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3. The rise and fall of Walrasian microeconomics: the Keynesian effect[1]

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> We may digress to point out that the general point of view and habit of mind reflected in the Hicks–Slutzky analysis has wide ramifications in recent literature and has led to utter confusion in the whole body of economic thought. We refer, of course, to the huge corpus of discussion beginning with Keynes's *General Theory* and following the lead of that work. (Frank Knight, 1944: 300)

1. INTRODUCTION

Pronouncements of the death of Walrasian microeconomics have become quite common in recent years. For a growing number of economists, the research program that was once the discipline's showpiece of rigor and technical sophistication "has finally run out of gas" (Rizvi, 1998, 274) and should be moved from the front lines of economic research to the back-burner of retrospective reflection (Bowles and Gintis 2000). In many cases the target for the narrative of demise is narrowly-focused on the most abstract version of Walrasian general equilibrium theory, and in such cases the story is usually that it succumbed to a host of internal technical difficulties, particularly those associated with stability analysis and the Sonnenschein–Mantel–Debreu (SMD) theorems on excess demand functions (Kirman 1989, 2006, Rizvi 1998, 2003). In other cases the target is much broader – neoclassical economics or rational choice theory in general – and here the downfall is often associated with the theory's questionable empirical record and the recent development of alternative approaches such as behavioral economics, experimental economics, and the economics of complexity (Colander 2000, 2006; Colander, Holt and Rosser 2004a, 2004b, Davis 2006, 2008). Commensurate with, although relatively independent of, these narratives about the fall of Walrasian microeconomics, a body of historical literature has developed during the last few decades

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which gives us a deeper understanding of the various forces that contributed to the rise of Walrasian economics and how the resulting theory came to take the particular form that it did. A few of the many books covering aspects of this recent historical literature include Amadae (2003), Giocoli (2003), Ingrao and Israel (1990), Mirowski (2002), and Weintraub (1985, 1991, 2002), but the relevant research is quite extensive and this is only the tip of the iceberg.[3]

This chapter will also discuss the rise, and to a lesser extent the fall, of Walrasian microeconomics, but it will focus on an aspect of the story that has received very little attention: the role of *Keynesian economics*. Of course, there already exists an extensive literature on the relationship between the Walrasian and Keynesian research programs. For example, the histories of macroeconomics offered by the Cambridge-centered critics of IS-LM Keynesianism that Alan Coddington once labeled the "Fundamentalist Keynesians" (Coddington 1983), clearly emphasize the relationship between Walrasian and Keynesian economics. They argue, as do post-Keynesians of a variety of stripes, that Walrasian ideas – initiated by John R. Hicks's original IS-LM paper (Hicks 1937) – influenced, and ultimately corrupted, the central message of John Maynard Keynes's *General Theory* (1936).[4] The economists Coddington labeled "Reconstituted Reductionists" (Clower 1965, Leijonhufvud 1968) have a different take, but they too have drawn attention to, and criticized, the Walrasian influence on textbook Keynesianism. But identifying the Walrasian imprint on standard Keynesian theory is not exclusive to those who would call themselves Keynesians. Milton Friedman's Marshallianism was associated in part with his identification and criticism of Walrasian theoretical influences within Keynesian macroeconomics (see DeVroey 2009, Hoover 1988, or Mayer 2009 for example). Finally, even Hicks himself, when explaining the origins of the IS-LM model, stressed the influence of Walrasian ideas on the Keynesian theoretical framework he set in motion: "the idea of the IS-LM diagram came to me as a result of the work I had been doing on three-way exchange, conceived in a Walrasian manner" (Hicks 1980-81: 142).

Although there is an extensive literature on the relationship between Walrasian and Keynesian economics, none of it really focuses on the issues examined here. All of these authors, and most others who have examined the relationship between Walrasian and Keynesian theory, have directed the explanatory arrow from the former to the latter. The two main questions have traditionally been: How did Walrasian ideas influence, condition, or possibly determine, what came to be the standard textbook Keynesian theory? and, Was that Walrasian influence a good thing or a bad thing (with respect to either the scientific adequacy of the resulting theory or its fidelity to Keynes's own thinking)? My focus will be quite different.

