# Aggregation Problem in Demand Analysis, 1930s-1950s

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**Abstract**

This article examines the emergence of the representative agent as the outcome of transformations that occurred in microeconomics in the 1930s-1950s years, especially in the subfield of demand theory. To tell this story, I begin with a particular historical interpretation of this subfield, propounded by Wade Hands and Philip Mirowski in the 1990s, known as the Hotelling-Schultz Impasse. Although this impasse was abandoned by the end of the 1930s, the testing of the Symmetry Restrictions and the validity of the Integrability Conditions continued to draw the attention of different research centers. The Cowles Commission, represented by its research director, Tjalling Koopmans, played an important role during this stage and, more to the point, in the subsequent emergence of the representative agent in microeconomics through their approach to aggregation problem. The significance of Paul Samuelson's introduction of homothetic preferences into General Equilibrium Theory and its connection to Koopmans's writings during the 1950s is also emphasized.

**Keywords**: Representative Agent, Aggregation Problem, Tjalling Koopmans

**Resumo**

O presente artigo examina a emergência do agente representativo como resultado da transformação que ocorreu na economia nos anos de 1930 a 1950, especialmente no subcampo da teoria da demanda. Para contar essa história, eu começo com uma interpretação histórica particular proposta por Wade Hands e Philip Mirowski nos anos de 1990 conhecido como o Impasse de Hotelling-Schultz. Embora esse impasse tenha sido abandonado ao final da década de 1930, o teste da Restrição de Simetria e a validade das Condições de Integrabilidade continuou a chamar atenção de diferentes centros de pesquisas. A Comissão Cowles, representado pelo seu diretor de pesquisa, Tjalling Koopmans, teve um papel importante durante essa fase e, além disso, na emergência subsequente do agente representativo na microeconomia através do problema de agregação. A significância da introdução das preferências homotética por Paul Samuelson na teoria do equilíbrio geral e sua conexão com os escritos de Koopmans durante os anos de 1950 também é enfatizado.

**JEL Codes**: B2, B21, B23

Área 1 - Metodologia e História do Pensamento Econômico.

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# 1 Introduction

> *Samuelson is omnipresent in American and even world economics; (...) he appears at every turn of history and in every disguise. The unwilling college student (...) finds him there, expounding economic wisdom in eight hundred well chosen pages (1967). The graduate student is disciplined to our trade by study of the Foundations (1947) and two dozen assorted papers of Samuelson. In the pages of every journal and many a collective volume he appears, read equally by esoteric specialists in optimal growth or integrability conditions and by the most policy-oriented in practical central banking, impatient with theoretical niceties.*
>
> - Kenneth Arrow, *1967*

> *(...) microeconomic theory begins with choices. Indeed, the theory not only begins with choices; it remains focused on choices for a very long time.*
>
> - David Kreps, *2013*

Consumer rationality is a cornerstone of microeconomic theory. The path to this outcome begins with axioms of preferences, proceeds through the existence proof of utility functions and continues with the solution of a (constrained) utility maximization problems that leads to Marshallian demand functions. Such functions are said to satisfy some desirable properties. It is possible to attain an equivalent result in a (constrained) expenditure minimization problem, in which case Hicksian demand functions are obtained.

The connection between Hicksian and Marshallian demand functions is attributed to a mathematical expression proposed by Eugen Slutsky, a fundamental equation that breaks the quantity demanded of a good—due to variation in its price or in another good's price—into substitution and income effects. The symmetry of the Slutsky matrix, along with the Integrability Theorem, guarantees the reversal of Marshallian demand functions to the axioms of preferences, thus, connecting the solution of the utility maximization problem with its initial assumptions.

This is at least the standard story one learns in undergraduate and graduate microeconomics textbooks. Practicing scientists, however, are rarely good historian of their fields. The consumer theory as we know it today developed out of a more intricate road than the story presented above suggests. Through a careful look into the works and careers of particular economists one can observe how—operating in networks—they created and modified knowledge that are frequently lost in the midst of standard narratives.

My goal in this paper is to tell one of these stories. It focuses on the rise of the representative consumer as an outcome of transformations that occurred in the subfield of consumer theory. Specifically, the representative agent appears in this narrative in the debates surroundings aggregation problem in the 1930s, 1940s, and 1950s, and key to this developments were the Dutch physicist Tjalling Koopmans. However, as Kenneth Arrow remarked about Paul Samuelson's 1967 *Collected Scientific Papers*, this essay also examines the degree to which Paul Samuelson helped shape this debate. I show that a significant part of Tjalling Koopmans's research in microeconomics was informed by Paul Samuelson's research in the field and this was especially so concerning the representative agent.

I begin this narrative with an important episode in the development of demand analysis in the 1930s, the "Hotelling-Schultz dialogue," a research effort that consisted in, among other objectives, testing the symmetry conditions of Hotelling demand functions (section 2.2). With the advent of contributions from Eugen Slutsky, the Cowles Commission for Research in Economics (1939-1955) conducted more than the testing of the new symmetry conditions. Under the influence of the Keynesian Revolution, the Slutsky income effect (defined at the microeconomic level) began to be used to shed light on facts defined at

the macroeconomic level. This objective, part of the Slutsky-Walras program, engaged Koopmans at least since the 1940s. I explain why this program was centered on the problem of aggregation and, above all, how from this effort a few considerations about the representative agent can be inferred (section 2.3). To bring further details into this analysis, I connect Koopmans's research efforts with Samuelson's, which also helped give rise to the representative agent (section 2.4). In section 2.5, I consider the meaning of dynamics in economics, a topic originating concomitantly in a important paper analyzed in this essay (Samuelson 1943), with interesting implications for the interpretation of the representative agent. In section 2.6, I provide some concluding remarks.

