford Foundation was the most significant relief. During 1962–63, the Ford grant of $135,000 covered almost half of the annual budget of the SSRI (1963, 99).[178] Naturally, the largest sponsor was expecting results from this huge investment, and Orcutt, as the principal investigator, inevitably faced pressure to ensure sufficient research progress.

Around the first half of 1964, Orcutt and his research team finally began a significant push to assemble household data into a computerised model.[179] However, the results were too unsatisfying to produce a workable programme for the household sector. While progress stalled, core members such as Zellner and Goldberger gradually refocused on their econometric works rather than on making the microsimulation practicable. As David recalled,

> Zellner concentrated a great deal of effort in trying to do we would call it a 'cohort analysis' of data on households in which one would try to combine information from surveys with time series data to produce results of considerably more detail than the pathbreaking econometric models of the time … Zellner was pursuing this more aggregated philosophy and eschewed the notion of simulation modelling at the microlevel, which was really Guy Orcutt's dream … his direction kind of moved off from Guy's … Goldberger became increasingly interested in estimation problems and the theory of how econometrics proceeded and did some consolidation of that field.[180]

Why did these deviations occur? Part of the reason was that under the institutional setting of the SSRI, members with their own priorities lacked incentives to pursue systematic goals.

[177] For Orcutt's struggle for funds, see Solovey (1993).
[178] Since then, the microsimulation has been renamed as Project MUSE (Simulation Models of the United States Economy) joint project with the Ford Foundation.
[179] Letter from Orcutt to Shubik, May 28, 1964, folder: 1964 (3 of 4), box 10, Martin Shubik Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University.
[180] Laura Smail, interview with Martin David, 1981, digital audio file, two tapes, Oral History Program, University Archives and Records Management Services, University of Wisconsin.

As Lampman observed, 'In the University there is remarkably little discipline possible in forming teams … Guy had in mind something like the Manhattan Project, which brought together a group of people of different disciplines and they all did fit into the plan sent down from the top … That kind of discipline was perhaps possible there, but not in an ordinary university setting'.[181] A top-down approach to push microsimulation would ultimately contradict the idea of the SSRI as a decentralised 'umbrella' institute, in which the university, rather than the SSRI, hired most of the affiliates. It was thus inevitable that he would be confronted with a management problem, as staff members began to prioritise their own research. In the end, Orcutt was disappointed by his research team's loss of interest in realising microsimulation, instead researching what he considered 'tangential products'.[182]

After 1964, his frustration became even more severe after the modest progress did not convince the Ford Foundation, which later withdrew its financial support. In 1964–65, Ford's extended grant had decreased to 36,000 dollars without extension for the next year (SSRI, 1965, 25). From 1965, under the new directorship of Charles Holt, who shifted the institute's approach toward a more interdisciplinary focus, the microsimulation project was practically abandoned.[183] Afterwards, Orcutt was exhausted from fund-raising and administrative duties that had constrained the progress of microsimulation. After spending a year back at Harvard as a visiting professor during 1965–66, he was determined to resign in 1966 'with deep regret' (Orcutt, 1990, 19).[184]

In retrospect, for Orcutt, his years at Wisconsin were indeed a discouraging outcome of his dream of microsimulation, though his effort of building the SSRI was not in vain. As a side product of the microsimulation project, the SSRI group produced wide contributions to econometrics. Two popular textbooks, *Econometric Theory* by Goldberger (1964) and *Econometric Methods* by Johnston (1963), were published. Research on Bayesian econometrics proliferated, centring on the works of Box, Zellner, and George Tiao.[185] By 1968,

[181] Laura Smail, interview with Robert Lampman, 1981, transcript, Oral History Program, University Archives and Records Management Services, University of Wisconsin.
[182] Laura Smail, interview with Martin David, 1981, digital audio file, two tapes, Oral History Program, University Archives and Records Management Services, University of Wisconsin.
[183] By then, Assistant to Chancellor Barbara Newell wrote to Chancellor Robben Fleming, saying that Holt was 'very anxious' about the SSRI failing to serve its interdisciplinary task, and Orcutt 'will not have a strong role in SSRI'. Laura Smail, interview with Robert Lampman, 1981, transcript, Oral History Program, University Archives and Records Management Services, University of Wisconsin.
[184] The main triggers that pushed Orcutt to leave Wisconsin are analysed in Cheng (2020, 204–6).
[185] George Ching-hwuan Tiao (1933–) was an eminent statistician and a student of Box. He was the first graduate student to sit in on the econometrics prelim at Wisconsin (Goldberger, 1993, 231). Along with Box, he played a leading role in the development of Bayesian statistics and time-series analysis (Peña & Tsay, 2010). For a history of Bayesian econometrics, see Qin (1996).

the SSRI had accumulated 168 papers in the reprint series and 34 doctoral dissertations. Orcutt also advised his graduate students to work on the econometric analysis of micro-components spanning from the demands of durable goods, liquid assets, to earning dynamics (SSRI, n.d.).[186] Those bibliometric records suggested that the SSRI under his directorship was still a productive community; it just was not the right place for his grand project.

5. Urban Institute and the 1976 DYNASIM

On April 26, 1968, President Lyndon B. Johnson, in line with his War on Poverty, announced the launch of the Urban Institute in Washington, DC – a new think tank for the federal government to 'renew our cities and transform the lives of people' (American Presidency Project, 1968). Based on the institutional model of RAND Corporation, the aim of this non-profit, quasi-governmental cooperation concentrated on urban problems such as poverty, housing, transportation, and education and provided solutions through technical assistances (Lindsay, 1968, 1220; *Social Service Review*, 1968). William Gorham became the founding president and chief executive officer.[187] As a new RAND, the institute gathered researchers from different backgrounds, such as administrators, economists, city planners, operations analysts, architects, and engineers, to work on the scientific analysis of policy implementation in urban areas (*Political Science*, 1968, 13). Financially, the institute would receive 80% of funds from the U.S. government and 20% from the Ford Foundation (Rosoff, 1969, 20).[188] The support from the federal agencies was scheduled to be 5 million dollars for 1968 and expected to be 10 to 15 million dollars in the future (*Political Science*, 1968, 13).

On Gorham's invitation, Orcutt joined the Urban Institute in 1968. During 1969–70, he went to Yale University as a visiting professor. In the summer of 1970, he was appointed by Yale as professor of economics and A. Whitney Griswold Professor of Urban Studies, where he stayed until his retirement in 1988. Until the new microsimulation project finished in 1976, he kept equivalent work duties between Yale and the Institute in Washington.

With the massive financial injection from the federal government, Orcutt was able to continue his microanalytic modelling on the household sector at the Urban Institute – this time

[186] Orcutt's PhD advisees at Wisconsin included Maw-Lin Lee (graduated 1961), Tong Hun Lee (1961), Edward Greenberg (1961), Marshall Hall (1961), and De-Min Wu (1963). Source: Lampman (1993, 319–20).
[187] William Gorham (1930–) was a former staff member of the RAND Corporation during 1953–62, former assistant secretary of Department of Defense (1962–65) and of Department of Health, Education and Welfare (1965–68), president of the Urban Institute during 1968–2000.
[188] Leading sponsors included the Office of Economic Opportunity, the Department of Health, Education and Welfare, the National Science Foundation, and the Treasury Department.

able to form an integrated team like the Manhattan Project, which included administrators, academics, and programmers, with the aid of improved high-speed computers. The new microsimulation project began in the fall of 1969 under the codirection of Orcutt and Harold Guthrie, and Orcutt served as the only full-time project director after 1972. After his frustration at Wisconsin, Orcutt's dream finally began to receive adequate attention and funds from the government. The Urban Institute provided the perfect place to carry out his dream.[189]

After seven years, the fruits of this project was published as *Policy Exploration through Microanalytic Simulation*, which presented the Dynamic Simulation of Income Model of the United States (Orcutt, Caldwell, & Wertheimer, 1976).[190] DYNASIM was built on a PDP-10 computer at the Brookings Institution through the computer program MASH (Microanalytic Simulation of Households), designed by George Sadowsky.[191] As presented in Figure 8, DYNASIM was modelled with three programme sectors called 'MICROPASS', 'MARRIAGE UNION', and 'MACROMODEL'. These sectors were grouped into two routes: the up-down route demonstrated how population evolved through the MICROPASS and MARRIAGE UNION sector, and the left-right route accounted for the transition of aggregate economic time series by the MICROPASS and MACROMODEL sector. The former captured the simulated demographic trajectory of the total population, while the latter focused on the economic aspect of the decision-making units.

[189] Since 1968, the SRC published the first Panel Studies of Income Dynamics (PSID), which offered a more comprehensive survey on household income and was also used in the new project.

[190] Apart from the main contributors, Orcutt (main coordinator), Steven Caldwell (Chapter 3 on death and immigration; Chapters 4–5 on family formation and dissolution; Chapter 7 on geographic mobility; Chapter 12 on demographic experiments), and Richard Wertheimer II (project manager; Chapter 8 on labour sector; Chapter 14 on women-wage experiments), the project also listed Steven Franklin (Chapter 10 on income, wealth, and inheritance), Gary Hendricks (Chapter 11 on Monte Carlo variability), Gerald Peabody (Chapter 3 on birth; Chapter 6 on education; Chapter 13 on model interactions), James Smith (Chapter 10), and Sheila Zedlewski (Chapter 9 on disability and transfer payment; Chapter 15 on divorce-on-income-distribution experiments). For those contents, see the discussions later.

[191] For technical details of MASH, see Guthrie et al (1974, 126–37) and Sadowsky (1988). George Sadowsky (1936–) got his BA degree (1957) in mathematics from Harvard. As an expert in computer and programming, during 1962–65, he worked in the computer centre and economics department at Yale for a year and then joined the graduate program in economics. His interest in microsimulation was stimulated by Orcutt's 1961 book and a course at Yale on gaming and simulation taught by Martin Shubik (Sadowsky 1988, iii–iv). During his graduate years, Sadowsky became a consultant to the Office of Tax Analysis of the U.S. Treasury Department and introduced microsimulation to analyse the consequence of Revenue Act of 1964. In 1966, he went to the Brookings Institution and found the computer centre where he was the first director. From 1970 to 1973, he moved to Urban Institute as senior research staff.

FIGURE 2-1

RELATION OF MAJOR PROGRAM SECTORS TO UPDATING OF MICRO AND MACRO DATA FILES

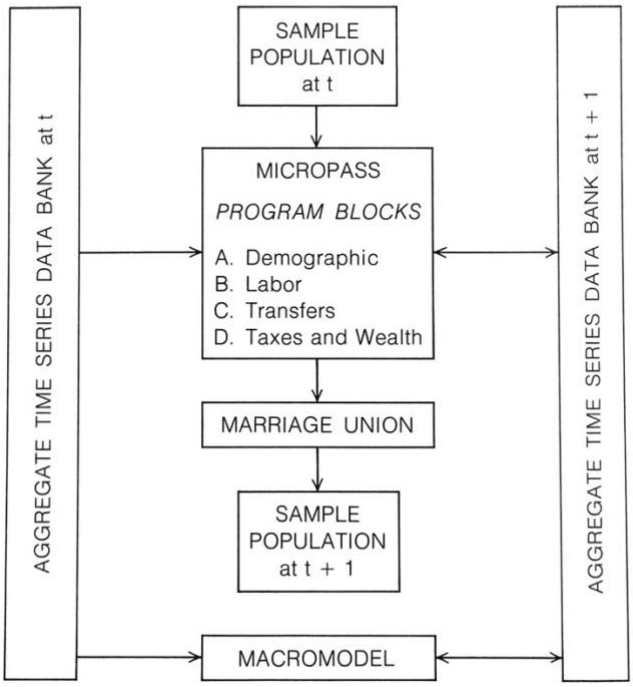

Figure 8 The sectors of DYNASIM (Orcutt, Caldwell, & Wertheimer, 1976, 28)

First, the MICROPASS sector contained four different blocks of OC, empirically specified based on the US Vital Statistics, Current Population Survey, and the Panel Study of Income Dynamics from the University of Michigan. Those were demographic (Chapters 3–7), labour (Chapter 8), transfer income (Chapter 9), and taxes and wealth (Chapter 10). The *demographic OC* considered birth, death, divorce, first marriage, and remarriage and also added migration, disability, and education. The *labour OC* included wage rates, labour force participation, working hours, the fraction of unemployed hours, and earnings. The *transfer income OC* encompassed the probability of receiving money from social transfer programmes, such as social security, pension plans, unemployment compensation, the Aid to Families with Dependent Children program, the Supplemental Security Income program, and food stamps. The *taxes and wealth OC* covered wealth income, savings, and taxation.[192]

Next, during each simulation run, the MARRIAGE UNION sector would receive the samples that were selected to marry or remarry in the MICROPASS sector; the programme would match with the sampled individual each other according to an individual ranking system

[192] Although specified, the 1976 DYNASIM did not include taxes and wealth OC.

based on race, age, education, region, and so on. Once their spouses were picked up, unmatched individuals would be thrown back into the unmarried population and joined to the next run.

Finally, as Orcutt thought the household sector 'does not operate in a vacuum' (Guthrie et al., 1974, 113), the MACROMODEL sector was an auxiliary model used to capture the macro-trends that might affect households' decisions and that the MICROPASS sector did not explain. In other words, it was a simulation environment that agents would interact with recursively, as shown in Figure 9. This macro-model sector consisted of several aggregated economic variables such as GNP, domestic investment, unemployment rate, capital stock, and capital consumption, in which the change of employment and population in MICROPASS would flow into GNP and private investment (I1), end as wealth income (WY1) and labour income (LY1), then again flow back to the micro-level. This model also assumed that the government could control the unemployment rate (U) through public policy and vary the micro-level factors. However, in the 1976 DYNASIM, the MACROMODEL sector was still developing and was not incorporated.

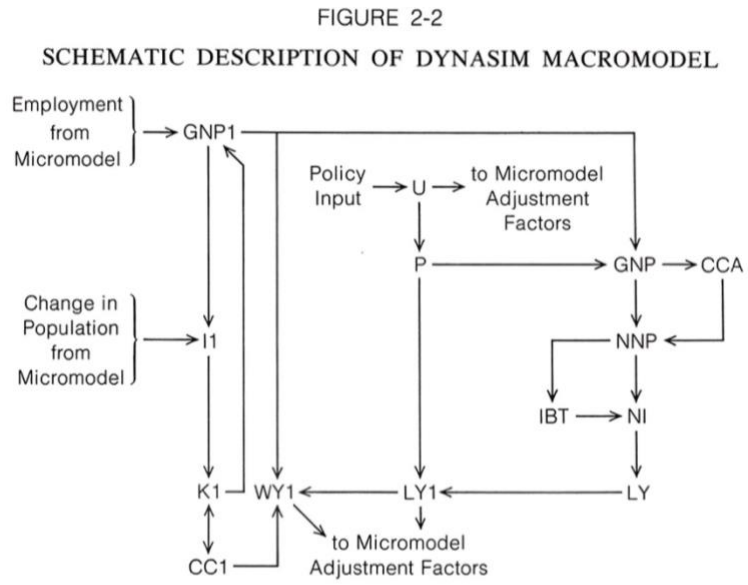

Figure 9 The structure of MACROMODEL (Orcutt, Caldwell, & Wertheimer, 1976, 37)

Based on this framework, five series of simulation experiments were performed, among which two of them concentrated on economic policy aspects.[193] One (Chapter 14) examined how different labour-policy counterfactuals could eliminate gender-wage inequality. Three experimental scenarios were applied to square women's working conditions with men's: equal pay, equal hours, and equal hours and pay. After selecting a representative sample of 4,000 from the 1960 census as an initial population, the trajectories of their incomes were simulated in a base run and then compared with three other experimental runs. The simulation results indicated that equalising the wage between men and women would reshuffle the income redistributions from single men toward female-headed families while increasing women's participation in the labour force would transfer a larger share of national income to husband-wife families (Orcutt, Caldwell, & Wertheimer, 1974, 290–318). On the other hand, chapter 15 estimated the future cost of the Aid to Families with Dependent Children (AFDC) programme. Under the settings of a 'static' and an 'accelerating' divorce rate,[194] 10,000 representative samples from the 1970 Census were used to project the population dynamics and their incomes and earnings until 1984. Since 80% of AFDC recipients were female-headed families, from those predictions of population, the future financial caseload of the AFDC programme could be aggregated. This simulation showed that in 1984, the caseload of AFDC in the 'accelerating' divorce-rate scene was estimated to be around 500,000 families larger than in the 'static' scene (Orcutt, Caldwell, & Wertheimer, 1976, 334).