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First and most importantly, I will run the explanatory arrow in the opposite direction: from Keynesian to Walrasian ideas. I want to explain not how Walrasian ideas played a role in shaping what became standard textbook Keynesian macroeconomics, but rather how Keynesian ideas played a role in shaping what came to be the standard textbook Walrasian microeconomics (with Arrow and Hahn 1971 as the canonical text). Secondly, my interest will be more explanatory than evaluative; I will focus on identifying influences and revealing the profession's theoretical preferences, not on evaluating whether those influences and preferences were scientifically a good thing or a bad thing (or whether they were exegetically faithful).

The chapter is organized in the following way. The first section lays out some definitions and presuppositions relevant to the overall discussion. Given that the argument cuts across such a wide swath of time, individuals, and ideas, it is useful to be clear right up-front how important terms will be used and to point out some of the things that will be taken as givens throughout the chapter. The second section is the heart of the argument and the chapter's main contribution. This section argues that Keynesian ideas played a role in the Walrasian rise to dominance and also influenced the content of the particular "Walrasian" theory that ultimately emerged. The neoclassical synthesis was a two-way street with influence between Walrasian and Keynesian ideas flowing both ways, and the particular versions of both "Keynesian macro" and "Walrasian micro" that stabilized during the mid-twentieth century were joint products of that synthesis. The third section discusses the connection between the neoclassical synthesis and the fall (or at least demotion) of Walrasian microeconomics. The final section contains a brief summary and a review of the main themes of the chapter.

2. A FEW PRESUPPOSITIONS AND STAGE-SETTING FOR WHAT FOLLOWS

I will talk about Walrasian economics and Keynesian economics as if they were research programs that can clearly be distinguished from other theoretical frameworks and are sufficiently stable to be identified, and re-identified, across various points in time.[5] Although I do assume that both research programs contain certain core or paradigmatic propositions/conceptualizations, this does not mean that I have captured the "essential nature" of these programs, or that such an essence even exists. These propositions are simply empirically-identifiable features of a particular sort – reliable identifiers of family resemblance – and their stability is always

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subject to particular time constraints. Roy Weintraub's six core propositions (Weintraub 1985: 109) do a reasonably good job identifying the key features of the Walrasian research program, and the core of the Keynesian program would include propositions such as: the short run aggregate level of output and employment are determined by aggregate expenditure; the interest rate is determined by the supply of and demand for liquidity; the marginal propensity to consume is positive and less than one; etc. Notice that accepting such core propositions – reliable identifiers of family resemblance – leaves a lot of room for variation and debate within the two research programs.

It is also useful to identify two presuppositions about the history of twentieth-century economics that will be assumed throughout the discussion. Both seem relatively uncontroversial, but it is useful to state them explicitly since they are taken as given in all of what follows. The first is that mainstream economics was dominated by the *neoclassical synthesis* from sometime during the mid-1950s until roughly the mid-1970s.[6] The neoclassical synthesis was a product of contributions by a number of different economic theorists – key texts include Hicks (1937, [1939] 1946), Lange (1944), Lerner (1944), and Samuelson (1947) – and although there were clearly differences among the various contributors, one of the main results of the synthesis was that the discipline came to be seen as an amalgam of two separate – but consistent and non-antagonistic – parts: macroeconomics and microeconomics.[7] As Paul Samuelson put it in the third edition of his famous *Economics* textbook: "the economist is justified in saying that the broad cleavage between microeconomics and macroeconomics has been closed" (Samuelson 1955: 360). The synthesis was the "instrument of reconciliation" (Pearce and Hoover 1995: 211) between these two branches of economic theory; its macroeconomics was Keynesian and its microeconomics (at least the "high theory") was Walrasian. By the 1960s the synthesis manifested itself in essentially every economics textbook in the United States (Pearce and Hoover 1995), it produced the "micro" and "macro" curricular structure that continues to dominate economics instruction, and for many years it formed the theoretical backdrop for effectively all research in economic theory.[8] As Brian Snowdon and Howard Vane explain:

> The synthesis of the ideas of the classical economists with those of Keynes dominated mainstream economics at least until the early 1970s. The standard textbook approach to macroeconomics from the period following the Second World War until the early 1970s relied heavily on the interpretation of the *General Theory* provided by Hicks (1937) and modified by the contributions of Modigliani (1944), Patinkin (1956), and Tobin (1958). Samuelson's best selling