## 2 Data-Theory gap in early Demand Analysis

In their historical quest to trace the rise of orthodox demand theory in the United States in the postwar period, Wade Hands and Philip Mirowski have identified a point of origin in the writings and interactions of two "relatively minor" figures. Everything began in depression-era North America when Henry Schultz and Harold Hotelling set out to find, *inter alia*, a theory capable of underpinning the empirical demand functions they had been trying to estimate.[1] One upside of this story is that the search for answers set forth by this research agenda led to the foundation of three important hubs of American neoclassicism, still influential to this day. Hands and Mirowski have dubbed this episode the "Hotelling-Schultz dialogue" or the "Hotelling-Schultz impasse."

The first person in this story was Henry Schultz, born in 1893 in Russian-occupied Poland.[2] After attending college at the City University of New York, Schultz began graduate work in economics at Columbia University under Henry Ludwell Moore, writing a dissertation on the statistical estimation of demand functions.[3] With the conclusion of the doctoral degree, Schultz was appointed professor of economics at the University of Chicago, where he carried out the major work of his career by founding a statistical laboratory to deepen the estimation of demand functions, "(...) a large program of research (...) definitely projected by earlier studies" (Hotelling 1939, p. 98).[4]

In need of finding a reasonable theory behind the estimated demand functions, Schultz began to look into the general equilibrium approach pioneered by León Walras and Vilfredo Pareto. In his mind, such functions were not only interdependent at the market level, but resulted from the spending decisions of consumers who maximized their utility function (exactly *à la Walras*). As in the natural sciences, there had to be "laws" behind empirical regularities in microeconomics as well. What Schultz did not realize at the beginning, nonetheless, was how difficult his applied task would turn out to be, let alone associating it with the Walrasian approach he so dearly praised.

A second contributor in those early studies of empirical demand analysis was Harold Hotelling. Born in Minnesota in 1895, Hotelling was raised in Seattle, Washington. Graduating in journalism in 1919, he went on to find a more fulfilling career in mathematics,

[1] Wade Hands and Philip Mirowski told this history in two separate articles, "Harold Hotelling and the Neoclassical Dream" and "A Paradox of Budgets: the Postwar Stabilization of American Neoclassical Demand Theory," both published in 1998. In line with the alphabetical protocol, I will simply use "Hands and Mirowski" when referring to this story.

[2] His family emigrated to New York City when he was still a child. See Hotelling (1939) for more information on the life of Henry Schultz.

[3] In a Festschrift to Schultz published after his death, Harold Hotelling (1939, p. 98) wrote: "[Schultz's] inspiration was in the work of Henry L. Moore, whoose piooner attempt to derive demand curves from time series stirred his enthusiam." Henry Moore (1869-1958), considered one of the founding fathers of econometrics, earned a PhD in Economics from Johns Hopkins University. Spearheading a generation of Americans who crossed the Atlantic to learn from Europeans, Moore took classes in mathematical statistics from Karl Pearson at the University of London and also corresponded with luminaires of that time such as Alfred Marshall, Francis Ysidro Edgeworth, Vilfredo Pareto and Léon Walras. On this and additional details of Moore's career, see Mirowski (1990).

[4] Schultz taught at the University of Chicago from the fall of 1926 until his untimely death in 1938. His name is often associated with the early formation of the Chicago School of Economics.

earning a master's degree in 1921. Three years later, Hotelling completed a doctorate also in mathematics at Princeton University with a dissertation on *analysis situs*, known nowadays as topology, under the mathematician Oswald Veblen.[5] As he would later recall of this graduate experience, after being rejected by the graduate program in economics at Columbia University, a fact that postponed his immediate desire to apply the science of mathematics to uncover "new truth" in economics, studying subjects as wide as mathematical physics, differential geometry and astrophysics gave him an even better grasp of the tools which would turn out to be helpful in his subsequent works (Darnell 1988).

Leaving university in 1924, a period of scarce jobs for pure mathematicians, Hotelling began his professional life at the recently created Stanford Food Research Institute (SRI) as a mathematical and statistical consultant (Hands and Mirowski 1998). It was at the institute, collaborating with Holbrook Working and observing first hand the widespread economic crisis and uncertainty (especially in the food sector), that Hotelling developed his own interests in demand theory and estimation methods.[6] In other words, as Arrow and Lehmann (2012, p. 903) later remarked, "it was during his Stanford period that he began to focus on the two fields—economics and statistics—in which he would do his life's work."

In 1929 Hotelling spent six months in England working with Ronald Aylmer Fisher, a leading statistician of his time. This travel made Hotelling "one of the few Americans who in the 1920s realized the revolution that R. A. Fisher had brought about in statistics" (ibid.). So long-lasting was this experience that following his relocation to Columbia University in 1931, "(...) most of his energy during the 15 years there was spent developing the first program in the modern (Fisherian) theory of statistics" (ibid.), a fact that certainly helped Hotelling hone his publications in economics. The bulk of Harold Hotelling's involvement with Henry Schultz took place during Hotelling's Columbia tenure, precisely when, serving as editor of the *Journal of Political Economy* in 1932, Schultz received an article written by Hotelling titled "Edgeworth's Taxation Paradox and the Nature of Demand and Supply Functions."[7]

Henry Schultz had been striving to find out whether the Walrasian model could fulfill the theoretical void he saw in the estimated curves. Walras's approach regarding the interdependency of commodities was a better choice than the Cournot-Marshall law of demand (which was a special case of the first), since it would not be "possible for any buyer to make up his demand schedule for a commodity without knowing the prices of competing commodities" (Schultz 1937: 7). Schultz found a perfect interlocutor in Hotelling, for Hotelling's article tackled precisely the same objective.[8]

Although Hotelling searched for a theoretical approach, while Schultz championed "em-

[5]Oswald Veblen (1880-1960), nephew of the Institutionalist Thorstein Veblen, earned a PhD in mathematics at the University of Chicago in 1903. A famous geometer of his time, he contributed with the first ever modern treatment of Topology (Darnell 1988; Mac Lane 1964). Having taught at Princeton University his whole career, Oswald Veblen helped found the Institute of Advanced Study's School of Mathematics, a place famous for its faculty who included such names as Albert Einstein, John von Neumann and Hermann Weyl.