6. Microsimulation as a New Source of Evidence in Policymaking

Since President Johnson's War on Poverty, vast amounts of the federal budget were scheduled for this activist proposal, which was followed by a launch series of new program evaluation agencies, for example, the Office of Economic Opportunity in 1964; the Office of Assistant Secretary for Program Coordination (later renamed Planning and Evaluation) in 1965 in the Department of Health, Education, and Welfare; and the Urban Institute in 1968. It was often the case that those programmes needed to be assessed quickly and systematically before their actual implementation so that the evidence could be used to convince Congress to pass annual budgets. As Orcutt observed, these evaluation offices were 'dominated by quantitatively

[193] Other series of experiments tested the possible errors of the Monte Carlo method (Chapter 11), and population variations when the demographic OC altered (Chapters 12 and 13).
[194] The 'static' divorce rate assumed the divorce rate was flat after 1974, and the 'accelerating' divorce rate had 5% annual increment after 1974.

trained economists, many of whom had earlier engaged in systems analysis and operations research at the Department of Defense' (Orcutt et al., 1980, 81).[195] However, when this demand first appeared, contemporary statistical analysis could not serve this task. As Alice Rivlin (1971, 64) once concluded, without a credible analytic framework for understanding individual behaviours, statistical evaluations of education programmes still provided 'discouraging' answers, as 'the analysts can provide little useful information about the relative effectiveness of various educational methods or health delivery systems'. This inadequacy thus induced a dilemma for the officials: they were encouraged to propose a reform while the consequences were mostly unknown, as described by Rivlin:

> They [administrators and social scientists] are afraid to admit that they do not know. And they may be wise. The Office of Economic Opportunity might have told Congress: "We don't know whether preschool programmes will work, or what kind would be best, but we have designed a program to find out." But would they then have gotten the money? (85)

During this period of dilemma, the microsimulation technique was almost absent in programme evaluations and remained underdeveloped because the higher-level officials who supervised the research budget were dubious about its financial viability and data accessibility. As Orcutt reflected, when he began to push microsimulation for the government agencies in the mid-1960s, even with Gorham's and Rivlin's support, it was not sufficient to overturn the upper hierarchy's conservative mind-sets (Orcutt et al., 1980, 84). Fortunately, over several years, their reluctance diminished as a result of two successful practices. First, in 1963, a microsimulation model of individual tax returns was developed by the Brookings Institution jointly with the U.S. Treasury Department for projecting tax revenue (Pechman, 1965).[196] Second, in 1968, the creation of the President's Commission on Income Maintenance Program also initiated a tax microanalytic modelling project for transfer income (Wilensky 1970; McClung 1970). The commission's tax model became the prototype of the Urban Institute's first microsimulation programme: Transfer Income Model (TRIM). Those examples convinced officials that microsimulation could be an alternative tool to produce credible and timely evidence; they subsequently used the latter model to simulate different possibilities of President Richard Nixon's Family Assistance Plan (FAP), which was presented in Congress debates (Orcutt et al., 1980, 85). Such epistemic impact was noted by Daniel Moynihan, an adviser to Nixon and previous executive secretary of the Council of Urban Affairs, who argued that the

[195] For instance, during that time William Gorham was assistant secretary for Program Coordination of the Department of Health, Education, and Welfare, and Alice Rivlin, Orcutt's former student and co-author, was deputy assistant secretary and later assistant secretary.
[196] Rivlin served as the collaborator and Sadowsky as the programming supervisor.

simulation experiments of FAP had improved the quality of evidence and facilitated Congress in reaching consensus:

> By early 1969 a simulation model had been developed which permitted various versions of FAP to be "tested" and costs to be estimated. Most of this work was done by the Urban Institute, which made its information available to all who requested it … This was a situation probably without precedent in the development of major social legislation; it disciplined and informed the debate for those in any degree disposed to restraint in the discussion of public issues. Once the President had made the proposal and congressional hearings were beginning, the Administration could in good conscience make statements about the effects it would have which never previously could have been made with any pretense to accuracy. (Moynihan, 1973, 190)

Moynihan's impression explains why microsimulation began to be perceived as a useful empirical tool in fulfilling the epistemic demands for policymaking. Through various simulation experiments, microsimulation aided officials in evaluating which redistributive policy would be beneficial for their optimum goals and which welfare programme would be financially sensible for the government before spending significant amounts. Once governments required evidence for policy evaluation, microsimulation models would, as asserted by the Urban Institute (2021), 'allow almost unlimited "what if" testing of prospective government policies'.

7. Concluding Remarks

As the answer to Orcutt's Tinbergen dream and distrust of the Haavelmo-Cowles programme, microsimulation was a totally new modelling style in aggregating household decisions that differed from the approaches of Bowley, Marschak, and Stone. From the 1970s onwards, microsimulation was widely used for making long-run forecasts of government policies and social security programmes. For instance, in the case of tax models, the TRIM2 arguably reconciled the debate over the Family Support Act of 1988, and the tax policy simulation model at the U.S. Treasury Department played a crucial role in forming the Tax Reform Act of 1986 (Citro & Hanushek, 1991, 3). Those usages were also usually combined with other models, such as TRIM2 and DYNASIM, which were applied to predict the short- and long-term run effects under the 1983 Amendments to Social Security (Michel, Storey, & Zedlewski, 1983). Among them, microsimulation constituted a unique epistemic source to reframe empirical knowledge and redirect policy implementation.

Chapter V. The Formation of Microeconometrician's Communities at Cambridge and Wisconsin: A Bibliometric Study

1. Introduction

Chapters III and IV explored the history of two academic institutions in advancing microeconometrics: Richard Stone's Department of Applied Economics (DAE) at the University of Cambridge and Guy Orcutt's Social Science Research Institute (SSRI) at the University of Wisconsin. Stone and his crew of microeconometricians was the first group attempt in the field that used British household budget surveys to conduct demand analysis. As a side product of Orcutt's microsimulation project, the SSRI econometricians analysed household behaviours with the aid of consumer finance surveys. Although harbouring different goals, these two institutions both contributed to the formation of microeconometric practices during the 1950s and 1960s.

However, while the last two chapters revealed the qualitative histories of the DAE and SSRI, two empirical issues are still unanswered. First, previous historical evidence has implied that different institutional setups and managerial styles will result in different outcomes. As such, many of his affiliates praised Stone as an inclusive, charismatic, and inspiring director, while SSRI's affiliates under Orcutt gradually shifted away from his dream of microsimulation. Nevertheless, in verifying this Stone-Orcutt contrast, there is still no empirical evidence on the interpersonal relationships within both communities. Second, the previous chapters examined the microeconometric practices of the DAE and SSRI as aggregated outcomes, but they did not expose how those practices were transmitted as exemplars within the community. In other words, overemphasis on the macro-developments of the microeconometric literature precludes the possibility of examining their formations from a microdynamic point of view.

This chapter assesses the DAE and SSRI on their academic relationships and citation patterns using bibliometric data in addressing those empirical concerns. The research questions are twofold: How close were those econometricians in the community under different directorships, and will the empirical finding support the Stone-Orcutt contrast? How did the

microeconometric exemplars of the DAE and SSRI emerge and transmit from a micro-level perspective? By integrating both aspects of evidence, this chapter shows how both communities of microeconometricians were formed and developed through interpersonal and citation networks.

Studying the role of interpersonal relationship and the citation patterns of practices within scientific communities will benefit two theoretical concerns in the history and sociology of science, respectively. First, sociologists have been interested in studying personal relationship networks. For instance, economic sociologist Mark Granovetter (1973) showed the potential of studying social networks by proposing his strength-of-weak-ties theory. Granovetter derived a deeper implication from personal networks, which predicted that people usually benefit from those who have weaker connections to them. As such, a weak tie in a social network had the advantage of facilitating personal communications and information transmissions. Historians of economics also share a similar interest in personal relationships. However, it is sometimes limited to personal gossip in article footnotes and seminar discussions. While admitting personal relationships is indeed one of the crucial ties, this chapter reframes this concept as an *academic relationship*, which is a mixture of the personal bond and intellectual embeddedness between individuals. Identifying such requires both information from personal histories and how individuals intellectually engaged with each other in the community. Second, as introduced in Chapter I, the model of empirical knowledge production contains some practices that become Kuhnian exemplars and are diffused in the community as personal triggers. This account also makes the assumption that practices are transformed into exemplars once they are cited. In that regard, the citation data is a crucial source in identifying exemplars and additional evidence for the accumulation process of empirical knowledge.

Emerging from the late 1960s, bibliometric studies in the history of economics were still few. As one of the earliest attempts, Holt and Schrank (1968) estimated the size and growth of professional literature in economics. Lovell (1973) obtained the production function of economic knowledge under the Cobb-Douglass specification and sampled 99 top economics journal articles published in 1965 to investigate their citation pattern. Stigler and Friedland (1975) studied the citation pattern of doctorates in economics during 1950–55 from six prestigious institutions. They further categorised top-cited ‘authorities’ and assessed their diverse impact on the citation behaviours of doctorates. Using a similar methodology, Stigler and Friedland (1979) revealed the long-standing pattern of citing authorities in economics between 1886–1968 based on random samples from the Index of Economic Journals. Using the same dataset, Bordo and Landau (1979) extracted articles in economic theory from 1945 to

1968 and listed the mostly cited economists during the post-war era. After 1990, citation analysis in the history of economics gradually shifted from the big-picture questions to the intellectual impact of individuals and the development of subdisciplines, for instance, the general equilibrium theory (Oehler, 1990), Wesley Mitchell (Biddle, 1996), Adam Smith (Wight, 2002), Zvi Griliches (Diamond, 2004), Frank Ransey (Duarte, 2009), the Haavelmo-Cowles programme (Qin, 2013, Chapter 8), and Haavelmo (Hoover, 2014). These works still adopted a closer methodology to their 1970s precursors but aimed at more specific historical contexts.

From the 2000s onwards, economists began to study social networks to understand their empirical relevance to economic theory.[197] In the past few years, historians of economics have also included network analysis as a practical toolkit (Claveau & Herfeld, 2018). The pioneering article in this area was Gingras and Schinckus (2012). They analysed the citation and co-citation pattern of journal articles in econophysics and visualised a co-citation graph in locating the position of econophysics among journals in physics, finance, and economics. Claveau and Gingras (2016) identified a substantial dataset of economics documents from 1956 to 2015 to explore the macrodynamics of academic publications in economics. The study also applied the text mining technique and then mapped the dynamics of the most common keywords appearing in their samples.[198] Wei (2019) visualised the geographical co-authorship and co-citation network of the top-five economics journal articles from 2012–16, from which the author also reported the most common keywords through the results of text mining. Aside from the macro-trend studies, network analysis was also used extensively in investigating specific subcommunities by exposing the interaction between scholars and research paradigms, for instance, the Austrian School in the Vienna Circle (Wright, 2016), the co-authorship networks of public choice theory (Farvaque and Gannon, 2018), the relationship between behavioural economics and psychology (Braesemann, 2019), the diffusion of rational choice theory (Herfeld and Doehne, 2019), and the co-citation networks of the vector autoregression model (Salazar and Otero, 2019).

This chapter contributes to the literature reviewed above and applies two empirical frameworks to the analysis of scientific communities. First, bibliometric data from the institutional reprint series and the network analysis technique were used to measure the

[197] For an overview of its applications in economics, see Jackson (2011).
[198] Similar method of text mining can be found in Ambrosino et al. (2018) and in Edwards (2020). The former applies a specific topic-modelling algorithm in searching for keywords, and the latter takes the article in *History of Political Economy* as its unit of analysis.

academic relationships between economists in both communities. Three networks of collaboration, bibliometric coupling, and acknowledgement were visualised, and their measurements were calculated and decomposed. Second, using the microeconometric subsamples from the same dataset, the co-citation analysis was applied to trace notable exemplars that carry the microeconometric knowledge to the scientific communities. Then, the framework of listening and talking citations was developed to analyse the transmissions of these exemplars.

The chapter is arranged as follows. Section 2 describes the bibliometric data and network algorisms used. Section 3 shows the results of network analysis by presenting three different networks of both communities. Section 4 discusses the interpretations of the Stone-Orcutt contrast with the strength-of-weak-tie theory and network measurements. Section 5 applies the co-citation analysis to identify crucial exemplars in the formation of both microeconometrics. Section 6 evaluates the contributions of these exemplars. Section 7 provides some concluding remarks.

2. Data

There are two ways to define the literature of the DAE and SSRI. The first would be collecting every published source produced by their affiliates, which would lead to an exhaustive collection of personal contribution but might suffer from a misidentification problem: many productive scholars' works are independent of their institutions which may be finished without institutional affiliation. This fact would inflate the impact of research institutions when personal contributions are overestimated. The second approach would be to take the reprint series of each institution in its annual reports as selective samples, which is a more balanced treatment for two reasons. First, many reprint series are previous working papers directly produced by its affiliates and were usually presented in its institutional seminars. It is thus reasonable to assume that the reprint series represents the more instinctive outputs of the community. Second, the reprint series is widely recognised as credits of that institution and are available upon requests. In this sense, any institution tends to include its representative products while compiling its reprint series.

The DAE sample was defined as its reprint series during Stone's directorship (1946–55) that are digitised from four DAE departmental reports for 1946–48, 1948–51, 1951–53, and 1954–57 (DAE 1948; 1951; 1954; 1958). There were 132 English documents, including 121

research articles,[199] four book chapters, three survey articles, two unpublished conference proceedings,[200] one note, and one news column. The bibliometric analysis focused on research articles in economics and statistics. Therefore, 21 research articles from *Bulletin of the London and Cambridge Economic Service* (13) and *Accounting Research* (8) were excluded.[201] The final DAE sample contained 100 journal articles written by 42 authors, where 87 samples had citation records either in Web of Science or Scopus.[202] The SSRI sample collected all the reprint series, which fairly covered Orcutt's directorship (1959–66). The complete list of 182 English sources between 1961 and 1968 were reported in SSRI (n.d.). Excluding 16 book chapters, 11 conference proceedings, one note, one reply, one meeting abstract, and one encyclopaedia entry, there were 151 research articles from a range of fields, including economics, statistics, sociology, political science, and demography. The final sample had 131 journal articles in economics and statistics by 73 authors,[203] and 127 of the articles had citation records. The numbers of both reprint series by years are shown in Figure 10.

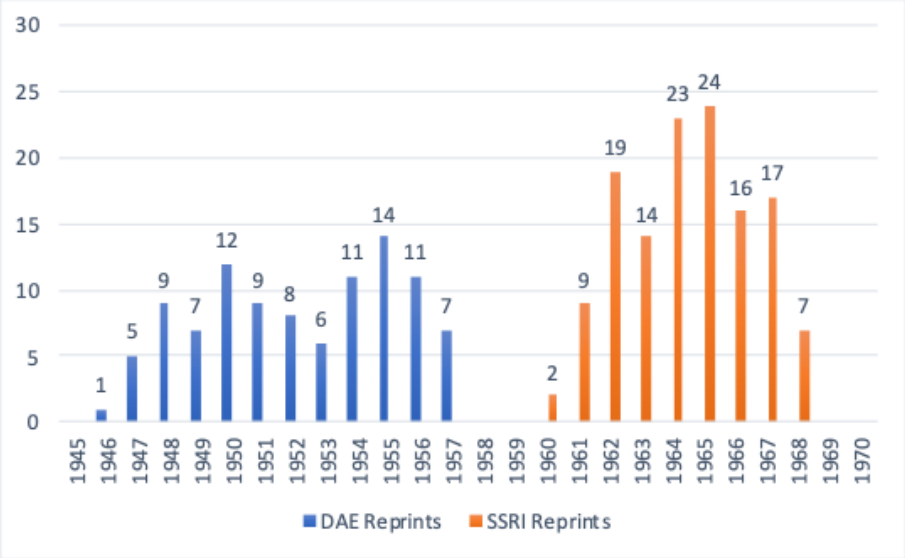

Figure 10 Numbers of the DAE and SSRI reprint series by year

[199] Seven non-English-language documents were excluded since they cannot be analysed due to the current author's limited language ability.
[200] Another conference proceeding that was published later is included as a journal article.
[201] Most DAE articles from the *Bulletin* reports British economic indices without further economic analysis. For the history of the journal, see Cord (2017). The *Accounting Research* is an accounting journal edited by a DAE affiliate Sewell Bray. Judging from the titles of its articles, the primary research concern of them is not about economic analysis but more about accounting methodology.
[202] The book chapters were excluded since they do not have citation records and some of the chapters are not accessible.
[203] Including five inaccessible and 131 accessible articles.

All references in both samples were compiled under three criteria: first, if cited conference proceedings and working papers are published later in the same title, that reference was coded to its future published item. Second, the upcoming or to-be-published reference was also changed to the future published item. Last, different book editions were treated as separate items, for example, Paul Samuelson's *Foundation of Economic Analysis* and John Hicks's *Value and Capital*. This treatment might bias the network measurements, but textbook citations only occupied a small number of total references.

Table 2 Summary statistics of the DAE and the SSRI reprint series

	DAE (1946–1957)	SSRI (1961-1968)
(a) Authors and Collaborations		
No. of documents	100	131
Single-author documents	74	92
No. of authors	42	73
Documents per author	2.38	1.79
Co-authors per document	1.29	1.33
(b) References		
No. of references	1,039	1,321
References per document	10.4	10.11
No. of times cited*	15,413 from 87 documents	9,448 from 127 documents
Average years published from 2020	67.82	55.61
No. of times cited per year**	227.28	169.91
(b) Publications		
No. of journals	33	40
Top-five journals (no.)	***Journal Royal Statical Society*** (21); ***Economic Journal*** (15); ***Biometrika*** (8); *Econometrica* (8); ***Review of Economic Studies*** (8)	*Journal of American Statistical Association* (15); *Review of Economics and Statistics* (13); *Econometrica* (11); *American Economic Review* (11); *Quarterly Journal of Economics* (9)

* Until Oct 2020. Thirteen of the DAE documents and four of the SSRI documents do not contain citation records from the Web of Science and Scopus.
** Calculated by total citations/average years published from 2020.
Note: British journals are marked as bold.

Table 2 shows the summary statistics. On the authorship, the SSRI has more authors than the DAE, but the DAE authors are more productive on average. Their co-authorship patterns are very similar, with an average of between 1.29 to 1.32 co-authors per document. This similarity can also be found in average references per document, which is around ten references per document. In regard to their annual citations, the DAE series averaged 227.28 citations per year since published, outperforming the SSRI's 169.91, suggesting that the DAE literature may

be more influential.[204] Most articles were published in top economics and statistics journals and highly concentrated in national journals. The top-five journals show that the DAE series tend to publish in British journals, except for *Econometrica*, and most of the SSRI series have appeared in U.S. journals.