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> textbook popularized the synthesis … making them accessible to a wide readership and successive generations of students. It was Samuelson who introduced the label 'neoclassical synthesis' into the literature in the third edition of *Economics* in 1955. The synthesis of classical and Keynesian ideas became the standard approach to macroeconomic analysis, both in textbooks and in professional discussion … (Snowdon and Vane 2005: 23)

The second historical presupposition is the *pluralism* and *diversity* that existed within microeconomics during the interwar period. Although this period was a bubbling cauldron of diverse economic ideas from Institutionalist, Marxist, and Austrian economics, this wide-ranging inter-programmatic diversity is not the diversity that will be emphasized here. The diversity emphasized in this chapter is a more intra-programmatic diversity – the diversity among various economists who were broadly marginalist or neoclassical (although not all would label themselves as such) and shared a common commitment to certain modeling strategies, mathematical tools, and types of evidence – yet who promoted and defended quite different economic theories.[9]

Focusing on demand theory, a partial list of these various approaches would include: defenders of the Marshallian tradition in either cardinal utility (Robertson 1952) or compensated demand (Friedman 1953) form; those who, Cournot- or Cassel-like, started from demand functions rather than individual choice (these took different forms including, among others, statistical (Moore 1914, Schultz 1928) and mathematical (Evans 1930) versions); Slutsky ([1915] 1952); Bernardelli (1952); Knight (1944); Gilboy (1930); Hicks and Allen (1934); Allen's non-integrable interpretation of Hicks and Allen (Allen 1936); Nicholas Georgescu-Roegen's psychological threshold (Georgescu-Roegen 1936) and directed choice (Georgescu-Roegen 1950) models; Harold Hotelling's entrepreneurial demand function model (Hotelling 1932); Ragnar Frisch's conditional preferences approach (Frisch 1926); Oskar Morgenstern's reconstituted demand theory (Morgenstern 1948); Paul Samuelson's radical behaviorism in his first "revealed preference" paper (Samuelson 1938); and W. E. Armstrong's just-perceptible-differences theory (Armstrong 1939). By the late 1950s this diversity of ways of explaining consumer choice and demand had been replaced by a Walrasian theory originating in the work of Leon Walras (1954) and Vilfredo Pareto (1971), but given its final (calculus-based) form in Hicks and Allen (1934) and Slutsky ([1915] 1952). Early influential book-length statements include Hicks's *Value and Capital* ([1939] 1946), Samuelson's *Foundations* (1947), and Henry Schultz's *Theory and Measurement of Demand* (1938); these n-good multivariate calculus-based versions of the theory formed the basis for the standard graduate microeconomics textbooks of the 1960s and 1970s (lower-level textbooks offered the same

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theory, but presented it in one- and two-dimensional diagrams). The argument will be that Keynesian economics had something to do with Walrasian demand theory emerging as *the* (rather than *a*) theory of demand as well as why certain aspects were emphasized and particular theoretical formulations emerged as they did.

The last two remarks I would like to make in this section are comments on, rather than presuppositions for, what is to follow. The first is that when I argue that particular aspects of Walrasian economics were "consistent with" Keynesian economics, I only mean to the architects of the neoclassical synthesis. These remarks – in fact, my entire argument – in no way implies an endorsement of the view that "Walrasian economics" and "Keynesian economics" are in any substantive sense consistent or could co-exist in a theoretical partnership indefinitely. In fact I generally agree with those who argue that the neoclassical synthesis exhibited a certain "theoretical schizophrenia" (Snowdon and Vane 2005: 21). The "fit" that formed the backbone of the neoclassical synthesis was at a best a temporary equilibrium. It existed because of the particular way the two research programs co-evolved, the historical situation (politically, economically, and epistemologically), the persuasive power of certain individuals and their self-conscious efforts to downplay differences, and many other historically contingent factors.[10]

Finally, I think it is useful right up-front to be clear about what I am not arguing. My argument is *not* that Keynesian economics was the *only* reason that the Walrasian version of neoclassicism emerged triumphant or that Walrasian microeconomics took the particular form that it did during its heyday. The reason research programs rise to dominance and the transformations they go through during their evolution is always a very complex story. In the first paragraph I cited a number of authors/texts who have recently made contributions to our understanding of the ascent and character of Walrasian general equilibrium theory. The argument here is not an alternative to those and other narratives;[11] it simply provides an additional, unrecognized, factor that deserves to be considered.