[6]Holbrook Working earned a PhD in Agricultural Economics from the University of Wisconsin-Madison in 1921 and joined SRI in 1925. Having made substantial contributions in the statistical analysis of commodity prices (JBES 1986), his influence on Hotelling's 1932 article can be gauged by the several times he was mentioned. Holbrook and his younger sibling, Elmer Working, were also two of the first economists who underscored the problem of *endogenous explanatory variables* in the estimation of demand functions. Deaton and Muellbauer's *Almost Ideal Demand System* is built, among other models, on Holbrook Working's "Statistical Law of Family Expenditure," published in 1943. Elmer's article, "What do Statistical 'Demand Curves' Show," published in 1927, remains to this day a valuable text in applied microeconomics courses such as Industrial Organization.

[7]As Hands and Mirowski (1998) have reminded, Hotelling and Schultz had already exchanged correspondence as early as the 1920s, but on other matters.

[8]Harold Hotelling's article came out in the *Journal of Political Economy*, published by the University of Chicago's economics department, where Henry Schultz was a Professor. The Journal was established in 1893. See the special issue edited by John List and Harald Uhlig (2017) on the combined influence on the economics profession of "Chicago economics" and its flagship journal.

pirical operationalism" (Hands and Mirowski 1998, p. 363), their shared comprehension of the historical context and the role economics played in ameliorating people's living condition outweighed any substantive differences. They were both aware of the reality plaguing the American countryside in the years leading up to and after the 1929 great depression.[9] Investigations into the workings of demand analysis should, they thought, prove instrumental in understanding the crisis. More than pushing for a neoclassical theory of value and exchange grounded in mathematics, both believed that it should be put to test.[10]

Henry Schultz and Harold Hotelling pioneered the use of linear regression analysis as well as Fisherian statistical hypothesis testing in demand theory. As Schultz commented in the preface of his 1938 book, although economists assumed the existence of demand functions is their explanations of economics events, they were unknown. Estimating demand functions was therefore paramount in testing theories. An important part of the ensuing development revolved around a key test called *symmetry condition*. But as much as it was the economist' job to correctly find interdependency empirically and base it upon the writings of Léon Walras, Vilfredo Pareto and Francis Edgeworth, things did not work out well in the past. As I will discuss further below, things does not work out well in the present either.

In the section 5 of his "Edgeworth's Taxation" paper, Hotelling demonstrated that a certain *symmetry condition* should hold if one took the cross-partial derivative of demand functions with respect to prices, a feature he alternatively dubbed *integrability conditions*. After reading the article, "Schultz immediately set his laboratory to work calculating the demand functions and estimating the partial derivatives to test for the symmetry condition of the Hotelling Economy" (Hands and Mirowski 1998, p. 355). This attempt to test the restriction is arguably one of the first carried out in demand analysis. After it acquired "the status of core empirical content," a condition definitely worth testing, the symmetry (or integrability) condition:

> (...) had frequently failed to hold, and worse, sometimes the sign differed, so the complementarity coefficient would give conflicting results when the order of calculation was inverted (ibid.).

When Schultz's tests went awry, several attempts to save the Hotelling economy were made. These efforts included, but were not limited to, changes in the functional forms of the demand equations, adoption of different estimators (other than the method of Least Squares), reinterpretation of the integrability question in economics, or even the whole disposal of the Hotelling framework. One problem, however, did not go unnoticed: aggregation problem.

Commenting on the set of objectives of the Arrow-Debreu research program (as advanced by Hands and Mirowski), Leonid Hurwicz reminded that the problem of aggregation "seems to have status akin to that of the five goals" (Hurwicz 1998, p. 399, italics added).[11] and went on to add:

> The issue of *aggregation* has, I believe, special status because of its relationship to econometric research. Since much econometric research (and this, I believe, includes the studies of Henry Schultz) uses data aggregated over certain populations or collections of firms, there arises the question to which properties known to hold for individual agents carry over to aggregates. One

[9] As the Stanford University historian David Kennedy once wrote about that period: "Herbert Hoover needed no comprehensive study to know that the farm issue was urgent. Virtually his first act as president, even before he commissioned his wide-ranging examination of recent social trends, was to convene a special congressional session to resolve the farm crisis" (Kennedy 1999, p. 17-18). Then, he continued, "(...) as the agricultural depression of the 1920s merged with the great depression of the 1930s, (...) the misery of rural America knew no relief" (ibid., p. 19).

[10] See, in particular, Schultz (1938).

[11] The Arrow-Debreu research program are listed as existence, uniqueness and stability of competitive equilibria; comparative statics, and welfare economics

> reason why this issue is important is that identifiability assumptions used (and needed) by econometricians are often derived from theoretical (or introspective) considerations applicable to *individual* economic agents (Hurwicz 1998: 401, italics added).

The data-theory gap was also the leitmotif of Theil's 1954 *Linear Aggregation of Economic Analysis*.

Koopmans's MRG issue?

A significant part of the Hotelling-Schultz dialogue lost its importance with the unexpected death of Henry Schultz in 1938, the definitive retreat of Harold Hotelling from demand theory, and, indisputably the most important factor, the rediscovery of Eugen Slutsky's 1915 "Sulla Teoria del Bilancio del Consumatore." This text provided the vital step in the proof to recover consumer preferences from her/his demand behaviour.[12] Then, when the Slutsky equation finally made its way into orthodox microeconomics (in the postwar period), becoming the "single most important result" in demand theory (Hands 2004) or its "fundamental equation" (Jehle and Reny 2011), another struggle began: this time to test the Slutsky symmetry conditions.[13]

## 3 Data-Theory gap in the Interlude, 1938-1948

The data-theory gap continued to be a problem after the demise of the Hotelling-Schultz dialogue. Chipman (2006a) provided a substantial overview...