Based on the samples above, the subsamples of microeconometrics were identified if the article focuses on microdata's econometric applications or methodological discussions. Table 3 provides the summary statistics. Twenty-eight of the DAE series and 35 of the SSRI series were classified as microeconometrics, including 234 and 412 references and 1558 and 4712 times referenced. Both distributions of times being referenced are highly unequal. The top-1 and top-5 referenced documents from both institutions constitute at least 60 and 86% of the total citations, respectively.

Table 3 Summary statistics of the DAE's and SSRI's microeconometrics

	DAE	SSRI
No. of documents	28	35
No. of references	233	411
No. of times cited*	1,558 from 23 documents	4,712 from 35 documents
Top-5 cited documents (no.)*	Roy, 1951 (947) Aitchison, 1955 (199) Tobin, 1950 (81) Houthakker, 1951 (65) Roy, 1950 (63)	Zellner, 1962 (3692) Hall and Weiss, 1967 (256) Weiss, 1966 (189) Zellner and Lee, 1965 (105) Larner, 1966 (70)
Top-1/All referenced	0.6078	0.7835
Top-5/All referenced	0.8697	0.9151

* Until Oct 2020

3. Evaluating the Strength of Academic Networks

3.1 Definition and Methodology

The definition of ties draws upon Mark Granovetter's (1973, 1361) concept of the strength of ties, which is elaborated as 'a (probably linear) combination of the amount of time, the emotional intensity, the intimacy (mutual confiding), and the reciprocal services which characterize the tie'. What Granovetter captures are interpersonal ties which are built on personal interactions when people meet each other. However, this concept alone fails to

[204] In this sense, both the calculations have accounted the fact that the DAE literature has more times than the SSRI in accumulating citations.

account for how people interact in academic communities since they also communicate through publications. For instance, scientists may cite a person they have never met and still see that person as an intellectual bond. In this sense, the concept of academic relationship is beyond personal communications that require other theoretical entities.

To describe the academic relationships in the community, the strength of academic ties as a broader concept was used, including *interpersonal* and *intellectual* ties. Define the strength of interpersonal ties S_p, and intellectual ties S_i, the strength of academic ties S_a can be written under a community structure $f(.)$,

$$S_a = f(S_p, S_i)$$

The former captures the personal connections of each scholar in Granovetter's sense, and the latter characterises the intellectual influence, which is not necessarily relevant to personal interactions. In other words, an intellectual tie presents a scholar's embeddedness to a research exemplar that is not shaped by interpersonal factors. This definition allows the possibility that a scholar would engage in a typical way of thinking without physically meeting anyone from that intellectual strand.

To assess S_p and S_i, four relationship factors are potential candidates for empirical studies: *co-authorship*, *bibliometric coupling*, *acknowledgement*, and *supervision*. These factors are informative once the scope of research is limited to only relationships within the community. First, co-authorships reveal how people in the scientific community collaborate, which assumes that when people coworking on a project would spend more time on face-to-face interactions, they would have a more robust intellectual connection. The assumption was more probable back in the earlier years when remote collaborations were less feasible. Second, bibliometric coupling represents the strength when two works have common references.[205] The assumption behind this is also intuitive: if people in a group tend to cite similar papers, they would be intellectually closer or at least have an affinity with disciplinary exemplars. Indeed, between scientific communities, their bibliometric couplings could be misleading. For instance, the Chicago School and Marxists would cite *The Wealth of Nations* for distinctive reasons. However, in a setup of the research institution, the bibliometric coupling is more likely to represent intellectual closeness while research institutions, unlike university departments, have fewer incentives to recruit their intellectual opponents.

Third, acknowledgements can be extracted from each author's note in the article whenever they acknowledged a person. The language could vary, such as 'in debt', 'thank', 'benefit',

[205] This concept was first proposed by Kessler (1963) for studying the coupling network of the *Physics Review*.

'assistance', and 'gratitude'. Sending acknowledgement to another person has always been considered proper scientific manner when that person provided positive feedback to the published work. A potential threat to the validity of acknowledgement data is that some economists would strategically avoid thanking people for a more friendly referee (Hamermesh, 1992, 171). This opportunistic behaviour might be true when assessing acknowledgements outside the community, but not for the internal relationships since journal editors tend to avoid selecting the author's colleagues as referees. Lastly, supervision is one of the most robust interpersonal connections in academia since supervisors meet their students frequently and involve them in academic discussions. For students, their supervisors are essential signals of their personal connections and intellectual origins. In most cases, supervisors also play roles in their career placements.

With the rationales above, defining the strength of collaboration ties as S_c, of bibliometric coupling ties as S_{bc}, of acknowledgement ties as S_{ack} and of supervision ties as S_{adv}, within the same community structure, S_p and S_i will satisfy:

$$S_p = f(S_c, S_{ack}, S_{adv})$$
$$S_i = f(S_c, S_{bc}, S_{adv})$$

The relationships above suggest that acknowledgement and bibliometric coupling only account for the strength of the interpersonal and intellectual ties, respectively. Both collaboration and supervision ties are distinctive illustrations of academic ties. Taking all relationships into account, S_a can be rewritten as,

$$S_a = f(S_c, S_{bc}, S_{ack}, S_{adv})$$

The interpretation for S_a is intuitive. The strength of academic ties is a combination of how scientists in a single community acknowledge, collaborate, and supervise interactively and their tendencies in citing similar references. However, it should be noted that the functional form of S_a does not suggest any econometric structures. As these variables are unquestionably interdependent, there still lacks an identification strategy legitimising the structural stability of $f(.)$ and the orthogonality of each variable in S_a.

Given such methodological limitation, what can be done instead is looking for an indexed measurement reporting the strength of these ties. Define the strength-of-x-tie index as I_x, the strength-academic-tie index I_a could be presented as the geometric means of other four normalised indexes:

$$I_a = \sqrt[4]{I_c I_{bc} I_{ack} I_{adv}}$$

I_c: the strength-of-collaboration-tie index

I_{bc}: the strength-of-bibliometric-coupling-tie index
I_{ack}: the strength-of-acknowledgement-tie index
I_{adv}: the strength-of-supervision-tie index

Note that the purpose of I_a is to obtain a standardised measure for systematic comparisons without any theory involved. In other words, I_a is interested in looking for an unweighted method approximating the extent of those relationships with an index number. Similar treatment of geometric means is found in Irving Fisher's measurement of 'ideal' price index, which synthesises the price indices developed by Lasperyes and Paasche.

3.2 Network Visualisations

The results of network visualisation are presented in the order of collaboration (Figure 11), bibliometric coupling (Figure 12), and acknowledgement (Figure 13).[206] All the codes were run under R and RStudio (R Core Team, 2020; RStudio Team, 2020). The first two network objects were produced by the R package *Bibilometrix* (Aria & Cuccurullo, 2017), and the last were transformed from the weighted adjacency matrixes using *igraph* (Csardi & Nepusz, 2006). All graphs are visualised by *tidygraph* and *ggraph* (Pedersen, 2020; 2021) under the Fruchterman-Reingold layout (Fruchterman & Reingold, 1991). The Louvain clustering algorithm (Blondel et al. 2008) was applied to detect the communities of bibliometric coupling.

First, the Fruchterman-Reingold layout is a force-directed network, which ensures that the nodes and vertexes do not in general overlap by treating the network as a balanced physical system. It is important to note that the layout is only designed for aesthetic purposes, and the positions of nodes and the lengths of edges *do not imply any meaningful interpretations*. Second, the Louvain clustering method is one of the most common methods in community detection. The idea is to maximise the network's modularity, which is defined as the difference between the fraction of the edges inside a group and the other possible edges outside (Brandes et al., 2008).[207] As solving modularity optimisation is complicated, the Louvain method may not be the most reliable, and various alternatives have been developed based on different assumptions of network properties, such as the Leiden method (Traag, Waltman, & Van Eck, 2019). Due to space constraints, the analysis here will not explore all the possible community

[206] The supervision network is excluded because it contains too few observations to form a network.
[207] The Louvain method has two phases. The first assigns each node to a community, and the second removes the node to another community to increase the modularity. The second phase stops when reaching the modularity's local maximum, and the first will be repeated until reaching the global maximum.

detection methods. Instead, the technical evidence will only be used in comparison with the qualitative evidence from Chapters III and IV.

The collaboration networks of the DAE and SSRI reprint series are presented in Figure 11. What each node presents are authors. The link between nodes is established whenever two scholars have co-authored one article. For instance, Cochrane and Orcutt in the DAE have co-authored two articles, while Zellner and Tiao in the SSRI have three. The size of the node is proportional to its degree, measured from the summative score under three criteria: (1) a single-author article gets two, (2) a two-author article gets three, and (3) a three-author article gets four. For instance, Orcutt in the DAE that has two single-author and three two-author articles will lead him to 13 degrees. For the sake of readability, authors with only one single-author article are not labelled.

For the DAE, Stone is the unique centre of its network where he has co-authored with eight of his colleagues once and ends up the highest degree (38) among the DAE. Extensive co-authorships between microeconometricians are clustered between Tobin, Houthakker, Prais, Brown, and Aitchison, especially the last two have four ties in-between. Stone serves as the only link between regional surveyors (Utting-Cole) and time-series econometricians (Durbin-Watson); each group has established three internal ties. The Cochrane-Orcutt-James-Stuvel group is also early time-series econometricians. Apart from these, DAE's productive single authors span from economic historians (Deane and Maywald), microeconometricians (Roy and Farrell) to macroeconometricians (Geary, Prest, and Tintner). Without building any collaboration tie, these authors produce only single-author papers.

SSRI's collaboration network contains more diverse groups with their core. Most microeconometricians (Orcutt, Korbel, Lee ML, Fisher JA, Huang, Lee TH) appear in the network, but their collaboration ties are weak, especially Orcutt does not form a cluster. As the most productive author, Zellner is the collaboration centre of the largest groups; he has built seven collaboration ties and ends up at the same degree as Stone. Meanwhile, Zellner is also the unique link between econometricians (*e.g.*, Theil, Huang, and Lee TH) and Bayesian statisticians (e.g., Tiao and Box). The second largest group is labour economists centring on Weisbrod that has connected five nodes, and the next are two small working groups of econometricians on Goldberger and industrial economists on Earley. The other nine groups have two and three nodes.

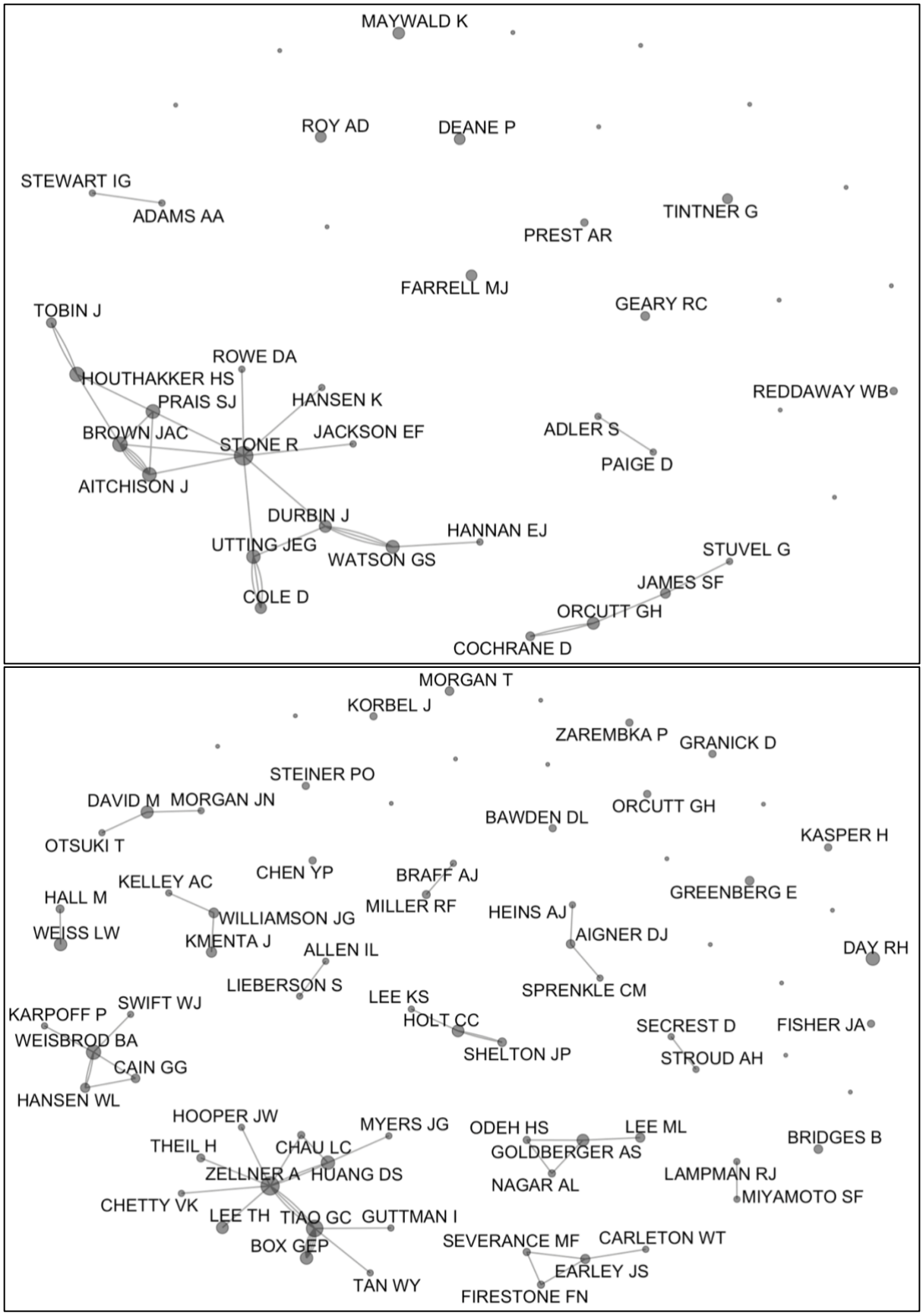

Figure 11 Collaboration networks of reprint series of the DAE (up) and the SSRI (down). Note: The size of nodes is proportional to their number of degrees.

Comparing the DAE and SSRI with their collaboration networks (top and bottom of Figure 11), some observations can be summarised. First, the centres of each network are Stone and Zellner, both of whom are connecting the most authors. Second, there are two co-authorship clusters in the DAE, while the SSRI has multiple groups of nodes with more diverse research focuses. Last, Orcutt's impact on collaborations is seemingly minor.

Figure 12 demonstrates the bibliometric coupling networks of the DAE and SSRI. Similar to the collaboration, the nodes present as authors, and each link is built while their citations are matched. Increases in the width of vertexes mean more citations in common. For instance, in the DAE network, the only edge between Deane and Reddaway shows that they have one matched citation; and the dense edge between Stone and Brown conveys that they tend to reference similar works. The node size is proportional to its degree, measured by the number of times that citations are matched with other articles. Since it is impossible to show all the nodes and links in one graph, only the authors with top-35 degrees in their network are shown. The Louvain algorithm was applied to test both the community structures under *Bibliometrix*, and the detected groups were plotted with *ggraph*. People in the same community are filled with the same colour.

Two coupling networks of the DAE and SSRI demonstrates that distinctive citation habits exist between them. Judging from the density of edges, the DAE authors reference similar items, while Stone's citations are prevalent. Conversely, except for the Bayesian econometricians (Zellner, Tiao, and Box), who form a strong coupling network, the SSRI's bibliometric connections within other groups are much weaker than the DAE's.

First, the DAE coupling network shows a similar pattern to its collaborations. Stone is the centre where he shares many commonalities with his colleagues in terms of citations. He is in the blue group with regional surveyors (Utting and Cole), economic historians (Deane, Maywald, Buckatzsch), and some others from diverse fields (Rowe, Paige, Adler, Hansen, Reddaway, Brumberg, and Strotz). This fact indicates that his citations cover a wide range of research themes. Apart from the blue group, with some occasional misses, the communities identified through the Louvain method are generally correct. The red group identifies microeconometricians with the exception of Bergstrom, Pfouts, and Briggs. Time-series econometricians are divided into two groups, where the pink group has Durbin, Watson, and Hannan, and the brown has James, Orcutt, Cochrane, Tintner, Geary, Prest, Jackson, and Anderson. The only exception is Roy, who was then mainly working on his self-sampled microdata.