3. WHY AND WHICH WALRASIAN ECONOMICS?

This section will discuss four ways (sections 3.1–3.4) in which the compatibility between the versions of Walrasian and Keynesian theory that stabilized during the neoclassical synthesis helped the former win out over its immediate competitors and how the theoretical structure of Walrasian theory was pulled in various directions that enhanced the fit.

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3.1 The Centrality of Market Demand

It should be uncontroversial that demand (as opposed to supply, production, or cost) is central to Keynesian economics. There are many different interpretations of the *General Theory*, but common to all is the idea that aggregate demand (aggregate expenditure, aggregate spending, …) is the major determinant of output and employment.

Of course, demand theory is also fundamental to Walrasian microeconomics. The core idea is that demand functions are the result of consumers solving a particular constrained optimization problem: choosing the most preferred (utility maximizing) bundle from the set of affordable bundles. The consumer's preference-ordering is the key primitive in the analysis; preferences are assumed to be well-ordered (complete, transitive, etc.) and thus can be represented by an ordinal utility function $U(x)$.

Writing out the standard consumer choice problem we have:

$$\begin{aligned} &\underset{x}{\text{Max}}\, U^h(x) \\ &\text{Subject to: } M^h = \sum_{i=1}^{n} p_i x_i, \end{aligned} \qquad \text{(CCP)}$$

where $p_i > 0$ is the price of good i and $M^h > 0$ is consumer h's money income. Given the standard assumptions on preferences and the linearity of the budget constraint, the utility function will have sufficient mathematical structure to guarantee the existence of a well-behaved solution.

The solutions to the consumer choice problem are the n individual *demand functions*. The demand for good i by individual h is given by:

$$x_i^h = d_i^h(p, M^h) \quad \text{for } i = 1, 2, \ldots, n \qquad \text{(ID)}$$

where $p = (p_1, p_2, \ldots, p_n)$. *Market demand functions* are obtained by adding up the individual consumer demand functions, so assuming there are H individuals, the demand for good i is given by:

$$x_i = D_i(p, M^1, M^2, \ldots, M^H) = \sum_{h=1}^{H} d_i^h(p, M^h)^{12} \qquad \text{(MD)}$$

As noted above, this Walrasian demand theory – now simply the theory of demand – comes down to contemporary textbooks from Pareto, through Slutsky ([1915] 1952) and Hicks and Allen (1934), and the influential presentations in Hicks ([1939] 1946), Samuelson (1947), and Schultz (1938).

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In relating this microeconomic theory of demand to Keynesian macroeconomics, it is useful to note that there are really *three separate parts to the micro side*: rational choice (the behavior of individual economic agents), individual demand (an individual's demand for a particular good), and market demand (the total market demand for the good). The market demand functions should then relate in some non-inconsistent way to the aggregate demand function of macroeconomics. The discussion in this section will focus on the three different parts of the micro side.

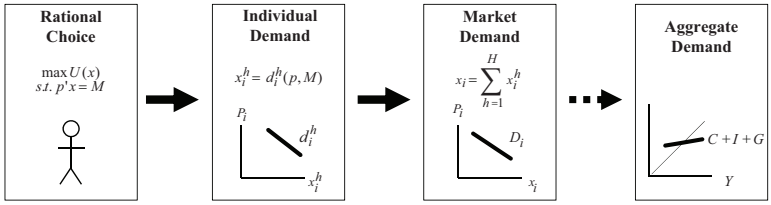

Figure 3.1

Consider Figure 3.1, which illustrates the relationship between macroeconomic aggregate demand (far right side) and the three different parts of microeconomic demand theory (left side). Almost all microeconomic theories of demand have some version of all three of these aspects, but most *also emphasize one of these aspects more than others*. For example, going back to the partial list of various pre-synthesis demand theories given in section 1, some of these focused primarily on the psychological specifics of human decision making (for example, Armstrong, Bernardelli, Georgescu-Roegen, and to a lesser extent Allen and Frisch). Although such theories frequently came up with something like a market demand function, their main focus was on individual choice (that is, the far left-hand side of Figure 3.1). In some ways this individual-choice-theory-first tradition has recently been revived by the work of experimental and behavioral economists (although it is seldom recognized as a revival since the experimental and expected utility aspects of the recent literature tend to blur its relationship to 1930s demand theory[13]). On the other hand, other theorists tended to focus primarily on market demand functions and had only a very thin, and in some cases non-existent, theory of individual behavior (for example, Cournot, Cassel, Evans, Moore, Schultz 1928 but not 1938, and others). Those theorists tended to focus more on the right-hand side of the microeconomic portion picture (and some, Cassel and Moore in particular, did not have, or believe it was necessary to have, a theory of individual behavior at all).