Historians of economics have placed a significant emphasis on the profession's struggles with aggregation problem in the postwar period (see, for example, Chipman (2006), van Haal and Walker (1990), Backhouse (2002)). But Harold Hotelling and Henry Schultz had also identified aggregation problem as one of the likely factor getting in the way of testing correctly the Hotelling's symmetry conditions in the 1930s. This section examines other paths by which different protagonists, representing different institutions, managed to circumvent it. I begin with Cowles Commission's attempt at testing the Slutsky's restrictions and then move on to discuss Paul Samuelson's incursion through a whole different trail: the Revealed Preference Theory.

With the demise of the Hotelling and Schultz research programme in 1938, three (relatively) independent research centers in the U.S. carried on the search for solutions regarding the relationship between utility-maximizing problems and the law of demand by tackling, among others questions, the testing of the Slutsky symmetry conditions and how to best use the Slutsky-based demand functions to make sense of two relevant and intertwined topics of the day. First, the Keynesian Revolution and, second, the ensuing challenge regarding the of compatibility of macroeconomics with Walras's microeconomics.

On the first topic, there was a general perception in the United States of the revolutionary ideas presented in John Maynard Keynes's 1936 *General Theory of Employment, Interest, and Money*. His theory of aggregate income became not only an important reference to comprehend business cycles but was seen as providing the needed tools to smooth them out, particularly at a time most people feared that the end of World War II could result in the same economic downturn of the aftermath of World War I. It is with this discernment that Carl Christ recalled: "The applied econometric work of the Cowles Commission, inspired by Marschak and directed at the *improvement of macroeconomic policy*,

[12] See Chipman and Lenfant (2002) for how Slutsky's 1915 and 1927 contributions grew in importance in economics. For both historical and intellectual contexts involved in Slutsky's writings, see Barnett (2004, 2007).

[13] While the Slutsky's approach turned into an essential building block of neoclassical demand analysis, Hotelling's was relegated to the so-called "Hotelling's Lemma." The Hotelling's Lemma is the computation of the supply function and input demand functions through the Envelop Theorem.

had a definite Keynesian flavor" (Christ 1994, p. 35, italics added).

On the second topic, Keynes's theory of aggregate income in combination with Slutsky's contributions enabled another much-sought interpretation in economics: the relationship of macroeconomics and microeconomics; by breaking it out as a separate magnitude, the Slutsky income effect considered at the agent level provided a theoretical basis to interpret income changes at the aggregate one, as though it provided some sort of microfoundations.[14,15] Veering towards Keynes and Slutsky seemed promising at Cowles in the mid-1940s: it permitted the pursuit of Alfred Cowles's dream of predicting stock market returns as well as Jacob Marschak's of taming business cycles (Christ 1994). As is plainly documented in the historiography, under the research directorship of Marshack (1942-1948) the Cowles Commission spent time and resources in structural estimation econometrics, precisely a macroeconometric project whose outcome went awry (Morgan 1990; Louçã 2007; Mirowski 2002; Weintraub 2002, 118).

What is, perhaps, slightly lesser known is that the Commission—possibly in pursuit of Cowles's desire—also engaged in applied works of microeconomic nature, a Walrasian-Slutsky program(?). This objective, like Schultz's efforts almost a decade before, failed as well. It did so because the estimations repeatedly rejected neoclassical price theory in the very crucial respect it rejected the Hotelling economy: the symmetry conditions. As addressed by Hands and Mirowski (1998, p. 375): "(...) actual quantitative restrictions implied by the neoclassical price rarely held in the empirical exercises undertaken at Cowles."

Then, roughly forty years later, with similar motivation, Princeton University microeconomist Angus Deaton (1986, p. 1796) wrote:

> All the techniques of demand analysis so far discussed share a common approach of attempting to fit demand functions to the observed data and then enquiring as to the compatibility of these fitted functions with utility theory.

For such a compatibility to hold, the integrability condition should be satisfied, which is a different way of saying that the symmetry conditions should be satisfied (see appendix to this text). In another passage, after pointing out the considerable body of empirical literature that has carried out the tests, including his (and John Muellbauer's) *almost ideal demand system* of 1980, Deaton (ibid., p. 1791) granted:

> Although there is some variation in results through different data sets, different approximating functions, different estimation and testing strategies, and different commodity disaggregations, there is a good deal of accumulated evidence rejecting the restrictions.

Even taking into account some of the latest developments in the estimation of consumer behaviour, e.g., the nonparametric approach developed since the 1980s (Stoker 1989; Härdle et al. 1991; Haag et al. 2009) or the use of nonseparable models in the 2000s (Hoderlein 2011; Imbens and Newey 2009; Altonji and Matzkin 2005), contemporary economists still fall short of attaining what Harold Hotelling and Henry Schultz wanted to attain more than eighty years ago.

In hindsight, given that most empirical studies have dealt, and continue to deal, with aggregate demand, could this failure be associated with a previous warning by Hotelling

[14] For a more extended discussion on how Keynesian macroeconomics might have influenced the profession's adoption of the Slutsky equation through the interpretation of the income effect, see Hands (2012a).

[15] As explained in Hoover (2012), the relationship of macroeconomics to microeconomics was already an important topic in the "prehistory" of microfoundational programs, a period which included research by such economists as Ragnar Frisch and John Maynard Keynes himself. Although "microfoundations" as a *systematic program of inquiry* did not exist prior to John Hicks's first general equilibrium program by the end of the 1930s (Hicks [1939] 1946), and continued with Lawrence Klein's macroeconometric project of the mid-1940s and Robert Lucas's New Classical Macroeconomics in the 1970s, the term was first used by a mainstream author only in the early 1970s (Phelps 1970).

and Schultz for whom the Slutsky model did not have nice properties under aggregation and, therefore, were destined to failures?

The rejections of the symmetry hypothesis in both approaches to demand theory made aggregation problems even larger a concern in the decade of the 1940s.[16] Chipman (2006a) provided a historical overview on the substantial literature on the estimation of system of demand functions that have dealt with such a problem, underlining the pioneering studies of Lawrence Klein, Richard Stone, Herman Wold, and others. Around this period there also appeared other programs of inquiry dealing specifically with problems of aggregation on both theoretical and empirical grounds.