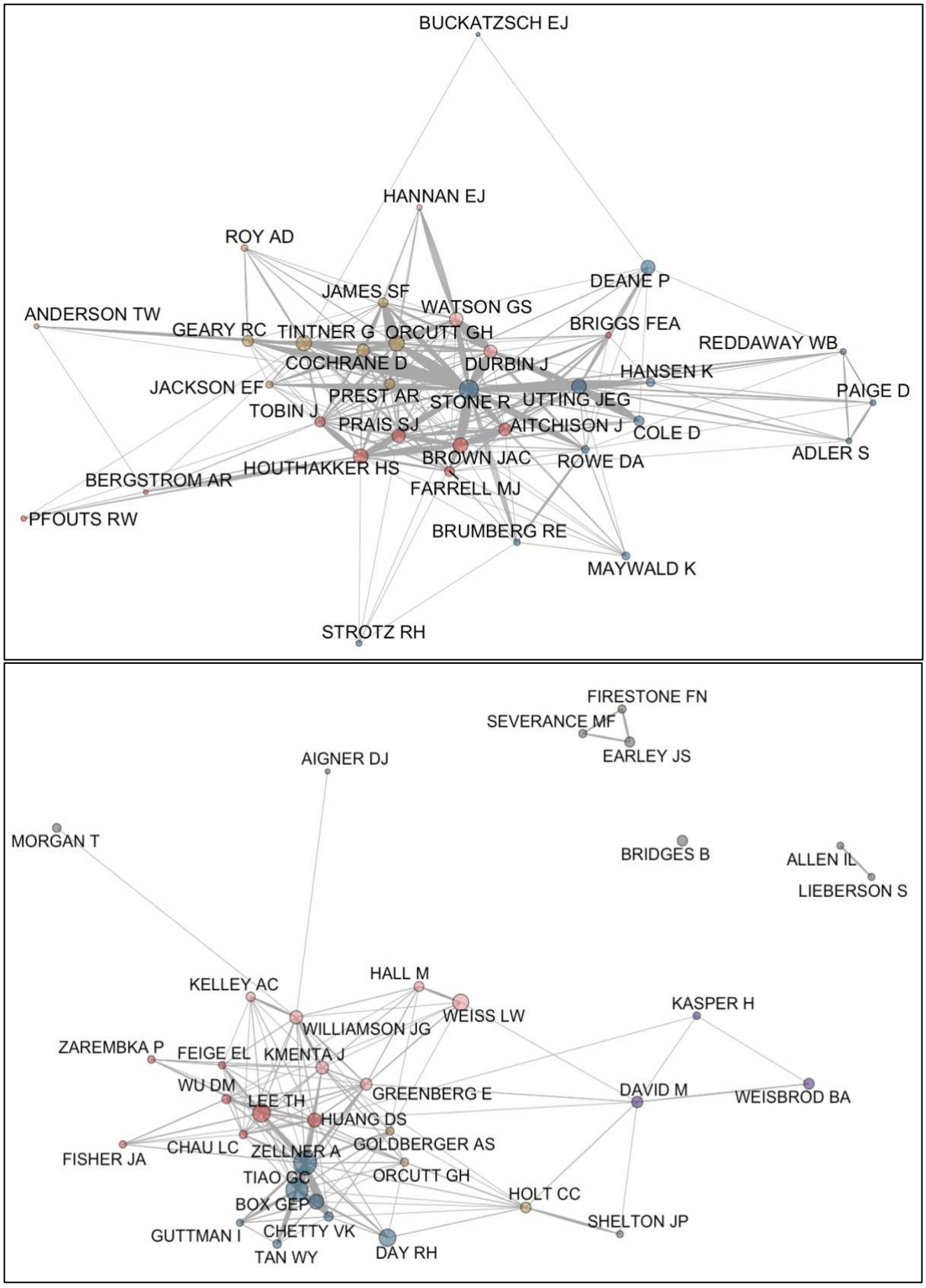

Figure 12 Coupling networks of reprint series of the DAE (up, node = 34) and the SSRI (down, node = 35). Note: The node's size is proportional to its number of degrees.

Second, the SSRI coupling network has diverse groups, and their coupling ties are much generally less dense than the DAE. The Zellner-Tiao-Box group has the most notable connections. Their group mainly captures other Bayesian econometricians (Chetty, Tan, and Guttman) except Day. The red group has identified the most microeconometricians (Huang, Wu, Lee ML, and Fisher), and Orcutt is coupled with Goldberger and Holt in the yellow group. The pink group includes labour economists (Hall and Weiss) and others (Greenburg, Kelly, Williamson, and Kmenta) in various fields. The classification of the purple group is based on only eight linkages and the other three isolated groups on the top-right of the graph. The appearance of the three isolated groups in the SSRI network verifies that its bibliometric coupling is very weak. The Earley-Firestone-Severance and Allen-Lieberson links have published only one three-author and two two-author articles, and these citation matches are sufficient for them to become the top-35 coupling degrees. This situation also happens to Bridges, who has three sole-author articles with no connections to other publications.

Figure 13 presents the directed acknowledgement networks. The data was collected from all the DAE and SSRI samples that only extract the information relevant to the authors who appeared in the samples. The acknowledgement will be equally shared in the co-authored pieces. The node's size is proportional to its degree, which is the number of acknowledgement links built on that node. The increasing darkness of the edges identifies the direction of reciprocal acknowledgements. For instance, Roy in the DAE has sent three acknowledgements to Carter and Prest, one to Stone, and one from Bergstrom. Nodes with less than one acknowledgement are not labelled.

The DAE's network is centred on Stone, who received the most acknowledgements out of the group and acted as a unique bridge between the right-hand group (Orcutt, Prest, Roy, Bergstrom, and Carter) and the rest. Mutual acknowledgements between microeconometricians (Tobin, Prais, Houthakker, Brown, Aitchison, and Farrell) formed the largest group in the network. Moreover, the person with the highest degrees in the SSRI network is Goldberger, who creates a triangle with Orcutt and Zellner, surrounded by microeconometricians (Lee ML, Lee TH, Fisher, and Wu). Outside the triangle, the SSRI tends to form diverse groups of individual acknowledgements, which are pretty evenly distributed. These clusters suggest that some group communications in other subdisciplines were developed at the SSRI but in a less intensive way. In this sense, both networks of the DAE and SSRI show that the microeconometricians there worked as research groups and benefited from each other's mutual advice. This positive peer effect seems to be weaker in other groups of economists.

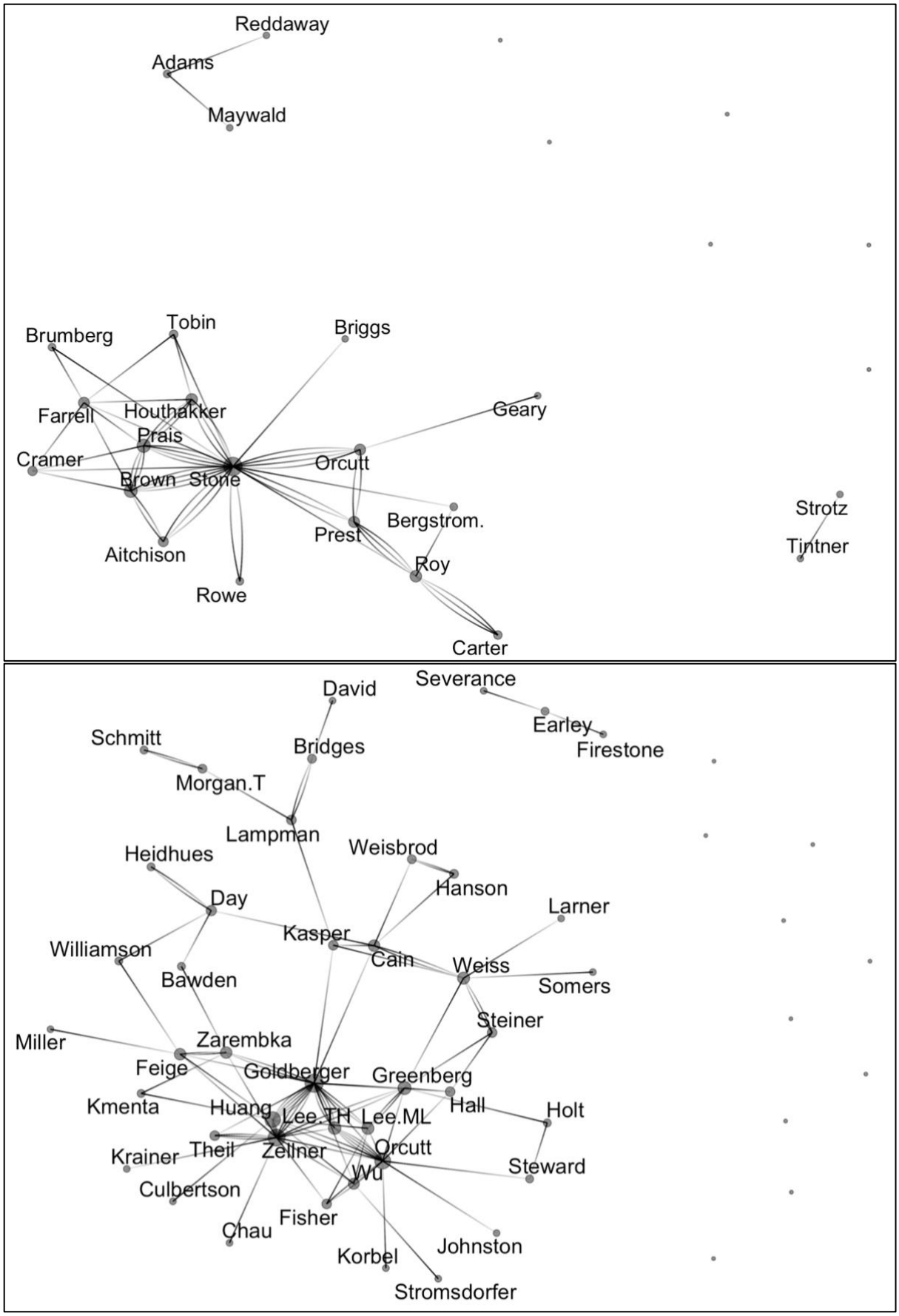

Figure 13 Acknowledgement networks of reprint series of the DAE (up) and the SSRI (down). Note: The node's size is proportional to its number of degrees.

Figures 11–13 suggest that the DAE and SSRI were distinct types of research institutions in the sense of collective efforts. The visualisations show that the DAE was a research institution that focused on econometrics. While intense collaborations, and bibliometric couplings, and acknowledgements were found between DAE econometricians, other affiliates were relatively independent. The contrast between econometricians and others demonstrated that the DAE was a strong econometrics collective, echoing Angus Deaton's evaluation that the contributions of the DAE were analogous to those of the Cowles Commission (Section 6, Chapter III). Comparing to the DAE, the institutional setting of the SSRI was more akin to a university department that allocated people from various subdisciplines. In the SSRI's collaboration network (Figure 11, down), apart from the collective centred on Zellner, there are diverse collectives working on their separate tasks. This community structure is again found in the acknowledgement network, where their acknowledgements are visually less centralised than those of the DAE. Evidence from the visualisations confirms that the SSRI was indeed an 'umbrella' institute for social sciences, (Section 4, Chapter IV).

4. Assessing the Stone-Orcutt Contrast

4.1 Interpretations of the Network Measurements

The network graphs presented above only show both institutions' clustering relationships of affiliates but not the spatial positions. Therefore, interpretations based on the visualisations of interpersonal networks can only be limited to identifying groups and their degrees. To derive deeper implications, computing network measurements are necessary. Three measurements in network analysis are commonly used: degree centrality, closeness centrality, and betweenness centrality. Degree centrality measures the number of links shared by a node divided by its highest possible number of links. A node with a higher degree of centrality is more capable of building connections. Closeness centrality indicates the average shortest length of one node travelling to all other nodes, meaning that the node with the higher closeness centrality is more likely to cluster with all other nodes. Betweenness centrality indicates how the node frequently acts as the shortest route between two different nodes. Thus, the node has a greater tendency to act as a local bridge as its betweenness centrality increases.

These three measurements are informative evidence for exploring the extent to which the DAE and SSRI people are linked in the sense of building interpersonal and intellectual

connections (degree centrality), clustering abilities (closeness centrality), and bridging other people (betweenness centrality). From these measurements, one can infer the roles that each node plays in the network and correspond to the people with different aspects of daily life. For instance, a node with a high degree but low closeness centrality is evidence that a person is good at making connections but finds it difficult to work closely with others; a person with few connections who mediates information efficiently has a low degree but high betweenness centrality, and so on.

Table 4 Aggregate centrality measurements of the DAE and the SSRI networks

	DAE	SSRI
(a) Collaboration		
degree	0.168	0.083
closeness	0.014	0.004
betweenness	0.068	0.018
(b) Coupling		
degree	0.521	0.281
closeness	0.088	0.020
betweenness	0.161	0.086
(c) Acknowledgement		
in-degree	0.351	0.202
closeness	0.042	0.030
betweenness	0.254	0.204
The degree-of-academic-tie index*	0.313	0.168
The closeness-of-academic-tie index*	0.037	0.013
The betweenness-of-academic-tie index*	0.141	0.068

* Calculates from the geometric mean of each measurement from (a), (b), and (c)

The aggregate centrality measurements of both networks are shown in Table 4, where all the strength-of-academic-tie indexes are calculated.[208] Since the acknowledgement network is a directed network, there are three ways to measure its degree centrality: in-degree, out-degree, and total-degree.[209] The table reports only the in-degree centrality to capture the intensity of how people are being thanked instead of thanking others. In general, the DAE networks show higher centrality than the SSRI in collaboration, coupling, and acknowledgement. Aggregate network measurements hinted that the DAE is a more interpersonally connected and intellectually embedded network than the SSRI. With regards to degree centrality, the DAE (0.313) has built averagely around twice connections to the SSRI (0.168), indicating that people

[208] Due to limitations of biographical data, the supervision-tie cannot be measured here.
[209] In-degree centrality takes a node's numbers of edges are directed by other nodes as the degree measurement, out-degree centrality takes the other way round and the total-degree centrality takes the sum of two.

in the DAE are better-connected in general. This fact also reflects on DAE's higher closeness and betweenness centrality, suggesting that the DAE requires shorter paths to reach other nodes, and the DAE contains local bridges that communicate efficiently among the nodes, respectively.

These aggregate measurements can be further decomposed to examine their individual contributions. Tables 5 and 6 summarise the normalised centrality scores of every individual in both the DAE and SSRI networks. Denote that $x = (c, bc, ack)$ refers to three types of networks of collaboration, bibliometric coupling, and acknowledgement, and $y = (deg, close, betw)$ three types of centrality measurement of degree, closeness, and betweenness. Let $f(i, x, y)$ be the centrality measurement of individual i, score $M(i, x, y)$ is defined as a linear scale-transformation of $f(i, x, y)$. For each combination of x and y, the individual score is defined as $M(i) = 100 * \{f(i)/\max[f(i)]\}$. The calculation procedures were presented as follows. First, calculate the $f(x, y)$ for every individual. For instance, Stone and Prais in the DAE collaboration network has the degree centrality of 0.195 and 0.0975, respectively, meaning that:

$$f(Stone, c, deg) = 0.195$$
$$f(Prais, c, deg) = 0.0975$$

The procedure leads to nine columns of measurements for every people in two communities. Second, use these measurements to obtain $M(i, x, y)$ by taking the highest number of each column as 100. For instance, in Table 5, Stone's 0.195 in the DAE collaboration network is the highest. His number would be then taken as 100, indicating that Prais's original degree centrality will be rescaled to 50. The tables can be interpreted in such a way: each column refers to a scoring system, which presents every individual's performance with respect to the best person within that typical network. Finally, after the normalisations, nine columns of new scores are added up, and the summative scores are ranked from highest to lowest.

In sum, ten inferences can be derived examining from Tables 5 and 6.

Inference 1: Stone is the unique centre of the DAE network because he obtained full scores among nine types of centrality measurement and outperformed the second (Prais) by almost 400 scores. In this sense, it is fair to claim that Stone's impact on the DAE network is significant, no matter what kind of networks or centrality measurements is examined: he is the strongest collaborator who shares the most citation similarity and the best benefit of his affiliates. As a local bridge, he is also the most efficient person in connecting the network.

Inference 2: Despite significantly lower than Stone, microeconometricians at the DAE (Prais, Brown, Houthakker, Aitchison, Utting, Tobin, Farrell) occupy seven of ten top-ranking

individuals with reasonably good collaboration rates and degrees of citation matching. This group is also placed closer to the centre intellectually and interpersonally with higher closeness centrality scores. Roy is the only microeconometrician excluded from the group; while he was not involved in the analysis of interwar household surveys, he self-sampled data of factory workers.

Inference 3: Apart from Stone and microeconometricians, short-term visitors Orcutt (active 1946–8) and Tobin (1949–50) received the highest two summative scores, especially from the coupling and acknowledgement measurements. Their higher rankings suggest that their intellectual impacts on the DAE community may persist after their departures.

Inference 4: The second efficient collaboration bridge is Durbin, which may be due to his works on both time-series econometrics and regional sampling. The second coupling bridge is Tintner, which can be explained by the fact that his works at the DAE tended to be general interest articles in econometrics that quoted materials with broader aspects. The second and third acknowledgement bridges are Roy and Orcutt because they reach another two people (Geary and Carter) who are not connected with Stone.

Inference 5: It would be misleading to interpret the DAE people with low scores as unimportant. These low scores only show that they are not the leading players in the networks of econometricians. For instance, Robinson was one of the committee members when the DAE was founded, and he only contributed one memorial paper on Keynes. Bray's work is mainly about accounting, so he would inevitably be excluded from this network of economists and statisticians. The same situation also happened to some economic historians (e.g., Deane and Buckatzsch).

Inference 6: Unlike the DAE, the SSRI does not have a unique network centre. Zellner's score is the highest among the SSRI people but only 185.83 higher than Goldberger's; thus, the former acts as a collaboration centre and the latter as an acknowledgement centre. In terms of bibliometric coupling, Zellner, Huang, and David are ranked the highest in degree, closeness, and betweenness centrality.

Inference 7: As director, Orcutt's impact on the SSRI is not as strong as Stone on the DAE, because he lost many points from the lack of collaboration. His acknowledgement centrality is ranked third within the group. Furthermore, he shares very similar coupling scores to Goldberger, SSRI's unique acknowledgement centre of the SSRI. These suggest that besides collaboration, Orcutt's influence as an intellectual adviser is still considerable.

Inference 8: The SSRI microeconometricians are still central to its network in terms of summative scores, with fewer concentrations than the DAE. Divided into diverse collaboration

groups, as shown in Figure 11 (down), microeconometrician's impacts on the SSRI networks are more evenly distributed from the higher- to lower-ranking people.