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Given this differentiation between choice-centered and market demand-centered theories, it is important to recognize that the version of Walrasian general equilibrium theory that became dominant during the 1950s and 1960s was much more of a market-focused theory than an individual behavior-focused theory. Of course Walrasian models of the time typically assumed rational economic agents with well-ordered preferences acting under constraint, but explaining *individual* behavior was never the main task. Synthesis-era Walrasian general equilibrium theory was primarily a market-focused approach where all the theoretical heavy-lifting was done by restrictions on *market excess demand functions*. The analysis of stability, uniqueness, and comparative statics – the program's primary theoretical output – was routinely conducted in terms of models specified entirely in terms of market excess demand functions. As Kenneth Arrow and Leonid Hurwicz explained in their influential work on stability theory:

> This work is characterized, in the main, by being based on models whose assumptions are formulated in terms of certain propensities of the individual economic units, although in the last analysis it is the nature of the aggregate excess demand functions that determine the properties of equilibria. (Arrow and Hurwicz 1958: 522)

For example, if the market excess demand for each good i is given by $z_i(p)$ and the model assumes a sufficient amount of continuity and interiority, the only two assumptions needed on the $z_i(p)$s to do "general equilibrium analysis" are zero degree homogeneity (H) and Walras's Law (W):

$$z_i(p) = z_i(\lambda p) \text{ for all } \lambda > 0 \text{ and for all } i = 1, 2, \ldots, n, \tag{H}$$

$$p^T z(p) = \sum_{i=1}^{n} p_i z_i(p) = 0 \tag{W}$$

Granted, the reason why one might think market excess demand functions have these two properties comes from the behavior of the underlying agents, but given (H) and (W), it is possible (and became standard practice) to kick away the rational choice ladder and conduct analysis entirely in terms of market-level excess demand functions. In fact, this is the main message of so-called Sonnenschein–Mantel–Debreu (SMD) theorems on excess demand functions (Debreu 1974; Mantel 1974, 1977; Sonnenschein 1972, 1973).[14] Basically these results say that any continuous function that satisfies (H) and (W) can be an excess demand function for a Walrasian economy. In other words, the SMD results demonstrate that Walrasian general equilibrium theory actually has very weak microfoundations. The

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standard assumptions on individual consumers do not put much structure on market excess demand functions because the theory has almost nothing to say about the behavior of individual economic agents.

One way to read the argument in this section is to reduce it to simply praising Hicks (1937) for having a good eye for finding the best microeconomic theory to hook up with Keynesian macroeconomics. If it is the late 1930s and one is looking for a microeconomic theory to connect up with Keynesian economics, then choosing the Walrasian program with its focus on market demand and its lack of emphasis on the behavior of individual economic agents (for Keynes a notoriously unreliable source of insight about macroeconomic policy and aggregate economic behavior) does seem to be a very wise move. But one can say more than this.

The Walrasian program in the hands of Pareto (1971) and later Schultz (1938) was more focused on individual choice than the Walrasian theory that came later; in fact Pareto had no market demand functions at all in the *Manual*.[15] These economic theorists never sought the serious psychological underpinnings that concerned some of the competitors to Walrasian theory during the 1930s, but their approach certainly focused "more" on individual behavior than the Walrasian framework that characterized general equilibrium theory at its peak. This would suggest not only that Hicks did in fact have a good eye, but also that Walrasian theory ultimately came to emphasize market demand as a result of the neoclassical synthesis and its co-evolution with Keynesian economics.

3.2 Tâtonnement Stability and Related Issues

Walras's main focus in the *Elements* was the formal characterization of competitive equilibrium: specifying the basic equations of the general equilibrium model and proving the existence of a solution (which for him meant demonstrating that the number of equations was equal to the number of unknowns). Walras did however also attempt, throughout the various editions of the *Elements*, to show how the theoretical solution would actually be reached by the competitive market process. As Walras himself explained in the 4th definitive edition: "Now let us see in what way this problem of the exchange of several commodities for one another to which we have just given a scientific solution is also the problem which is empirically solved in the market by the mechanism of competition" (Walras 1954: 169).[16] His approach to this "empirical" question was to specify an adjustment mechanism where prices changed "by a process of groping ['par tâtonnement']" under the rule that if "the demand for any one commodity is greater than the offer, the price of that commodity in terms of the *numéraire* will rise; if the offer is greater than the demand, the price will fall" (Walras 1954: 170).