In the theory realm, as I mentioned before, there was John Hicks's 1939 *Value and Capital,* an attempt at constructing a Walrasian macro model, "(...) a general-equilibrium microfoundational program, which is conceived as showing how Keynesian problems can arise directly from the interactions of individual agents" (Hoover 2012, p. 37).[17] In the empirical case, there was Lawrence Klein's 1946 *Econometrica* article, "Macroeconomics and the theory of rational behaviour," an attempt to build aggregate variables couched in the behaviours of both households and firms; this effort inaugurated, in methodological terms, a new tradition in macroeconometrics.[18]

After Klein's article, other works followed suit, for example, in determining aggregation in a one-industry model (May 1946), in building macroeconomic values in the so-called "general case" (Pu 1946), or in studying the impacts on macro values (aggregate production functions) of changes in micro values (sectoral production functions), "with which it is derived" (May 1947, p. 51). Following May (1947), Nataf (1948) tried to determine the conditions under which an aggregate production function corresponded exactly to the production functions of individual firms (rather than of isolated sectors). Analyzing instability features in systems of differential equations, Hawkins (1948) proposed aggregation methods that could curb them. Finally, also included in these articles was a report of papers presented at a session of the Chicago Meeting of the Econometric Society on the "Relationships Derived from Aggregate Data," with contributions from Kenneth Arrow (1948) and Francis Dresch (1948).[19]

To van Daal and Walker (1990), this spate of articles published in *Econometrica* between 1946 and 1948 consisted perhaps in the greatest testimony yet to the importance of aggregation problems in economics. More dramatically, still according to the authors, with the possible exception of a few remarks made in Cournot's famous 1838 book (*Recherches sur les Principes Mathématique de la Théorie du Richesse*), no economists had ever taken the matter seriously before.[20]

# 4 Koopmans and the Cowles Commission' approach to the problem of aggregation

Tjalling Koopmans's research was stirred by such an aggregation concern: a problem he decided to address by renouncing the empirical approach and adopting the theoretical one.

[16] Trade theorists have long striven with the concept of aggregation, at least since William S. Jevons's concept of *trading bodies*.

[17] Weintraub's 1979 *Microfoundations - The Compatibility of Microeconomics and Macroeconomics*, contains a thorough discussion of Hicks's microfoundational program.

[18] Post-World War II macroeconometrics owes to Hicks, Modigliani and Keynes as much as it owes to Tinbergen, even though the latter two authors disagreed fiercely. Klein's innovations in the subfield came in the form of establishing a middle-ground between Keynes and Tinbergen (Hoover 2012). Pinzón-Fuchs (2017) details the history of Klein's macroeconometrics program.

[19] This annual meeting was held on December 27-30, 1947. As is customary, since the texts presented in one year are published in the following one as a joint report, the authors' contributions were dated 1948. A third paper listed in the session, by Ronald Shephard, was not presented and thus did not have its abstract included in the volume.

[20] One can find a similar impression of Cournot's work, for example, in James Friedman's (2000) survey of the French economist's works and life.

This move coincided with the dismissal of Jacob Marschak as Cowles's research director in 1948; when Koopmans acceded to that position, rather than sanctioning research in applied works, he shifted the Commission's focus to "(...) extract the 'abstract structure' of Walrasian system and subject it to the most rigorous axiomatic scrutiny" (Hands and Mirowski 1998, p. 375). As I discussed elsewhere, this research occupied most of his time from the 1950s to the mid-1970s, a research program that opened an important chapter in the history of axiomatization in economics.[21]

An illustration of how aggregation and general equilibrium theory were interlinked subjects can be drawn from Leonid Hurwicz's appraisal of Hands and Mirowski's alleged objectives of the Arrow-Debreu research program:

> [Hands and Mirowski] impute to the neoclassicists (mainly Arrow, Debreu and Samuelson) a programme containing a set of five objectives. These are listed as existence, uniqueness and stability of competitive equilibria; comparative statics, and welfare economics (...). (*The problem of aggregation, discussed elsewhere, seems to have status akin to that of the five goals*) (Hurwicz 1998, p. 399, italics added).

But as much as Koopmans and other economists placed an enormous amount of effort into this aggregation project, and even though "(...) one can discern the skeleton of the Slutsky conditions buried within the [general equilibrium] system" (Hands and Mirowski 1998, p. 375), the authors maintained that:

> Unfortunately, axiomatic elaboration also eventually led to the conclusion that the system placed no effective restrictions upon excess demand functions, thus subverting the original impetus for the research program (ibid.).

This dim view was also shared by Hurwicz (1998, p. 401) for whom the framework was inevitably "(...) inadequate for aggregate prediction purposes."

Even though placed in an econometric setting, the same difficulty can be observed in a letter Koopmans wrote to Marc Nerlove after the Cowles Commission moved to Yale University in 1955:

> (...) I have developed a general feeling that the clue to a number of different problems in econometrics lies in further exploration of aggregation problems, that ties our notions of individual decision making with somewhat more aggregated relationships. I doubt that this reflection is of much value to you in regard to distributed lags, but it does seem to me that this is one of the problems that may stand to gain if progress should be made on aggregation. Theil's book is excellent as far as it goes. My main reservation about it is that it studies primarily the implications of current econometric procedures, rather than trying to answer the deeper question what procedures we should be using in view of aggregation problems.[22]

In this vein, and also highlighting the frequency with which aggregation concerns arise in theoretical as well as applied econometric works, Hurwicz wrote as follows:

> The issue of *aggregation* has, I believe, special status because of its relationship to econometric research. Since much econometric research (and this, I believe, includes the studies of Henry Schultz) uses data aggregated over certain populations or collections of firms, there arises the question to which properties known to hold for individual agents carry over to aggregates. One reason why this issue is important is that identifiability assumptions used (and needed) by econometricians are often derived from theoretical (or instropective) considerations applicable to *individual* economic agents (Hurwicz 1998, p. 401, italics added).