Inference 9: The second-best collaboration centre is Tiao, who does not appear once in the acknowledgement profile. That score is due to his collaborations with Zellner. Huang is the second coupling centre as his articles cover a wide range of data use. The second efficient acknowledgement bridges are Kasper, Lampman, and Cain, who are not econometricians but labour economists mediating between other groups and the core Goldberger (Figure 13, down).

Inference 10: The acknowledgement index on Box and Tiao could be biased, as their articles are published in statistics journals where no acknowledgement is documented. It might be due to the academic culture in the statisticians' profession, which does not tend to thank people in the footnotes. However, it is also notable that their names are not mentioned in any articles of other members of the SSRI.

However, it should be noted that what those rank scores show are individual relative situations in their networks. As exposed by the aggregate measurements in Table 4, the scales of centrality measurements of the DAE are at least twice more than the SSRI. In other words, when putting both networks on the same scale, scores of the SSRI will be significantly lower. For instance, as the SSRI's collaboration centre, Zellner's degree centrality, closeness centrality, and betweenness centrality are 0.097, 0.016, and 0.018, while the DAE's unique centre Stone has 0.195, 0.034, and 0.070. If taking all Stone's scores as 100, Zellner only scores 49.74, 47.06, and 25.71 in the DAE networks.

In sum, evidence from the centrality measurements demonstrates that members at the DAE and SSRI responded to their institutional settings differently. As presented in Table 4, from the aggregate perspective, the SSRI contains weaker academic ties than the DAE in collaboration, bibliometric coupling, and acknowledgements. By decomposing the individual contributions to these measurements, as shown in Table 5 and 6, the results indicate that the DAE is indeed a Stone-centred department, whereas the SSRI is a multi-centred institute where Orcutt's role is not overwhelmingly important.

Table 5 Individual centrality measurements in the DAE networks, ranked by the sum of nine network scores

Author	Collaboration			Coupling			Acknowledgement			Sum
	degree	closeness	betweenness	degree	closeness	betweenness	in-degree	closeness	betweenness	
Stone R	**100.00**	**100.00**	**100.00**	**100.00**	**100.00**	**100.00**	**100.00**	**100.00**	**100.00**	**900.00**
Prais SJ	50.00	99.50	17.24	69.70	94.41	26.86	36.36	96.74	0.74	491.55
Brown JAC	50.00	99.50	17.24	63.64	92.86	16.18	45.45	96.74	1.66	483.27
Houthakker HS	37.50	98.76	20.69	66.67	93.89	22.08	27.27	96.45	0.37	463.68
Aitchison J	37.50	99.34	0.00	63.64	92.86	13.01	18.18	95.88	0.00	420.40
Orcutt GH	25.00	74.75	3.45	57.58	92.35	19.24	18.18	96.45	16.57	403.57
Tobin J	12.50	97.79	0.00	57.58	92.35	10.15	27.27	96.17	0.00	393.81
Utting JEG	37.50	99.42	20.69	66.67	92.86	28.21	0.00	43.12	0.00	388.47
Farrell MJ	0.00	69.45	0.00	66.67	93.89	22.83	18.18	97.31	4.97	373.31
Durbin J	37.50	99.50	37.93	51.52	91.35	6.05	0.00	43.12	0.00	366.97
Rowe DA	12.50	99.01	0.00	42.42	89.42	7.60	9.09	95.60	0.00	355.65
Prest AR	0.00	69.45	0.00	60.61	92.86	11.41	18.18	96.74	2.21	351.45
Tintner G	0.00	69.45	0.00	63.64	93.37	45.92	9.09	44.72	0.00	326.19
Roy AD	0.00	69.45	0.00	21.21	86.67	0.58	9.09	96.74	17.13	300.87
Geary RC	0.00	69.45	0.00	36.36	88.95	2.88	9.09	92.35	0.00	299.09
Brumberg RE	0.00	69.45	0.00	24.24	85.79	0.64	18.18	95.88	0.00	294.18
James SF	25.00	74.75	3.45	42.42	89.89	9.87	0.00	43.12	0.00	288.50
Cochrane D	12.50	74.66	0.00	54.55	91.85	10.35	0.00	43.12	0.00	287.02
Briggs FEA	0.00	69.45	0.00	30.30	88.02	2.78	0.00	95.60	0.00	286.16
Watson GS	25.00	98.68	20.69	42.42	89.89	7.53	0.00	0.00	0.00	284.22
Carter CF	0.00	69.45	0.00	21.21	84.92	0.85	9.09	92.61	0.00	278.15
Bergstrom AR	0.00	69.45	0.00	24.24	84.92	2.50	0.00	96.17	0.00	277.29
Cramer JS	0.00	69.45	0.00	24.24	85.79	0.97	0.00	96.45	0.00	276.91
Cole D	12.50	98.44	0.00	24.24	86.22	0.75	0.00	43.12	0.00	265.27

Table 5 (continued)

Author	Collaboration			Coupling			Acknowledgement			Sum
	degree	closeness	betweenness	degree	closeness	betweenness	in-degree	closeness	betweenness	
Adams AA	12.50	71.15	0.00	24.24	85.79	0.00	18.18	46.44	1.10	259.40
Jackson EF	12.50	99.01	0.00	36.36	88.95	0.88	0.00	0.00	0.00	237.70
Stewart UG	12.50	71.15	0.00	24.24	85.79	0.00	0.00	43.12	0.00	236.80
Maywald K	0.00	69.45	0.00	27.27	86.22	0.29	0.00	46.37	0.00	229.61
Hansen K	12.50	99.01	0.00	27.27	86.22	3.07	0.00	0.00	0.00	228.08
Reddaway WB	0.00	69.45	0.00	24.24	85.35	1.52	0.00	46.37	0.00	226.95
Strotz RH	0.00	69.45	0.00	15.15	84.50	0.12	0.00	44.72	0.00	213.94
Hannan EJ	12.50	97.71	0.00	12.12	78.24	0.00	0.00	0.00	0.00	200.57
Deane P	0.00	69.45	0.00	27.27	87.11	10.69	0.00	0.00	0.00	194.53
Paige D	12.50	71.15	0.00	21.21	84.50	0.44	0.00	0.00	0.00	189.80
Adler S	12.50	71.15	0.00	21.21	84.50	0.44	0.00	0.00	0.00	189.80
Stuvel G	12.50	74.66	0.00	9.09	79.34	0.00	0.00	0.00	0.00	175.59
Pfouts RW	0.00	69.45	0.00	15.15	83.66	0.00	0.00	0.00	0.00	168.27
Anderson TW	0.00	69.45	0.00	12.12	84.92	0.42	0.00	0.00	0.00	166.92
Buckatzsch EJ	0.00	69.45	0.00	6.06	79.72	0.00	0.00	0.00	0.00	155.23
Robinson A	0.00	69.45	0.00	0.00	9.81	0.00	0.00	0.00	0.00	79.27
Bray FS	0.00	69.45	0.00	0.00	9.81	0.00	0.00	0.00	0.00	79.27
Forsyth FG	0.00	69.45	0.00	0.00	9.81	0.00	0.00	0.00	0.00	79.27

Note: Let $f(i,x,y)$ be the y-centrality measurement of individual i in network x, the network centrality score $M(i,x,y)$ is a linear scale-transformation of $f(i,x,y)$. For each combination of x and y, the individual score is defined as $M(i) = 100 * \{f(i)/\max[f(i)]\}$. Authors of the microeconometric articles are highlighted with bold font. Table 6 applies the same criteria.

Table 6 Individual centrality measurements in the SSRI networks, ranked by the sum of nine network scores

Author	Collaboration			Coupling			Acknowledgement			Sum
	degree	closeness	betweenness	degree	closeness	betweenness	in-degree	closeness	betweenness	
Zellner A	**100.00**	**100.00**	**100.00**	**100.00**	99.74	56.48	91.67	98.71	44.73	791.32
Goldberger AS	42.86	88.65	4.35	55.56	98.97	15.10	**100.00**	**100.00**	**100.00**	605.49
Huang DS	42.86	99.84	21.74	96.30	**100.00**	96.77	0.00	97.45	22.73	577.68
Williamson JG	28.57	87.40	2.17	70.37	99.10	91.06	16.67	93.85	2.83	492.03
Orcutt GH	0.00	85.01	0.00	59.26	98.78	16.52	75.00	98.32	44.62	477.52
David M	28.57	87.40	2.17	51.85	98.78	**100.00**	8.33	86.41	0.00	463.53
Kmenta J	14.29	87.39	0.00	85.19	99.48	43.56	16.67	93.62	0.34	440.53
Holt CC	28.57	87.40	2.17	51.85	98.91	58.18	16.67	93.39	0.79	437.93
Weiss LW	14.29	86.19	0.00	37.04	98.78	38.31	16.67	96.58	32.90	420.75
Cain GG	28.57	89.90	0.00	14.81	95.90	7.17	33.33	98.07	48.89	416.64
Lee TH	14.29	99.78	0.00	74.07	99.16	18.37	8.33	96.22	0.00	410.23
Greenberg E	0.00	85.01	0.00	51.85	99.16	19.49	16.67	97.70	33.07	402.94
Tiao GC	57.14	99.91	58.70	66.67	98.85	14.79	0.00	0.00	0.00	396.06
Day RH	0.00	85.01	0.00	48.15	98.53	31.35	16.67	94.67	19.40	393.77
Weisbrod BA	57.14	89.94	10.87	25.93	96.19	15.65	0.00	93.62	0.00	389.34
Kasper H	0.00	85.01	0.00	22.22	97.84	14.81	8.33	97.95	59.79	385.95
Chau LC	28.57	99.82	0.00	48.15	98.59	2.95	8.33	94.08	0.00	380.50
Theil H	14.29	99.78	0.00	51.85	98.85	10.72	8.33	95.73	0.00	379.55
Feige EL	0.00	85.01	0.00	48.15	98.53	6.08	16.67	97.07	22.99	374.50
Wu DM	0.00	85.01	0.00	40.74	98.28	12.41	16.67	96.95	12.99	363.04
Johnston J	0.00	85.01	0.00	44.44	99.04	21.36	0.00	93.73	0.00	343.58
Lee ML	14.29	88.62	0.00	22.22	97.90	0.12	8.33	97.32	4.16	332.97
Hall M	14.29	86.19	0.00	29.63	98.03	4.60	0.00	96.70	2.19	331.63
Hansen WL	28.57	89.90	0.00	7.41	93.34	0.00	16.67	93.62	0.00	329.51

Table 6 (continued)

Author	Collaboration			Coupling			Acknowledgement			Sum
	degree	closeness	betweenness	degree	closeness	betweenness	in-degree	closeness	betweenness	
Zarembka P	0.00	85.01	0.00	18.52	96.98	0.47	8.33	96.83	14.10	320.23
Fisher JA	0.00	85.01	0.00	22.22	97.84	0.08	16.67	94.90	0.17	316.89
Krainer RE	0.00	85.01	0.00	25.93	98.09	1.24	0.00	94.08	0.00	304.35
Lampman RJ	14.29	86.19	0.00	3.70	29.75	0.00	25.00	94.31	48.59	301.82
Korbel J	0.00	85.01	0.00	14.81	97.66	0.00	8.33	93.73	0.00	299.55
Bawden DL	0.00	85.01	0.00	11.11	96.31	1.38	8.33	93.62	2.16	297.92
Schmitt HO	0.00	85.01	0.00	18.52	97.72	1.84	8.33	86.41	0.00	297.83
Morgan T	0.00	85.01	0.00	3.70	95.96	0.00	8.33	90.30	12.82	296.12
Steward DV	0.00	85.01	0.00	11.11	96.74	0.00	0.00	93.97	0.99	287.81
Heidhues T	0.00	85.01	0.00	7.41	95.48	0.00	8.33	90.40	0.00	286.63
Nagar AL	28.57	88.63	0.00	40.74	98.34	1.30	0.00	27.69	0.00	285.28
Odeh HS	28.57	88.63	0.00	40.74	98.34	1.30	0.00	27.69	0.00	285.28
Myers JG	14.29	99.62	0.00	37.04	98.53	6.03	0.00	27.69	0.00	283.19
Larner RJ	0.00	85.01	0.00	3.70	95.66	0.00	0.00	92.15	0.00	276.52
Aigner DJ	28.57	87.40	2.17	11.11	96.13	21.16	0.00	27.69	0.00	274.23
Chetty VK	14.29	99.78	0.00	51.85	98.53	8.54	0.00	0.00	0.00	272.99
Kelley AC	14.29	87.39	0.00	33.33	98.28	1.05	0.00	27.69	0.00	262.01
Box GEP	14.29	99.69	0.00	37.04	98.09	3.83	0.00	0.00	0.00	252.93
Karpoff P	14.29	89.88	0.00	14.81	95.90	0.00	0.00	27.69	0.00	242.56
Swift WJ	14.29	89.88	0.00	14.81	95.90	0.00	0.00	27.69	0.00	242.56
Morgan JN	14.29	87.39	0.00	25.93	97.90	15.44	0.00	0.00	0.00	240.94
Hooper JW	14.29	99.78	0.00	25.93	97.23	0.06	0.00	0.00	0.00	237.27
Steiner PO	0.00	85.01	0.00	0.00	29.34	0.00	25.00	94.90	0.51	234.75
Miller RF	14.29	86.19	0.00	0.00	29.34	0.00	8.33	92.60	0.00	230.74
Tan WY	14.29	99.69	0.00	18.52	96.80	0.00	0.00	0.00	0.00	229.29

Table 6 (continued)

Author	Collaboration			Coupling			Acknowledgement			Sum
	degree	closeness	betweenness	degree	closeness	betweenness	in-degree	closeness	betweenness	
Guttman I	14.29	99.69	0.00	18.52	96.80	0.00	0.00	0.00	0.00	229.29
Sprenkle CM	14.29	87.39	0.00	3.70	93.17	0.00	0.00	27.69	0.00	226.23
Stromsdorfer EW	0.00	85.01	0.00	3.70	29.75	0.00	8.33	92.48	0.00	219.27
Somers GG	0.00	85.01	0.00	3.70	29.75	0.00	8.33	92.15	0.00	218.94
Bridges B	0.00	85.01	0.00	0.00	29.34	0.00	0.00	90.30	12.82	217.46
Culbertson JM	0.00	85.01	0.00	0.00	29.34	0.00	8.33	92.94	0.00	215.61
Otsuki T	14.29	87.39	0.00	11.11	95.96	0.35	0.00	0.00	0.00	209.08
Earley JS	42.86	88.65	4.35	11.11	30.60	0.00	0.00	28.77	0.34	206.67
Shelton JP	14.29	87.39	0.00	7.41	96.80	0.00	0.00	0.00	0.00	205.88
Heins AJ	14.29	87.39	0.00	7.41	96.07	0.00	0.00	0.00	0.00	205.15
Lee KS	14.29	87.39	0.00	3.70	95.78	0.00	0.00	0.00	0.00	201.15
Severance MF	28.57	88.63	0.00	11.11	30.60	0.00	8.33	28.76	0.00	196.00
Firestone FN	28.57	88.63	0.00	11.11	30.60	0.00	8.33	28.76	0.00	196.00
Haslem JA	0.00	85.01	0.00	11.11	96.31	0.00	0.00	0.00	0.00	192.43
Stroud AH	14.29	86.19	0.00	3.70	29.75	0.00	0.00	27.69	0.00	161.61
Braff AJ	14.29	86.19	0.00	0.00	29.34	0.00	0.00	27.69	0.00	157.50
Carleton WT	14.29	88.62	0.00	11.11	30.60	0.00	0.00	0.00	0.00	144.61
Lieberson S	14.29	86.19	0.00	3.70	29.75	0.00	0.00	0.00	0.00	133.92
Allen IL	14.29	86.19	0.00	3.70	29.75	0.00	0.00	0.00	0.00	133.92
Secrest D	14.29	86.19	0.00	3.70	29.75	0.00	0.00	0.00	0.00	133.92
Miyamoto SF	14.29	86.19	0.00	3.70	29.75	0.00	0.00	0.00	0.00	133.92
Chen YP	0.00	85.01	0.00	0.00	29.34	0.00	0.00	0.00	0.00	114.35
Granick D	0.00	85.01	0.00	0.00	29.34	0.00	0.00	0.00	0.00	114.35
Geffert JA	0.00	85.01	0.00	0.00	29.34	0.00	0.00	0.00	0.00	114.35

4.2 Interpretations with the Strength-of-weak-tie Theory

Chapter III interpreted Stone's role at the DAE as akin to the captain of the rowing club, who collaborated between and allocated crews to different boats. Evidence from the former section verified Stone's importance by analysing centrality measurements. However, while the centrality measurements indeed provide a typical way of understanding the Stone-Orcutt contrast, the impact of the directors on this difference is not fully answered. Framed under Granovetter's (1973) concepts of weak and strong ties, this section utilises the acknowledgement data as a proxy of information transmission to further explore Stone and Orcutt's respective roles as directors.