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To the post-synthesis reader, Walras's words may suggest the system-of-ordinary-differential-equations version of the tâtonnement popularized by Samuelson (1941, 1942, 1944, 1947):

$$\frac{\partial p_i}{\partial t} = k_i z_i[p_1(t), p_2(t), \ldots, p_n(t)] \quad \text{for all } i = 1, 2, \ldots, n, \tag{T}$$

(where $p_i(t)$ is the price of the ith good at time t, $z_i[\cdot]$ is the excess demand function for the ith good, and $H'_i > 0$), but Walras did not employ this version of the adjustment process or anything like it. Walras's own explanation involved a fairly elaborate "sequential" process of clearing one market at a time based on changing only the price of the good in that market. From any initial disequilibrium position the price of good 1 is adjusted on the basis of the rule that if excess demand is positive the price would be raised and if it is negative it would be lowered until the excess demand for good 1 is equal to zero. Then the same procedure is applied to the market for good 2, then good 3, and on and on in sequence. Obviously in the standard case where the excess demand for each good depends on the prices of all goods, there is no reason to believe that the first iteration will be sufficient to reach equilibrium, so the process would need to be repeated again and again. But under the assumptions of Walras's original model this sequence of iterations need not converge to the general equilibrium.[17]

Walras's sequential tâtonnement was very different from the way the price adjustment mechanism was characterized in the post-Samuelson literature (that is, T). As Walras's translator William Jaffé explained: "The current reformulations of the theory, though they proudly bear the Walras patronymic, display only a distant family resemblance to their ancestral prototype, for the infusion of new technical refinements has all but obliterated any recognizable similarity between the descendant theories and their progenitor" (Jaffé 1967: 1). To see why this difference is important for the issue of the relationship between Walrasian and Keynesian economics, it is useful to rewrite the later version of the tâtonnement (T) in its common "speed of adjustment" form:

$$\frac{\partial p_i}{\partial t} = k_i z_i[p_1(t), p_2(t), \ldots, p_n(t)] \text{ for all } i = 1, 2, \ldots, n \tag{T'}$$

Where $k_i > 0$ is the speed of adjustment for the ith market (Arrow and Hurwicz 1958: 525; Arrow and Hahn 1971: 285). As will be discussed in more detail below, this form makes it possible for some markets to be "slower" or "stickier" in the process of adjustment than others, allowing for Keynesian-type behavior in certain markets while staying broadly within

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the Walrasian framework. Of course, one can ask whether this characterization of disequilibrium accurately captures what Keynes had in mind, but that is not the issue. The point to note here is that (T') – and thus (T) since it is just a more general version of (T') – accommodates certain ideas associated with Keynesian economics much better than Walras's original sequential process. According to Walras's version, each market will be in equilibrium at a certain point (and generally multiple times during the iterative process), a framework that makes it much more difficult to accommodate the idea that some particular markets are consistently slower or stickier in their adjustment than others.

In addition to and perhaps even more important than the fact that Walras's original sequential formulation of the tâtonnement was difficult to combine with Keynesian theory, is that between Walras's *Elements* and Samuelson (1941), general equilibrium theory systematically moved away from any discussion of the competitive price adjustment mechanism. As Jaffé explains (Jaffé 1967, 1981), Walras recognized that the "realistic" or "empirical" dynamics[18] that he was attempting to model would involve trading at "false prices" which in turn would involve "income" or "endowment" effects that could potentially change the equilibrium price vector. This is a problem even in the pure exchange case, but it is more problematic in the production version of the model. Walras eventually adopted a "no trade outside of equilibrium" condition for both the pure exchange and production models, but this solution is entirely counter to his original purpose for introducing the tâtonnement process. In Jaffé's words: "It is, in fact, an abandonment of realism and with this abandonment the initial purpose of the theory of tâtonnement is lost from sight" (Jaffé 1967: 12). These problems – and here is the point for the Keynesian story – led Pareto to completely abandon any discussion of the tâtonnement mechanism. There was a brief mention in the *Cours*, but it is totally absent from the *Manual* (Donzelli 2006: 12–19). Thus if one considers the evolution of "Walrasian" general equilibrium theory from the early editions of the *Elements* to the *Manual*, the tâtonnement goes from being an important part of the story but modeled differently than (T), to being very problematic, to being entirely abandoned.