[21] I have elaborated this point in greater detail in Chu (2018a).

[22] Letter from Tjalling Koopmans to Marc Nerlove, January 31, 1957, TKP, box 16, file folder 304.

Koopmans's research was also stirred by a quarrel with affiliates of the National Bureau of Economic Research (NBER), one of the world's oldest business cycle research institutes.[23] As will be shown below, this discussion encompassed the problem of aggregation and general equilibrium theory as well.

In reviewing the 1946 book *Measuring Business Cycles*, written by Arthur Burns and Wesley Mitchell, Koopmans accused their methods of investigation as being "(...) made with a minimum of assistance from theoretical conceptions and hypotheses regarding the nature of the economic processes by which the variables studied are generated" (Koopmans 1947, p. 161). Their theory, Koopmans reasoned, lacked an explicit formulation about the way an economic agent made choices, essential to explain "man's economic behaviour" and therefore the ultimate causes behind the cycles.[24]

In response to those charges, Rutledge Vining alluded to a certain vagueness in the alternative interpretation proposed by the Cowles economist, skeptical with what Koopmans thought stood behind people's choices:

> Koopmans doesn't give his hypotheses specific economic content (...); and suggests that the kind of content it should have in *general terms*, such as "the behaviour of groups of economic agents," "underlying human responses," "knowledge of man's behaviour and its motives" (Vining 1949, p. 79-80, italics added).

That Koopmans was ambiguous when he referred to individual vis-à-vis group choices, and that he conveyed his ideas through elaborate mathematical arguments specially after the 1960s, have already been stressed in my discussion of his writings (see chapter one of this dissertation). Perhaps it may come as a surprise that such characteristics were already pointed out by Vining as early as 1949, as the continuation of his text illustrates:

> But apparently *all he has to insist upon at present is the mathematical form*, and from his discussions it appears not unfair to regard the formal economic theory underlying his approach as being in the main available from works no later than those of Walras (ibid., italics added).

Another interesting feature that stands out in this passage relates to how closely connected, in Vining's discernment, Koopmans's approach appeared to be with Walras's. More comprehensively, in different excerpts, Vining spoke of Koopmans's attempt at providing aggregate formulations within the Walrasian framework. In one instance, Vining (ibid.: 81) stated:

> (...) some of us may feel that the unit of analysis and the entity the behaviour of which it is of interest to study is not the individual economizer in his conscious, problem solving state of mind. I believe that much of the statistical regularities that are to be observed in population phenomena and that are relevant for the discussion of economic problems involves the behaviour of social organisms that are distinctly more than simple algebraic aggregates of consciously economizing individuals. I think that in a positive sense the aggregate has an existence over and above the existence of Koopmans' individual units and behaviour characteristics that may not be deducible from the behaviour of these component parts.

In another passage, in criticizing Koopmans's econometric practice (inherited from Jan Tinbergen), he affirmed:

[23] For an early history of the NBER, see Morgan (1990, especially chapter two) and Fogel et al. 2013.

[24] This initial denunciation by Koopmans sparked animosity between members of the Cowles Commission and the NBER. It became known in the literature as the "measurement without theory" debate, named after the title of Koopmans's article.

> In a sense, these are the only problems that have been attacked by this entire line of development - the problem of statistical estimation that would be presented by the empirical counterpart of the Walrasian conception. Add to Walras the simple notion of lagged effects (if it is not already there) and certain devices of the nature of the difference equation, and the problem is wholly statistical as contrasted with economic (ibid.: 80).

In closing this section, if the "measurement without theory" criticisms Tjalling Koopmans raised against the Bureau were to have some effects, he had to contribute with the Walrasian theory: building better theories of choice, perhaps with better aggregation properties, was Koopmans's task.

# 5 Samuelson on Homothetic Preferences, the Strong Axiom of Revealed Preference, and the Representative Agent

Among the various contributions to economics, Paul Samuelson made crucial inroads in the Walrasian general equilibrium theory. In a series of papers written between 1956 and 1983, he introduced into the apparatus the assumption by which all individuals in the economy had *identical* homothetic preferences and, by doing so, helped establish certain desirable properties in the framework.

Beginning with "Social Indifference Curves," published in February of 1956, Samuelson adopted *homogeneous of degree one* utility functions as the algebraic representation of homothetic preferences. An important aspect of this formulation is that demand functions resulting from such utility functions could be aggregated to form a *representative consumer* or a *representative agent*.[25] As stated by Hands (2016), in addition to the existence of representative consumers, bearing on the assumption of homothetic preferences warranted three other worth noting properties, each one resulting in a particular interpretation of the general equilibrium model: aggregation, market rationality, and welfare.

Because Samuelson's intention in the 1950s had been to contribute to the international trade literature, i.e., searching for community indifference curves, making a case for homothetic preferences permitted also the simultaneous re-interpretation of aggregation problems. As Samuelson (1956, p. 21) summarized:

> Since most "individual" demand is really "family" demand, the argument can be made that such family demands have been shown to have none of the nice properties of modern consumption theory. However, if within the family there can be assumed to take place an optimal reallocation of income so as to keep each member's dollar expenditure of equal ethical worth, then there can be derived for the whole family a set of well-behaved indifference contours relating the totals of what it consumes: the family can be said to *act as if* it maximizes such a group preference function. The same argument will apply to all of society if optimal reallocation of income can be assumed to keep the ethical worth of each person's marginal dollar equal.

It is thus precisely due to this extension that the Walrasian general equilibrium model

> (...) did have sufficient structure at the *agent level* to be able to say very specific (and desirable) things about the *market-level* results generated by the competitive interaction of such agents. The model was the homothetic Santa Claus case of uniform homothetic tastes ... (Hands 2016, p. 427, italics added).

Along with this aggregative property, the existence of the representative agent meant that rather than a *sypply = demand type equilibrium* (for all goods in the economy), now it sufficed to arrive at the solution to a constrained optimization problem of that single agent.

[25] On the mathematics of the aggregation see, for example, Acemoglu (2009) and Blackborby et al. (2008).