In his famous article 'The strength of weak ties', Granovetter (1973) argues that the people in social networks are more likely to benefit from their acquaintances (weak ties) rather than their close friends (strong ties). The main merit of weak ties is that indirect contacts expand the possibility for people to reach others and facilitate information transmissions in interpersonal networks, as elaborated by Granovetter (1983, 202),

> … individuals with few weak ties will be deprived of information from distant parts of the social system and will be confined to the provincial news and views of their close friends. This deprivation will not only insulate them from the latest ideas and fashions but may put them in a disadvantaged position in the labor market …

His hypothesis predicted that the strong tie could hardly act as a bridge between people as its outreach limits the transmission route. This point was verified in his interview of 54 employees from a Boston suburb. His result showed that over half of the job information were transmitted by the interviewees' occasional contacts (Granovetter, 1973, 1371).[210] As such, weak ties are crucial elements of effective communication between individuals,

> The macroscopic side of this communications argument is that social systems lacking in weak ties will be fragmented and incoherent. New ideas will spread slowly, scientific endeavors will be handicapped, and subgroups separated by race, ethnicity, geography, or other characteristics will have difficulty reaching a *modus vivendi*. (Granovetter, 1983, 202)

Under Granovetter's framework, evidence from the bibliometric information is applied to test the Stone-Orcutt contrast. In doing so, the definitions of weak and strong ties within interpersonal networks should be made operational for the applications of bibliometric data. First, if one person collaborates with or acts as a supervisor to another person, they will form a strong tie in-between. The rationale behind this definition is intuitive. In general, it is unlikely that people under academic rules cannot get along with their co-authors and supervisors, and

[210] Grannovetter (1973, 1371) defines the intensity of contact as 'often = at least twice a week; occasionally = more than once a year but less than twice a week; rarely = once a year or less'.

in almost all cases, they will meet frequently. Accordingly, Stone's strong ties at the DAE includes Aitchison, Brown, Cochrane, Prais, and Utting; Orcutt's strong ties at the SSRI includes Maw-Lin Lee, Tong Hun Lee, Greenberg, Hall, and Wu.[211] Second, if the person is neither a co-author nor a supervisor to another person, they will form a weak tie in-between. This definition works in the institutional settings of the DAE and SSRI, where people work in the same building, share some mutual friends, and meet occasionally. Last, the definitions were applied to infer the information interchanges within interpersonal relationships, namely, acknowledgement networks. Since most people tend to thank others subjected to academic convention, the acknowledgement is clear evidence that some knowledge has been transferred.

Three observations with the strength-of-weak-tie theory can be summarised:

Observation 1: Stone has built 50% acknowledgement linkages in the DAE's network (14 of 28), while Orcutt has built 18.5% in the SSRI's (10 of 54).

Observation 2: 38.6% of Stone's received acknowledgements come from his strong ties (8.5 of 22), while Orcutt has 60.7% (8.5 of 14).

Observation 3: Once the scope was limited to networks of microeconometricians, who were defined as the authors of microeconometric samples presented in Table 3, Stone received 61.4% acknowledgements from microeconometricians (13.5 of 22), while Orcutt received 85.7% (12 of 14).

These facts suggest that Stone and Orcutt, as directors of their institutions, contain very different leadership styles. As a weak tie, Stone benefited his staff members more than Orcutt as Stone was more likely to act as an information bridge for other staff members. The decompositions of their received acknowledgements further support this claim. Orcutt has a higher tendency to benefit his strong ties, and almost all of Orcutt's acknowledgements were made by microeconometricians, indicating that Orcutt is still an influential leader within a small group, and Stone's impact on DAE is more general. Back to the rowing-crew analogy, Stone is indeed a captain of the rowing club, whereas Orcutt is more like a coxswain leading the boat of SSRI microeconometricians.

[211] Cochrane and Prais are known as Stone's students from various sources. The list of Orcutt's advisees was taken from Lampman (1993, 319–20).

5. Picturing the Development of Microeconometric Exemplars

5.1 Definition and Methodology

The definition of the Kuhnian exemplar as the methodological paradigm was highlighted in Chapter I. This model of empirical knowledge argues that consolidated practices are diffused as exemplars in the community, and the most apparent sign of becoming a methodological paradigm is whenever other people cite it. Based on this framework, sections 5 and 6 offer an empirical recipe and its application in the use bibliometric materials. Two historical concerns are addressed here. The first is to understand the formation of the DAE's and SSRI's microeconometric exemplars in terms of the critical issues addressed and primary materials relied upon. Second, to further examine the Stone-Orcutt contrast to see whether the DAE practices follow Stone's exemplars and if the SSRI practices move away from Orcutt's.

The analysis consisted of two stages. The co-citation analysis was first applied to locate the microeconometric exemplars of both institutions and then to the citation analysis of these exemplars in tracking how empirical knowledge is transmitted between them and the samples.

Using co-citation analysis prevents focusing on a specific reference while examining the commonalities in practice. Indeed, the co-citation analysis considers how citations appear as a pair, which will inevitably exclude some standalone but influential references. However, when the exemplar is more frequently cited with other exemplars, it often indicates that it better accommodates the rest of that literature and has stronger reasons to be seen as paradigmatic. This advantage is helpful to identify more credible exemplars based on the community's consensus.

In addition, the citation analysis concentrated on how the microeconometric samples perceive their exemplars. In doing so, that calls for a contextual criterion in analysing the citations during the transmission process. A possible approach to the history of economics is the one used by Stigler and Friedland (1975, 488), who categorised attitudes toward citations into 'favourable', 'unfavourable', and 'neutral'.[212] However, this treatment might not fully capture the development of econometrics. In the case of normative economic theories, as Stigler and Friedland examined, diverse research values and schools of thought affect scholars' personal judgements, so it would matter whether the citation is favoured or not. Since

[212] Their criteria are based on judgements from two graduate students who identify all attitude profiles in 5,581 citations appeared in their selected samples of value theory, monetary theory, and fiscal theory. They ultimately classified 648 favourable and 566 unfavourable citations.

econometrics is empirical-oriented, rather than towards normative judgements, how the econometric argument actually relates to other cited empirical evidence is more crucial. Given such a difference between normative and empirical economics, another criterion in describing citation relation is needed.

The idea of *talking* and *listening* citation will be employed to understand the relationship established in econometrics. This dichotomy was inspired by Howlett (2008), who adopted the method of 'listening citation tree' and 'talking citation tree' to evaluate the impact of the Indian Green Revolution.[213] Unlike Howlett's account, both concepts in this section are applied to describe the connections between the material. The talking citation actively responds to the core issue of the cited work and shows intellectual progress within the literature. In other words, a talking citation should always put some theoretical or methodological issues forward no matter whether the cited work is favourable or not. The listening citation mainly receives the information from its cited work and does not question further methodological issues. Making listening citations is always considered an academic responsibility for various reasons, depending on the community's current consensus. In sum, the talking-listening method distinguishes the citations between *scientific communication* and *scientific credit*. A talking citation shows how scientific conversations are moving forward, and a listening citation signals how the exemplar is reinforced in the community.

Applying the concepts of talking and listening citation will result revealing interpretations of the development of econometric exemplars. For instance, taking the citations of Allen and Bowley (1935, henceforth the AB1935) from the DAE microeconometric samples as an example, six citations are listed below:

Case 1: cited in Houthakker (1952)

> The subjects discussed will be found to be largely the same as in the pioneering monograph of Allen and Bowley (1935), which is still without rival as an introduction to the theory and econometrics of this field … (1)
>
> It would be convenient to have a homoscedastic normal distribution [of the residuals], and Allen and Bowley (1935, pp. 140–1) have given reasons why this might in fact be found, but their argument is not convincing since they ignore the fact that consumption cannot be negative (cf. 3.3). (3)

Case 2: cited in Prais (1952, 87)

> Though the Engel curve may be approximated by a linear relationship over short income ranges (as found by Allen and Bowley [1935]) it has for some time been recognised that it

[213] Howlett (2007, 8) has put the definitions, '… it [the listening tree] takes a particular article and shows all the other articles in the sample that it cited and then it takes each of those articles in turn and repeats the process, and so on' and 'the talking citation tree takes a particular article and shows all the other articles in the sample that cite it and then it takes each of those articles in turn and repeats the process'.

is necessary to take into account the substantial non-linearity of the relationship in describing the full range of variations found in typical budget collections.

Case 3: cited in Farrell (1953)

If we number these groups with a variable k, we can write any individual demand function:
$c = c(y, k, u)$
where u represents the random factors. (195)

Now assume that u is distributed according to some frequency function $f = F(k, u)$ independently (in the statistical sense) of y … [footnoted] Allen and Bowley ([1935], p. 115 et seq.) make the assumption, but do not exploit it in any way and miss its interesting implications. (203)

Case 4: cited in Farrell (1954, 176n)

… the assumption [in case 3] itself was made by Allen and Bowley (1935), although, since they only applied it to linear individual demand functions, they missed its many interesting implications.

Case 5: cited in Aitchison and Brown (1954a, 35)

In 1935 Allen and Bowley based their analysis of family budgets on linear Engel curves, though they were not unaware that the linear form was 'only a first approximation to a regular curve.' Subsequent investigators who had available information over wider ranges of incomes had recourse to curvilinear forms and found it especially compelling to postulate different curves for necessities and luxuries.

Case 6: cited in Brown (1955, 65)

… it is possible to tabulate the values of consumption corresponding to different income levels for the purposes of comparison with nutritional standards. Data and discussion on this relationship are to be found, for this country, in the works of Allen & Bowley (1935) …

From those citations, a Stigler-Friedland interpretation is possible: case 1 praised AB1935 as 'pioneering' but also noted the methodological problems of the analysis; cases 2 and 5 extended the empirical Engel curve to a non-linear form from AB1935; and case 6 simply cited AB1935 as an early example. Those cases which do not imply any agreement or disagreement with AB1935 would be interpreted as neutral citations. In contrast, cases 3 and 4 referred to AB1935 due to its uncritical acceptance of the independence assumption between random factors and personal income. These are examples of unfavourable citations. However, this classification may not help generate informative relationships in the empirical writings as they fail to capture the progressiveness of the literature; in such types of studies, scholars often make academic references without explicitly expressing their attitudes.

Under the talking-listening criterion, the citations of AB1935 induced an alternative classification that cases 1-5 are talking citations, and case 6 is listening. The first five have shown how econometric problems are progressed and inherited: in cases 1, 2, and 5, the authors clearly addressed AB1935 as their intellectual precursor and noted that more attempts should

be made in the literature on the empirical Engel curve. In cases 3 and 4, the author challenged one of the statistical assumptions in AB1935 and hoped to derive more interesting interpretations. The AB1935 is a talking exemplar for those who want to provide more abundant evidence on the Engel curve and push more discussions on the econometric identifications. In case 6, the author just used the AB1935 as stylised evidence in the literature on the relationship between income and consumption, indicating that it is taken as a listening exemplar.

5.2 Locating the Microeconometric Exemplars

The algorithm of the co-citation analysis in *Bibilometrix* was adopted to locate exemplars for further analysis of listening and talking citations. The samples are taken from 28 microeconometric articles of the DAE and 35 of the SSRI summarised in Table 3. The bibliometric information of top-20 co-cited materials are summarised in Tables 7 and 8.

The tables allow some initial interpretations. The DAE's top co-cited materials are all empirical studies. The top three of them are Allen and Bowley (1935), Houthakker (1952), and Tobin (1950), which are early microeconometric studies published in the U.K.; Henderson, Houthakker, and Stone are the dominating authors, contributing to 11 of them; ten of them are produced by the DAE authors, and eight belong to the DAE reprint series. The SSRI's most co-cited material is Orcutt's 1961 microsimulation study, and only two of them belong to SSRI's microeconometric samples. The next four are Klein and Lansing (1955), Stone and Rowe (1957), Tobin (1958), and Watts and Tobin (1960), in which none of them are works by SSRI affiliates.[214] It is also worth noting that two doctoral dissertations by Huang (1961) and Wu (1962) appear in the SSRI network.

[214] Klein and Lansing were at Michigan, Stone and Rowe at Cambridge, and Tobin and Watts at Yale. Watts came to the SSRI in 1963.

Table 7 List of top-20 co-cited materials of the DAE, ranked by the times being cited (TBC)

Author (year)	TBC	Title	Source
Allen and Bowley (1935)	6	Family Expenditure	Book
Houthakker (1952)	6	The Econometrics of Family Budgets	Journal of the Royal Statistical Society
Tobin (1950)	6	A Statistical Demand Function for Food in the U.S.A.	Journal of the Royal Statistical Society
Massey (1942)	5	The Expenditure of 1,360 British Middle-Class Households in 1938-39	Journal of the Royal Statistical Society
De Wolff (1941)	4	Income Elasticity of Demand, a Micro-Economic and a Macro-Economic Interpretation	Economic Journal
Houthakker and Prais (1952)	4	Les Variations de Qualité dans les Budgets Famille	Économie Appliquée
Nicholson (1949)	4	Variations in Working Class Family Expenditure	Journal of the Royal Statistical Society
Prais (1952)	4	Non-Linear Estimates of the Engel Curves	Review of Economic Studies
Stone (1951)	4	The Demand for Food in the United Kingdom Before the War	Metroeconomica
Allen (1942)	3	Expenditure Patterns of Families of Different Types	Studies in Mathematical Economics and Econometrics (eds. Lange et al.)
Henderson (1949a)	3	The Cost of a Family	Review of Economic Studies
Henderson (1949b)	3	The Cost of Children. Part I	Population Studies
Henderson (1950)	3	The Cost of Children. Parts II and III	Population Studies
Houthakker (1953)	3	Forme des Courbes d'Engel	Cahiers du Sdminaire d'Econometrie
Stone and Rowe (1954)	3	The Measurement of Consumers' Expenditure and Behaviour in the United Kingdom, 1920-38	Book
Stone (1945)	3	The Analysis of Market Demand	Journal of the Royal Statistical Society
Stuvel and James (1950)	3	Household Expenditure on Food in Holland	Journal of the Royal Statistical Society
Hajnal and Henderson (1950)	2	The Economic Position of the Family	Papers of the Royal Commission on Population
Mendershausen (1946)	2	Changes in Income Distribution during the Great Depression	Book
Stone (1948)	2	The Analysis of Market Demand: An Outline of Methods and Results	Review of the International Statistical Institute

Note: Microeconometric samples are marked with bold font. Table 8 applies the same criterion.

Table 8 List of top-20 co-cited materials of the SSRI, ranked by the times being cited (TBC)

Author (year)	TBC	Title	Source
Orcutt, Greenberger, Korbel and Rivlin (1961)	7	Microanalysis of Socioeconomic Systems: A Simulation Study	Book
Klein and Lansing (1955)	4	Decisions to Purchase Consumer Durable Goods	Journal of Marketing
Stone and Rowe (1957)	4	The Market Demand for Durable Goods	Econometrica
Tobin (1958)	4	Estimation of Relationships for Limited Dependent Variables	Econometrica
Watts and Tobin (1960)	4	Consumer Expenditures and the Capital Account	Consumption and Saving (eds. Friend and Jones)
Fisher (1962)	3	An Analysis of Consumer Durable Goods Expenditures in 1957	Review of Economics and Statistics
Friedman (1957)	3	A Theory of the Consumption Function	Book
Lansing (1954)	3	Concepts Used in Surveys	Contributions of Survey Methods to Economics (ed. Klein)
Orcutt and Rivlin (1960)	3	An Economic and Demographic Model of the Household Sector: A Progress Report	Demographic and Economic Change in Developed Countries
Tobin (1957a)	3	Consumer Debt and Spending: Some Evidence from Analysis of a Survey	Consumer Instalment Credit (The Board of Governors of the FED)
Wu (1962)	3	An Empirical Analysis of Household Durable Goods Expenditure	PhD Dissertation
Huang (1961)	3	The Demand for Automobiles in 1956 and 1957 – A Cross-Section Analysis	PhD Dissertation
Alchian and Kessel (1962)	2	Competition, Monopoly and the Pursuit of Money	Aspects of Labor Economics
Feige (1964)	2	The Demand for Liquid Assets: A Temporal Cross-Section Analysis	Book
Johnston (1963)	2	Econometric Methods	Book
Koyck (1954)	2	Distributed Lags and Investment Analysis	Book
Muth (1960)	2	The Demand for Non-farm Housing	The Demand for Durable Goods (ed. Harberger)
Nerlove (1960)	2	The Market Demand for Durable Goods: A Comment	Econometrica
Stone and Rowe (1960)	2	Durability of Consumers' Durable Goods	Econometrica
Zellner (1962)	2	An Efficient Method of Estimating Seemingly Unrelated Regressions and Tests for Aggregation Bias	Journal of the American Statistical Association

Titles in Tables 7 and 8 further confirm that the microeconometrician's general research concern in demand studies shifted from the food expenditure in the 1950s to non-food expenditure and savings in the 1960s. Fourteen DAE exemplars are relevant to the analysis of family expenditure and Engel curve and 12 of the SSRI's work on consumer durables and saving behaviour. The driving forces behind such differences are twofold. First, it reflects both institutions' relative availability of data at respective timings: in the early 1950s, there was no sufficient income data but only food expenditure surveys. This shortage of data limited DAE's ability to explore the demand for non-food commodities. In the early 1960s, when the Michigan Survey of Consumer Finance extended the research scope in terms of information collected, the SSRI used this information on saving and consumptions on non-food commodities. Second, the difference again characterises the respective usages of micro-evidence by Stone and Orcutt. In Stone's case, evidence from the food expenditure is a supplement for his time-series demand equation (Section 6, Chapter III). In Orcutt's case, evidence from the durable goods and savings defines one of the OC of the household sector for his microsimulation (Section 4, Chapter IV).

6. Assessing the Development of Microeconometrics

Section 5 has identified the exemplars of the DAE's and SSRI's microeconometrics that fit into the general picture obtained in Chapters III and IV. To further decompose the formation process, the concepts of listening and talking citations are applied in this section to analyse how these exemplars were received and capture the crucial elements in the evolution of microeconometric knowledge.

Table 9 summarises all 133 citations listed in Table 7 and 8 regarding the way being referenced. The DAE has more citations of exemplars than the SSRI in general. This difference is due to the talking citations. With very similar listening citations, the DAE has twenty-five talking citations while the SSRI has 11, implying that the authors at the DAE are more outward-looking than those at the SSRI. Furthermore, the DAE outnumbers the SSRI in terms of in-community and in-sample citations. Although differences in the in-community citation were not significant, the SSRI econometricians were not very active in referencing their reprint series compared with the DAE, substantiating that the SSRI series had less contemporary influence on its members.

Table 9 Compositions of the listening and talking citations in each community

	Listening citations			Talking citations			Total
	total	in-community*	in-sample	total	in-community*	in-sample	
DAE	48	33	19	25	5	4	73
SSRI	49	23	5	11	2	0	60

* Indicates that the authors and cited authors belong to the same community.

To further examine the contributions of these exemplars, Figures 14 and 15 visualised the DAE's and SSRI's listening citations into flowcharts and Figures 16 and 17 into their talking citations. All the figures were constructed under the criteria that (1) unframed publications are the microeconometric samples, (2) publications with a dashed frame are the microeconometric samples and exemplars, (3) publications with a solid frame are exemplars but do not belong to its reprint series, (4) in-community publications are capitalised and highlighted with bold font, and (5) dash arrows indicate in-sample citations. Eight inferences can be summarised.

Inference 1: The DAE's listening citations weaves a dense network of in-sample citations. The four exemplars are Tobin (1950), Stone (1951), Houthakker (1952), and Prais (1952), which account for 14 of the in-sample listening citations in total. Among them, Tobin (1950) is the earliest exemplar of studying food demand combined with annual and budget data and is all listening-cited. Stone (1951), Houthakker (1952), and Prais (1952) are early reports of the micro estimates that are subsequently adopted in their DAE monographs (Stone & Rowe, 1954; Prais & Houthakker, 1955). The last two articles are also referenced by Brown (1954; 1955).

Inference 2: Houthakker and Prais (1952) and Houthakker (1953) are both cited by Houthakker (1952) and Prais (1952). Although the former two are microeconometric articles, they were excluded at the first stage since they are written in French (footnote 199, this chapter). Thus, the influence of the DAE's in-sample citations may be underestimated since the impact of French publications was omitted.

Inference 3: The crucial listening exemplars of the DAE are De Wolff (1941), Massey (1942), and Stone (1945; 1948). Among them, De Wolff (1941) is a theoretical study that derives the properties of income elasticities at different levels and argues that aggregating from the individual Engel curve to a higher level requires more information on income distribution. Massey (1942) provides the descriptive summary of a 1938–9 British middle-class survey. Stone (1945, 1948) are early macroeconometric studies of market demand.

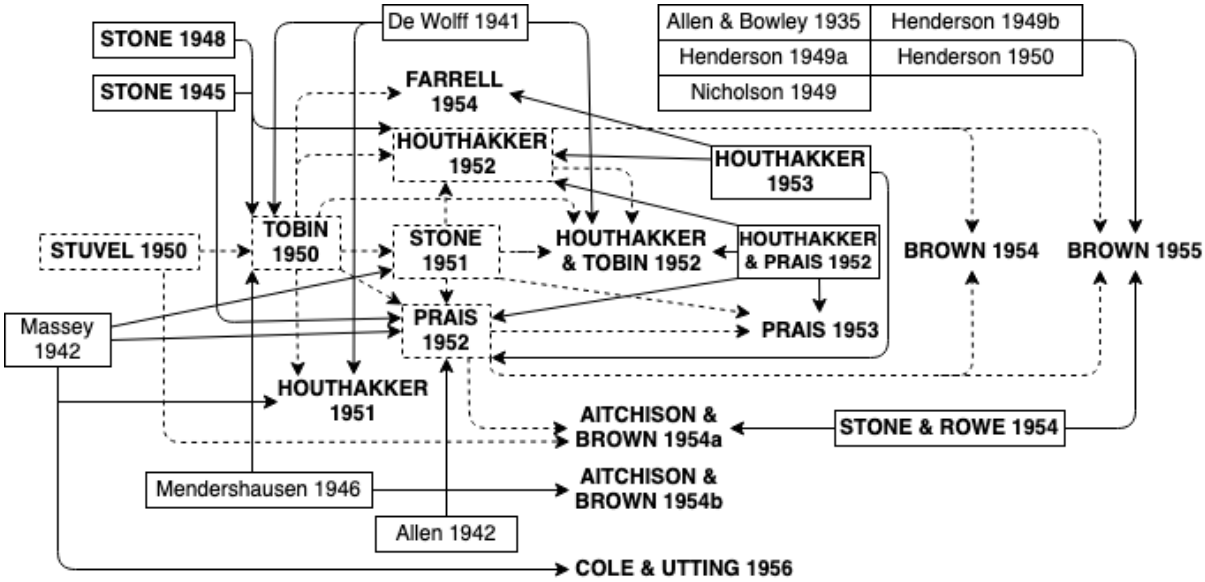

Figure 14 The listening citation flowchart of DAE's microeconometrics. Note: Arrows indicate the publication is listening-cited by its destination.

Inference 4: SSRI's listening citations between the samples are less dense. The two exemplars are Zellner (1962) and Fisher (1962), which only account for five in-sample citations, suggesting that scholarly communications between the SSRI samples are also less intense. The most co-cited exemplar is Orcutt's microsimulation study (Orcutt et al., 1961), which is all listening-cited. This fact again verifies the interpretation of section 4.2 that Orcutt's impact among microeconometricians is still strong.

Inference 5: Wu (1965) is a notable centre among the SSRI's listening citations. While never cited by its contemporaries, the article has 14 listening citations among 20 exemplars. While the paper contributes to most co-citation relations, identified exemplars in this network may be unstable.

Inference 6: Proportional numbers of the SSRI's listening exemplars come from outside the community, such as Cambridge (Stone & Rowe, 1957; 1960), Michigan (Lansing, 1954; Klein & Lansing, 1955), and Yale (Tobin, 1957a; Watts & Tobin, 1960). Apart from Lansing (1954), which is an outline of the concepts of Michigan's SCF, all others are empirical investigations of the demand for durable goods: Stone and Rowe (1957; 1960) on its estimations using time-series data, Klein and Lansing (1955) on its relations to expectations, Tobin (1957a) on its associations between personal status and saving behaviours, and Watts and Tobin (1960) on its different consumption patterns.

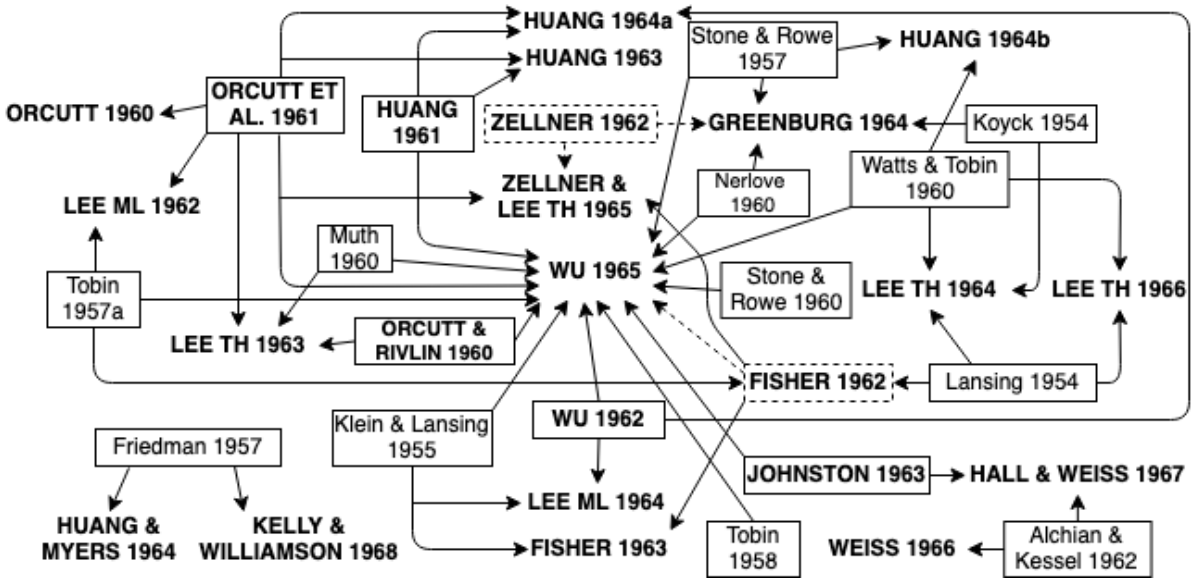

Figure 15 The listening citation flowchart of SSRI's microeconometrics

Inference 7: Houthakker (1952) and Prais (1953) built over half of the talking citations (14 of 25), suggesting these two articles make most attempts in progressing the research questions. Allen and Bowley (1935) and Nicholson (1949) are two of the essential talking exemplars of the DAE samples. The two publications share similar methodologies but use distinct budget datasets.

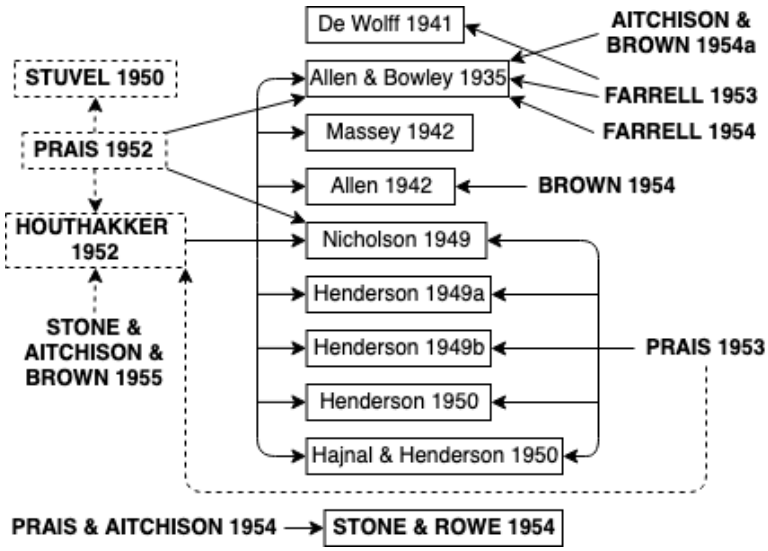

Figure 16 The talking citation flowchart of DAE's microeconometrics. Note: Arrows indicate the publication is talking-cited by its starting point.

Inference 8: Like its listening exemplars, the SSRI's talking exemplars compared with the DAE are fewer in number. The most significant concern addressed are the three talking attempts that look for an alternative estimation to Tobin (1958), which is his classic paper of

the Tobit model expanding the probit model to match the application of limited dependent variables. Another concern is Stone and Rowe's time-series econometric analysis of durable goods.

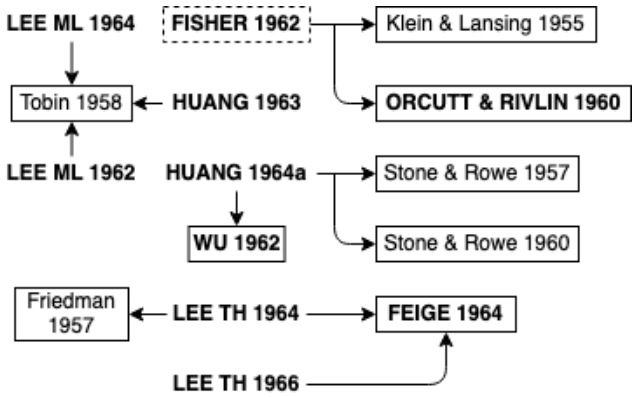

Figure 17 The talking citation flowchart of SSRI's microeconometrics

The formation of microeconometric knowledge of both communities and their differences are made clear in the flowcharts. From the perspective of listening citations, the DAE is still a better-connected community than the SSRI after limiting the samples to microeconometrics. The DAE has more significant numbers of in-sample citations, whereas the SSRI authors tend to listen to outsiders. Furthermore, their listening citations show the field is in a different stage of development. The DAE samples are pretty much developed independently, and very few of their listening exemplars are microeconometric studies. By contrast, the SSRI samples rely more on concurrent studies from Cambridge, Michigan, and Yale, indicating that in the 1960s, microeconometrics as a research agenda had been consolidated by other scientific communities.

The fact that Orcutt et al (1961) is well-listened verifies his considerable influence among the SSRI microeconometricians. As such, the key in understanding the Stone-Orcutt contrast is not due to whether the director has possessed a fascinating research programme. What makes their citation flowcharts different is the lateral communication between young scholars. In the DAE, there are some mediators such as Tobin and Houthakker who bridge between the director and other PhD students. In the SSRI, despite the hiring of some early-career PhDs (e.g., Fisher and Huang), such a mediating character seems to be absent.

From the talking citations, the DAE people have shown their ambition to inherit and revise the interwar Allen-Bowley framework of the Engel curve, but they still talk to their exemplars. Conversely, while the SSRI people were trying to find a Tobit alternative for estimating survey

data, their works speak less to their exemplars. Since the SSRI's talking exemplar is more recent than the DAE's, community relations in the 1960s may be more competitive than earlier.

7. Concluding Remarks

The techniques of network and citation analysis with bibliometric data were used to explore the centrality of directorship at the DAE and SSRI and the formations of microeconometric practices through the citations of exemplars. Empirical results from network measurements show that the density of DAE networks approximately doubles the SSRI networks in terms of collaboration, bibliometric coupling, and acknowledgement. To further verify the Stone-Orcutt contrast, a Granovetter-style analysis of the acknowledgement data suggests their different styles of directorship. While Orcutt's impact on the SSRI is limited to the microeconometrician group, Stone's influence has a greater outreach in the DAE community. Furthermore, the talking-listening citation analysis again confirms this contrast by analysing their microeconometric practices. Despite Orcutt's strong practical influence, SSRI's microeconometrics rely on fewer internal citations than the DAE's, suggesting that the SSRI faces more community competition than the DAE and that household microeconometrics had been well established back in the 1960s.

Chapter VI. Conclusion

This thesis explores how various communities of econometricians used household-behavioural data since the interwar period and creates a new chunk of the history of microeconometrics. Like charting the history of rock-and-roll, the foundation of household microeconometrics between 1920 and 1960 can be understood as multiple themes of collective contributions. Early influences were indeed many. Demand analysts may regard the Bowley-DAE literature as the ancestors of large-scale regressions of microdata (Chapters II and III). Some may attribute Orcutt's microsimulation as a pathbreaking innovation of a bottom-up micro-oriented methodology (Chapter IV). From these accounts, this thesis does not intend to propose a single story about the practical movements toward microdata. Instead, the foundation of household microeconometrics entails stories of multiple communities, where econometricians worked back-and-forth between microdata, machines, theories, and existing empirical programmes.

The model of empirical knowledge production offers an illuminating framework for capturing the historical complexity of microeconometric practices. The intellectual journeys of Bowley, Stone, and Orcutt have shown how they extended their empirical minds to actions and reshaped the materialistic supply and intellectual demand. Their final practices were taken as exemplars that triggered other microeconometricians in their respective communities. This process of knowledge accumulation suggests that these materialistic and intellectual elements are crucial for the contextualisation of every story, and these stories are necessary for making the whole historical narrative credible.

The first part of Chapter II presented Bowley's story as an interesting case of a British econometrician in the interwar period when the microeconometric analysis was at the crossroads of the Booth-Rowntree tradition and neoclassical economics. Starting as a Fabian-inspired statistician, Bowley was initially more into the Booth-Rowntree tradition, which led him to introduce the randomisation technique into poverty surveys. With these self-collected surveys, the primary aim of his proto-econometric analysis was to inform the real-world situation of poverty. Nevertheless, he hesitated to apply microdata to economic inferences. What triggered Bowley's microanalysis of the Engel curve was the development of economic

theorisations. Bowley's 1935 study on family expenditure was due to the appearance of government-run budget surveys and his intention to test the new Hicks-Allen neoclassical thesis.

As discussed in the second part of Chapter II, along with Bowley's story, other attempts to estimate demand curve and consumption function characterised interwar microeconometrics. Econometricians continued to search for a valid method to transform the budget materials into other theoretical entities with limited data supply. The two methodological lessons were that deriving the market demand suffered from the translation problem between the income-expenditure curve and price-quantity curve; and that aggregating the Keynesian consumption was impossible without income distribution. As a result, Marschak's 1943 pooling method was developed in response to the earlier practices that constructed an empirical framework synthesising both the macro- and micro-level materials in demand studies.

Stone's story in Chapter III provides an early counterexample of the time when most econometricians were delving into macroeconometrics. Stone's early trigger was Great Depression; he wished that economics could be an applied science that stressed data collection and empirical analyses. His national accounting estimates with Meade provided the new data source for his 1945 demand macroanalysis. From the works of Durbin-Watson and Orcutt-Cochrane, Stone was gradually dissuaded from the validity of macro-level data. With the new microdata and the EDSAC, Stone's 1954 book with Rowe was a hybridisation of micro- and macroeconometric demand analysis. The empirical method adopted Marschak's pooling method as an exemplar that contrasted with the sole reliance of Haavelmo-Cowles approach's on macro-level estimates. Meanwhile, Stone was keen to follow the Haavelmo-Cowles framework to construct a compact and simultaneously determined demand system, and the linear expenditure system was his final answer.

At the DAE, Stone was like a captain of the rowing club, who allocated the affiliates to different boats. Under Stone's leadership, econometricians established working groups that initiated the new age of econometric collaborations. The crew members contributed to their research concerns under a distinct division of labour regarding data collection, econometric problems, and computer programming. Once new instruments or materials were constructed, they could be soon used by other teams in their scientific works. Such a pattern of collaboration facilitated the production of empirical knowledge. Eventually, this collective efficiency benefited the household microeconometrics of Prais and Houthakker on the Engel curve and of Brown and Aitchison on income distribution. In the mid-1950s, the DAE became one of the top-tier research institutions in econometrics.