As a consequence, following all usual assumptions of demand theory, the rationality of a single individual is (as if) transferred to the whole system.[26]

Parallel to this event, in separate efforts, Samuelson (1950) and Houthakker (1950) developed the *strong axiom of revealed preference* (SARP) approach to consumer theory, one which emboldened the perception that a representative agent could in fact exist in the economy: "(...) if the SARP axiom holds on market (rather than individual) demand functions, then there always exists a rationalising representative agent: i.e. the so-called Wald case where the market reflects 'revealed group preference' (...)" (Hands 2016, p. 429).

Contrary to the reliance on the *weak axiom of revealed preference* (WARP), the fulfillment of SARP signified the satisfaction of integrability condition necessary for the demand function to be treated as if it were generated by a budget-constrained utility maximizing agent. Hence, borrowing a last passage from Hands (ibid., p. 430)

> (...) if the SARP holds on market demand functions then the demand functions behave as if they were generated by a representative agent and the Walrasian general equilibrium of the whole economy reduces to the consumer's equilibrium of that agent.

An important point here is that more than enabling the construction of a particular breed of general equilibrium model, the elimination of the heterogeneity of agents and income effects—as a result of homothetic preferences—freed the representative agent to be used in other fields of economics as well, such as growth theory, international trade, and financial economics.[27] As a matter of fact, in the last chapter I investigate how, if at all, Samuelson's representative agent as used in macroeconomics made it to Koopmans's: it turns out that the channel of influence might have played out in the subfield of optimal growth theory. But first, I shall discuss a change in the meaning of dynamics as portrayed in Hands (2010).

# 6 The Postwar Stabilization of Consumer Choice Theory: redefining dynamics

It seems that with regard to the stabilization of consumer choice theory, the thesis of a "skein," or of an "interlocking competitive system," consisting of theories that culminated in the three American neoclassical hubs (the Cowles general equilibrium approach, the Chicago Marshallian variant and Samuelson's revealed preference doctrine) has been virtually uncontested.[28]

In an article published in the *European Journal of the History of Economic Thought* in 2010, Wade Hands introduced into the former interpretative thread yet another simultaneous modifying feature: consumer choice theory ceased to be dynamic. This time, the source of amendment came from another series of contributions by Paul Samuelson, notably his PhD dissertation *Foundations of Economic Analysis*, published in 1947, a source that played a key role.[29] In his appreciation of Samuelson's contributions, Hands (2010, p. 332) wrote:

[26] The attainment of the representative agent means also the solution to two other specific problems within general equilibrium theory: uniqueness and stability. However, I will not pursue such topics here.

[27] "Financial Economics" studies the interactions of households, firms and financial intermediaries in both domestic and international settings. In the latter case, national economic policies might be even more important a force. Robert C. Merton defines it as the overlapping of finance, monetary economics and public finance.

[28] Besides Hurwicz (1998), Cartwright (1998), and other criticisms contained in Backhouse et al. (1998), Daniel Hammond also challenged the validity of the Hotelling-Schultz impasse; specifically, Hammond (2006) disagrees with chief aspects concerning the unfolding of events from the standpoint of the Chicago School.

[29] In Wade Hands's argument, since Samuelson's articles on stability were all incorporated into his 1947 volume, it suffices to refer only to this last work as his main contribution to the topic. Furthermore, on

> By the 1950s "dynamic" meant "based explicitly on differential or difference equations involving time," and optimization problems - maximum or minimum - were *not* of this sort. Maximization was not a dynamic process; the Walrasian tâtonnement was.[30]

In another passage, Hands claimed that:

> The ultimate impact of the separation - or the impact of the profession generally accepting this separation - was that consumer choice theory, which was based on utility *maximization*, ceased to have anything to do with movements or dynamics. Of course no dynamics means no paths, no endowment effects, no reference dependence, no order of consumption, none of the other problems associated with integrability$_B$. The concept of economic dynamics is stabilized and in the process consumer choice theory is relieved of the responsibility for dealing with all of these potentially troublesome issues. Economic agents with well-ordered preferences defined over the entire choice space became the standard basis for consumer choice theory, and the non-integrable case and all the difficulties associated with it quietly left the stage. Stabilizing dynamics thus helped stabilize consumer choice (ibid.).[31]

If one were to follow Hands's classification on what (additionally) might or might not belong to the stabilized body of consumer choice theory, Koopmans's articles in intertemporal economics would not pass the test. That is to say, Koopmans continued to carry out his studies in the 1950s and 1960s in the "old" way, searching for a dynamic nature within it even after a possible separation between optimization and dynamics became more visible in the literature.

Even though it is possible to insist that Koopmans did not undertake dynamic analysis proper since neither differential nor difference equations were used, Hands's exemplification quoted above, wherein "well-ordered preferences defined over the entire space became the standard basis for consumer choice theory" (ibid., p. 332-3), cannot be used to shed light on Koopmans's contributions either. As I have treated in some detail elsewhere, a crucial part of his analysis shored up on the notion of impatience and time perspective, thus making time a crucial matter.[32] Furthermore, as it has also been shown, Koopmans continued to pursue a distinctive preference-based demand theory in the 1970s by postulating preference orderings *over time* rather than *on a prospect space*. For several authors, such a distinction cannot be easily applied either.

In Louis Phlips's instructive *Applied Consumption Analysis* (1983), all chapters concerning Koopmans's contributions to consumer choice theory were allotted to a whole section titled "Dynamics." In appraising the case in which the consumer looks into the future, Phlips (1983, p. 263) stated:

> We want to explain the allocation of his budget among $n$ commodities, when due attention is given to the fact that he is not maximizing an instantaneous "static" or an instantaneous "dynamic" utility function (in which the influence of the past behaviour is incorporated) as in previous chapters, but is maximizing an "intertemporal" utility function defined on sequences over time (from now to some future date) of commodity bundles.

Judy Klein (2007) made the case in which Richard Bellman used dynamic programming intensively to carry out "economizing" efforts during World War II. Economists then began to apply the same functional equation formulation first in microeconomics and, later,

the simultaneity issue, such contributions by Samuelson coincide temporarily with the formation of the "skein," as defended by Hands and Mirowski (1998) and Mirowski and Hands (1998).

[30] So clear was the message that Samuelson wanted to convey in his book that he separated it into Part I, dedicated to optimization problems, and Part II, dealing with dynamics.

[31] For an explanation of integrability$_B$, see Hands (2006).

[32] See chapter one of this dissertation.

in macroeconomics. A successful incursion of economic dynamics into the realm of microeconomics would surely have required its application in consumer choice theory. Lars Peter Hansen (2010) argued that the first use of recursive preferences can be traced to Koopmans (1960) and Koopmans et al. (1964). Stokey et al. (1989) added to this list the article by Beals and Koopmans (1969).

Following this reasoning, considering that for Shone (2002, p. 3) "By its very nature, dynamics involves time derivative, $dx/dt$, where $x$ is a continuous function of time, or difference equations, $x_t - x_{t-1}$ where time is considered discrete units," and that, in the context of a neoclassical growth model, Corbae et al. (2009, p. 92) asserted that "A dynamic optimization problem is one in which a decision must be made over time in which early decisions affect later options," the rupture as proposed by Wade Hands seems hardly straightforward.

Although providing a historical account that cannot be taken at face value, the graduate textbook *Recursive Methods in Economic Dynamics* (1989) has this to say regarding the inclusion of dynamic (and stochastic) features in economics:

> These theoretical developments are based on a wide variety of results in economics, mathematics, and statistics: the contingent-claim view of economic equilibria introduced by Arrow (1953) and Debreu (1959), the economic applications of the calculus of variation pioneered long ago by Ramsey (1928) and Hotelling (1931), the theory of dynamic programming of Bellman (1957) and Blackwell (1965) (Stokey et al. 1989, p. 3).

The passage shows that at least some of the publications that contributed seminally to dynamics, and which were later applied to consumption theory, came out at the approximate time of their alleged separation (stabilization).[33] Finally, it can be argued that dynamic optimization techniques solve problems posed, for example, in the Hamiltonian dynamic system and they date back at least to Samuelson and Solow (1956) (Wulwick 1995).

In distinct ways, the excerpts above coincide with a key economic substance in Koopmans's research: the blurring of optimization (choice theory) and dynamics (intertemporal features). One can discern such a feature in the articles written in the 1950-1970 period, as well as in the only application he made of that theoretical framework: the 1963 normative growth model.

# 7 Concluding Remarks

Besides the historical value of providing a more accurate account of demand theory, building such an alternative version served another purpose: it allowed us to better understand the rise of the representative consumer in the 1950s and 1960s. Furthermore, considering that this period also coincided with the discussions concerning the compatibility between general equilibrium and demand theories, I have attempted to offer a history of the representative agent in an even more convoluted context.

Even though it was not my aim to emphasize the complex Hotelling and Schultz dialogue, it is worth quoting a passage from Mirowski and Hands (1998) for two reasons. In addition to conveying precisely the methodological and economic significance of Samuelson's contributions to the impasse, the passage *per se* also helps us observe the state of demand theory at that time, and hence is useful for comparing what the Cowles Commission was at the end of the 1930s with what it became after an important person in this essay, Tjalling Koopmans, took over the research directorship of the institution in 1948:

> The problem for Samuelson was to find a way of formulating the theory of demand that would be consistent with his positivist-operationalist methodology while simultaneously *avoiding the type of econometric testing* associated

[33] In the case of Ramsey (1928), there is even a clearer intersection between dynamics and choice theory.

> with Schultz. How could this possibly be done? (...) In essence his answer was to *change the place where the empiricism lived in the neoclassical theory of demand*. Instead of having empiricism enter at the back end - by testing the empirical implications deduced from theory - the revealed preference approach would place empiricism right up front at the beginning of the exercise. If the epistemologically dubious notion of subjective utility could be replaced with a strictly behaviorist - thus objective, observational, operational, and meaningful - concept of consumer action, demand theory could be reconstituted on what Samuelson considered legitimate scientific foundations (Mirowski and Hands 1998, p. 283, italics added).

In other words, in the 1930s, and for most of the 1940s, demand analysis was overwhelmingly an econometric enterprise.

An important remark in this chapter, therefore, is that if the Cowles Commission carried out applied econometric projects of microeconomic and macroeconomic natures before Koopmans, after his appointment as research director in 1948 the institution was transformed into a pole of pure theoretical microeconomics. More important, motivated by aggregation discussions taking place in economics, the new research program at Cowles was also directed to tackle such a problem.

I have also stressed in this chapter that a significant portion of this discussion, of the compatibility between micro and macroeconomics, can be traced to another historical fact: the widespread adoption of homothetic preferences, first introduced by Paul Samuelson in 1956. Then, I have showed that Koopmans's research purpose intersected with Samuelson's push for homothetic preferences.

Although consumer theory and dynamics stabilized as two separate branches within microeconomics in the 1950s and 1960s, Koopmans still tried to extend the first subfield through the addition of dynamic features, initially with respect to the structure of utility functions and, later, with respect to preferences. This observation, however, seems to be at odds with interpretations given, for example, in Hands (2010), and I provided some examples in the literature showing why it is hard to tell consumption theory and dynamics apart.

I hope the narrative in this chapter has identified a few junctures that made more transparent the relationship between the development of demand theory and the rise of the representative agent in microeconomics.

At least two important questions remain open for further investigations. The first one concerns Koopmans's own use of homothetic preferences and how much he interacted on this matter not only with Paul Samuelson, but with Gerard Debreu, an important mathematical economist who made essential contributions to the axiomatization of economics and who was also a member of the Cowles Commission. Following this query, a second question concerns a clear association between the Koopmans-Samuelson writings on homothetic preferences and the time-separability preferences used in modern macroeconomics, as discussed in Barro and King (1984).

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