As documented in Chapter IV, another attempt that contrasted the Haavelmo-Cowles approach was Orcutt's microsimulation, derived from his engineering background and inspired by Tinbergen's macroeconometrics. Despite Orcutt at the DAE developed the Cochrane-Orcutt transformation as a solution for the autocorrelation problem of time series, he came to suspect the usage of macro-level data in constructing a compact system of the economy. In the early-1950s, he developed a series of methodological criticisms of the Haavelmo-Cowles approach and later shifted to using the microanalytic Monte Carlo approach and the IBM 704 to model the economy. With the aid of Michigan's consumer finance data, he finally finished the first demographic microsimulation in 1961. In Orcutt's view, microsimulation was the actual realisation of his Tinbergen dream and an analytical tool that produced more credible evidence to substitute the macro-estimates.

Orcutt's grand project of microsimulation brought him to Wisconsin, where he founded the SSRI. However, the SSRI's institutional setting as an umbrella institute hampered the research process of microsimulation. Although the SSRI had progressed its empirical works on the durable demand, the multi-centred research institute did not upgrade the microsimulation model to incorporate the household sector. After he left the SSRI, Orcutt finished the model in 1976 at the Urban Institute; there, he benefited from new microdata and improved computing power, an integrated research team, and most importantly, long-standing financial support from the federal government. These supports were derived from the intellectual demand of the programme evaluation officials when the U.S. economic policies switched to an activist style of domestic programmes since the mid-1960s.

Drawing upon these thematic accounts, what were the foundations of household microeconometrics from a bird's-eye view? Perhaps a proper analogy would be to illustrate this bigger picture as a jigsaw puzzle, where historical pieces are *to some extent coherent* but *locationally independent*. Though not exhaustive, Chapters II–IV provided pieces that are encouraging to solving the household microeconometric puzzle. At first glance, the interwar literature shows how various historical pieces brought up the initial picture. A classic example was the Allen-Bowley study, which originated from the poverty survey tradition, Engel curve, theory of statistics, and the neoclassical analysis of income and expenditure. In other demand studies, learning from unsuccessful attempts to obtain the demand curve and consumption function, Marschak gradually crystallised a synthetic approach to situate microdata in the demand macroanalysis. The interwar cases were the best fit to the jigsaw analogy. It was the period when econometricians were looking for different but complementary pieces of economic phenomena, and missing any one of the stories will obscure the whole picture.

Next, in the mid-1940s, the community turned to the Haavelmo-Cowles time-series analysis following the outflow of national income data, which Orcutt and Cochrane later proved problematic. Under the dominance of the Haavelmo-Cowles approach, the post-war microeconometric studies followed two paths. While Marschak's method only lingered as an impractical task in Stone's 1945 demand macroanalysis, Bowley's empirical framework of the Engel curve was extended by Houthakker and Prais in the early 1950s. Due to the DAE's collective effort, the 1954 Stone-Rowe demand synthesis was the final unification of these pieces that applied the Cochrane-Orcutt transformation to time series, modelled the market demand in Marschak's manner, and estimated the income elasticity like Houthakker and Prais.

Lastly, after leaving Cambridge, with his distrust of the Haavelmo-Cowles programme, Orcutt proceeded with a more radical means of aggregating microdata. In contrast to macroeconometric modelling, his microsimulation programme adopted the ideas of engineering analogy, Wold and Tinbergen's recursiveness, and the Monte Carlo method, where the last was inspired by Slutsky's random summation and the British sampling experiment tradition. While these pieces were distinct intellectual products derived from the Bowley-Houthakker-Prais and Marschak-Stone lines, Orcutt's innovative microsimulation undoubtedly added a new part to the whole picture of household microeconometrics.

In sum, the evolution of household microeconometric practices between the 1920s and 1960s suggested a big picture that can be disassembled into many historical pieces. Although the research themes and historical details in every piece could be completely different, these unique pieces were interconnected – like a jigsaw puzzle – through common materials and exemplars in the scientific community.

Continuing with this jigsaw analogy, constructing the picture of microeconometric practices turns into collecting and unifying these historical pieces. First, the model of empirical knowledge production helps to build more comprehensive narratives from both micro- and macro-historical perspectives. For instance, Chapters III and IV argued that the five historical statements below are equally and separately relevant for understanding the empirical works of Stone and Orcutt:

(S1) The availability of pre-war expenditure surveys and the EDSAC helped to produce the econometric estimates of Stone and his DAE colleagues.

(S2) IBM 704 and the Monte Carlo technique were essential backgrounds of Orcutt's 1961 demographic microsimulation.

(S3) Stone's demand synthesis could be driven by his unwillingness to apply solely macro-level data in deriving the demand equation.

(S4) Orcutt's disbelief of the Cowles programme gave birth to his microsimulation.

(S5) President Lyndon Johnson's 'Great Society' proposal aided Orcutt's 1976 DYNASIM at the Urban Institute.

S1 and S2 highlight the materialistic supplies, and S3–S6 stress the intellectual demands, which can be further categorised into personal incentives (S3 and S4) and social forces (S5). Traditionally, S3 and S4 are often considered the domain of internal histories, whereas S1, S2, and S5 are external histories. For the former, histories of macroeconometrics are complementary pieces in understanding household microeconometrics. The main reason is that most early econometricians often worked in ways that crossed over applications using macro- and micro data. In that regard, new macro-materials and discoveries affected the demands and supplies for microeconometric knowledge. On such a development, Stone and Orcutt are representative cases, switching between the application of macro or micro data in the context of household microeconometrics. For the latter, other external factors can be further decomposed into various historical objects that contain their unique historical contexts of knowledge construction. For instance, the presence of EDSAC, Monte Carlo technique, and 'Great Society' proposal. When other factors also bear their histories of practices that can be approached in the same regard, the main task of historians of economics is to reconstruct all historical pieces with different levels of evidence and then put those pieces together to complete the puzzle.

To further unify the insights in the historical picture constructed in Chapters III and IV, Chapter V furnished a rigorous empirical framework using citation and network analysis. Bibliometric data from the DAE and SSRI reprint series were applied to understand community relations and how the literature was formed by citing exemplars. The first aim was to understand the Stone-Orcutt contrast: At the DAE, Stone and his crew of econometricians worked mainly as an independent body, distinguished from other economists of the Cambridge Circus, i.e., the Faculty of Economics, whereas at the SSRI, Orcutt wished that the SSRI could be a focused research group on microsimulation and an umbrella institute for social science research. The empirical results from the network analysis show that their community structures were hardly similar. The DAE was a more interconnected community than the SSRI in terms of collaboration, bibliometric coupling, and acknowledgement. Quantitative evidence verifies that the Stone-Orcutt contrast in institutional setting led the community toward different directions.

To explore the Stone-Orcutt contrast, Granovetter's theory and acknowledgement data were applied to test the differences in leadership style. Stone and Orcutt played different roles in

transmitting information in the acknowledgement network: Stone tended to benefit his weak ties that was indeed like a caption of the club, whereas Orcutt was almost thanked by his strong ties that was akin to the coxswain of a boat.

However, it should be noted that despite the differences in community structures and leadership styles, the findings do not suggest that any one leadership was superior to the other. In fact, both directorships of Stone (1946–55) and Orcutt (1958–66) did not last long. In Stone's case, the segregation between the Cambridge Crew and Circus yielded outstanding research outputs in econometrics; nevertheless, such distinction accelerated the political controversy between the empirical and theoretical camps of economists that eventually brought Stone down. In Orcutt's case, the seemingly inclusive institute impeded his microsimulation project and eventually halted funding opportunities. Disintegrated and slow research progress fostered his departure from Wisconsin to search for a better position without administrative duties. Therefore, while they are both representative stories of how a powerful figure built a scientific community from scratch, the evidence only shows how scientific communities accommodated their institutional features and then contributed to the literature of household microeconometrics.

The network visualisations not only extract interesting historical lessons on the academic leadership but also provide new possibilities of studying the collective outcome of research institutions. As illustrated in Figures 11–13, the DAE and SSRI were comprised of different research groups focusing on their respective tasks. The collective nature of these collaborations indicates that the style of directorship may not have been the only factor in forming interpersonal networks. In this sense, the current individual-centred analysis can be reinterpreted as stories of multiple clusters where institutions are more distinct units of analysis.

The bibliometric information and network measurements have limitations. First, to validate the measurement comparisons between the DAE and SSRI, one needed to assume that these two groups of literature share approximately similar patterns of collaboration, citation, and acknowledgement. In other words, it was assumed that economists' preferences for these actions in building academic ties were stable from the 1950s to the 1960s. However, the current investigation cannot provide a comprehensive rationale for this assumption due to the lack of relevant knowledge. Second, supervision ties were a missing element in the network measurements. Although this relationship was applied to approximate Granovetter's strong tie, the present network analysis does not account for supervisor's influence in measuring academic ties. The practical difficulty of measuring supervision ties is that it contains too few

observations to form a personal network. In addition, the concepts of supervisor might vary in the doctoral trainings of the US and UK.

Addressing these limitations requires an overall examination of the economist's behavioural patterns of collaboration, citation, acknowledgement, and supervision. What was the collaboration pattern in econometrics from the 1930s? Did econometricians' reference habits change over time? What does an acknowledgement really represent? How does one evaluate supervisions in various institutional contexts? These questions have not yet been answered,[215] but any further understanding of them will help to revise the network measurements of this chapter.

Studying the citation relationships between microeconometric practices and their exemplars offered new evidence on the community relations and knowledge accumulations. The co-citation analysis and the listening-talking dichotomy inspired by Howlett (2008) helped to locate notable exemplars which were actual triggers for the practitioners. After limiting the samples to household microeconometrics, the DAE still contained more internal citations than the SSRI within the literature, suggesting that the institutional contrast founded in the network analysis remained consistent in the listening-talking citation analysis.

The co-citation analysis indicates that the main trigger of the DAE literature was the 1935 Allen-Bowley study; for the SSRI, it was Orcutt's 1961 microsimulation. Nevertheless, the story changes when applying the listening-talking dichotomy: the DAE citations on the Bowley-Allen study were primarily talking-cited, while the SSRI on Orcutt's study were mainly listening-cited. Even after expanding the analysis to the whole microeconometric sample, the DAE still contained more talking citations than the SSRI in general. To this end, the listening-talking analysis offers a new way in studying academic interactions within the empirical literature.

The explanations for the difference in their talking citations may be twofold. First, the two institutions varied in the sense of progressiveness, meaning that compared with the SSRI, the DAE was more willing to reflect on past exemplars. Second, the citation habits and styles differed between England in the 1950s and the US in the 1960s, but subscribing to this hypothesis requires a detailed historical analysis of econometric citations. As discussed above, since the relevant research on citations is still few, the current evidence cannot judge which

[215] There have been some attempts to understand collaboration (e.g., McDowell & Melvin, 1983; Barnett, Ault, & Kaserman, 1988; Piette & Ross, 1992; Medoff, 2007), citation (reviewed in Section 1, Chapter V; Hamermesh, 2018), acknowledgement (Rose and Georg, 2021), and supervision (Svorenčík, 2014); however, these studies do not fit into the context of this thesis.

one is a better explanation. Nevertheless, the current framework is a rigorous first step on the historical and empirical issue of econometric citations.

The framework of citations analysis, however, bears two limitations. First, the analysis relies on a relatively small sample of citations. The historical inferences may suffer from the underdetermination problem when the observations are too few to verify a stable relationship. Second, the analysis adopted the citation data as the only material in searching for exemplars, but how an exemplar becomes paradigmatic can also explain with other internal and external factors. For instance, as documented in Chapter IV, Orcutt read Slutsky's article on random summation, and then he realised the distinction between random and autocorrelated series. While the Slutsky paper for Orcutt was by all means an exemplar, he barely cited the paper in his research. Given the two constraints, evidence from the citation analysis should not be overinterpreted and is primarily supplementary to the qualitative evidence.

Despite these limitations, the citation evidence may be helpful in informing some missing contexts from the qualitative study. For instance, since the qualitative evidence showed that Orcutt failed to redirect his fellow members to focus on microsimulation, it may indicate that Orcutt's intellectual impact at the SSRI was smaller than Stone's at the DAE. However, given that Orcutt's 1961 study was the most co- and listening-cited exemplars in the community, this fact verifies Orcutt's paradigmatic influence among the SSRI practices. To this end, the evidence offers another point of view for understanding personal contributions to microeconometric practices.

The empirical framework proposed in Chapter V can be further applied to study the relations and consolidations of other scientific communities. As illustrated in the chapter, the network and citation analysis that utilised all possible bibliometric information were apparently replicable. What is more difficult is how to locate a suitable scientific community and literature of investigation. While the bibliometric analysis only offers one approach to a typical element of the historical piece, relevant knowledge of the history of that community and an overall understanding of that literature are both indispensable. Based on that criterion, the literature of macroeconometrics and Chicago's labour economics are potential candidates for further network analysis and, in particular, the application of the listening-talking analysis. Some examples may be the formations of the Haavelmo-Cowles and Chicago approach, the centrality of Marschak in the Cowles commission and Chicago economists in their department, and the relationship between the Cowles econometricians and Chicago economists. While their qualitative histories are to some extent studied, further empirical histories can supplement the existing historical analyses for a broader picture.

Apart from this thesis's finding, there are other historical pieces yet to be investigated in completing a more comprehensive picture of microeconometrics: histories of microeconomic policymaking, other household and firm microeconometrics, and empirical labour economics.

First, an interesting but only slightly explored subject in this study is how microeconometric practices corresponded to the intellectual demands of microeconomic policymaking. As shown in Chapter IV, Orcutt's microsimulation did not receive much attention from the government until the need for domestic policies became more pressing. Since the mid-1960s, U.S. economic policies gradually shifted to an activist style of domestic programmes. Led by Kennedy's New Frontier and Johnson's Great Society, the Democrats were looking for allocating more government spending in improving poverty, education, and medical care. Following the rising need of evaluating social programmes, microsimulation stood out as a new tool for generating evidence. This driving force of policymaking could impose not only on microsimulation but on other practices examined in this thesis, such the democratic socialism in Interwar Britain on Bowley's work and the idea of post-war reconstruction on Stone's demand analysis. To this end, results from Chapters II and III need to account for more historical contexts in policymaking.

Second, two communities missed in this thesis are the SRC at Michigan and the Cowles Foundation at Yale, led by George Katona and James Tobin, respectively. As shown in Chapter V, econometric practices from these two institutions were taken as SSRI's exemplars. Back in the 1950s, econometricians at Michigan had developed some microanalysis with the advantage of consumer behaviour data, and Tobin had started using the Michigan data before becoming the director of the Cowles Foundation in 1955. Further historical studies on these practices and communities at Michigan and Yale may be the next step to modifying the obtained picture of household microeconometrics.

Third, as mentioned in Chapter I, the history of using firm-behavioural data was not chosen due to its historical coherence. This empirical literature relies on the examinations of firm-size distribution and Gilbrat's law and one of the contributors is Sigbert Prais. It may be interesting to examine Prais's role in this literature to link it with his studies of the UK's Engel curve. Furthermore, some stylised facts suggest that economists started to apply firm-level census in the 1970s, and Gunnar Eliasson formulated the first microsimulation study on the Swedish firms in the mid-1980s. Those historical pieces still call for unifications from wide aspects of historical study and bibliometric evidence.

Finally, another gap in the history to be filled is how labour economics evolved from the Chicago approach to the credibility revolution literature. Recent histories have suggested that

Chicago and Princeton are two important institutions in this development. The development of neoclassical and Chicago labour economics has been explored by the secondary literature (Heckman, 2001; Boyer & Smith, 2001; Kaufman, 2010; Heckman, 2017). The literature indicates that the initiation of labour economics at Chicago was linked to H. Gregg Lewis, the central figure of Chicago's econometrics in the 1960s. Afterward, he was replaced by James Heckman, who is famous for his Heckman correction of selection bias. The secondary literature does not fully answer the foundations of these practices. In addition, as Angrist and Pischke (2009) have suggested, the credibility revolution may be started by Orley Ashenfelter of the Industrial Relations Section at Princeton University since the 1980s. However, while the development of the toolkit used during the revolution has been recorded by Panhans and Singleton (2017), Princeton's intellectual impact on the revolution is still not fully explored.

The framework of empirical knowledge production has the advantage when reconstructing the history of both communities in empirical labour economics. Series of research questions asked in this study can be transplanted there. What microdata did they apply? On what computer? Who did the programming? What theory drove the empirical concerns in the community? What kind of relationships were they looking for? Who were the central figures and mediators of the group? What exemplars were primary triggers for other members? How were these exemplars cited and transmitted? Once clarifying these questions, it is possible to revise a more comprehensive account of the history of individual-behavioural data and demystify the credibility revolution in the 1980s.

Although not exhaustive, this thesis contributes to a new account for the history of microeconometrics. Focusing on household-behavioural data, the historical analysis relies on the theoretical model of empirical knowledge production and the empirical framework of network and citation analysis. The first three substantial chapters addressed unique historical themes from the interwar period to the 1960s to understand how communities of microeconometricians constructed their practices with their specific data, technology, and research concerns. Then, with bibliometric data, the last chapter developed an original citation approach and techniques (listening-talking analysis) for analysing the closeness of econometricians' communities and formations of microeconometric literature. As the current findings appear to be promising, the theoretical and empirical frameworks can be further applied to other historical contexts to solve the jigsaw puzzle of microeconometrics.

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