Moving forward in time to Hicks and Samuelson, Hicks discussed multiple market stability in *Value and Capital* ([1939] 1946) by generalizing the stability condition for a single market. Samuelson (1941) argued that Hicks's conditions did not represent "true dynamic stability." Samuelson's tâtonnement adjustment mechanism (T) and his stability condition – negative real parts of the characteristic roots of the excess demand Jacobian evaluated at equilibrium prices – became the standard tool for talking about *local* stability in Walrasian theory. The literature on the local stability

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of the Walrasian tâtonnement that appeared in a steady stream during the next twenty years focused primarily on trying to find reasonable economic restrictions that would be sufficient for Samuelson's matrix condition. The analysis of global stability came later during the late 1950s as a result of applying Liapunov theory; the conical results were provided in Arrow and Hurwicz (1958) and Arrow, Block, and Hurwicz (1959). These papers proved that the Walrasian general equilibrium price vector (p^*) would be unique and globally stable under a variety of specific restrictions (gross substitutes being the most important).

Samuelson's initial papers on local stability were published in 1941 and 1942, but they were included in Part II of *Foundations* as chapters nine and ten. It is significant that *Foundations* was divided into two separate parts. The first part discussed economic models where the equilibrium was a maximum or minimum of some function (extremum problems). The examples in Part I were the topics that would come to dominate microeconomic textbooks during the next few decades: consumer choice (demand) theory, cost and production, profit maximizing firm behavior, welfare economics, etc. Part II was also about comparative statics, but it examined models where the equilibrium could not be associated with the maximum or minimum of any function. Comparative statics results are more difficult in such cases because of the weaker mathematical restrictions in such problems (in particular, no second order conditions). For these non-optimization-based models Samuelson proposed the *correspondence principle*. It employed similar mathematical techniques, but used the *stability* of the model, rather than optimality, to obtain comparative statics results. The explicit motivation for discussing this class of models and subsuming them under the same formalism was the fact that Keynesian models (business cycle theories) are of this second, non-optimization-based, kind. As Samuelson explains in the introduction to *Foundations*:

> However, when we leave single economic units, the determination of unknowns is found to be unrelated to an extremum position. In even the simplest business cycle theories there is lacking symmetry in the conditions of equilibrium so that there is no possibility of directly reducing the problem to that of a maximum or minimum. Instead the dynamical properties of the system are specified, and the hypothesis is made that the system is in 'stable' equilibrium or motion. By means of what I have called the *Correspondence Principle* between comparative statics and dynamics, definite *operationally meaningful* theorems can be derived from so simple a hypothesis. (Samuelson 1947: 5)

Important to the story here is the fact that Samuelson's discussion of the stability of the Walrasian tâtonnement (in fact all of his explicit discussion

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of the Walrasian model) was contained in Part II (the non-optimization-based Keynesian part) of *Foundations*. Chapter nine – which was Samuelson (1941) – starts out discussing the correspondence principle, moves to the stability of two-dimensional market models, then the stability of Walrasian multiple-market general equilibrium (his criticism of Hicks, his main stability result, etc.), and finally analyses a 3-variable, 3-parameter, Keynesian model. For Samuelson, the analysis of Walrasian dynamics was more like the analysis of a Keynesian model than the microeconomic theory discussed in Part I of *Foundations*. Of course, Samuelson, like others working on Walrasian models during this period, was assuming that utility maximizing consumers and profit maximizing firms were in some sense "behind" the excess demand functions of in the tâtonnement (T), but Walrasian dynamics was nonetheless directly linked, by formal structure and in its dependency on the correspondence principle, to Keynesian economics.

Another important contributor to the neoclassical synthesis – perhaps even more self-conscious about forging a synthesis than Samuelson – was Oscar Lange. The goal of Lange's *Price Flexibility and Employment* (1944) was to restate general equilibrium "in a way which explicitly takes account of money" (Lange, 1944: ii). The second paragraph of Lange's preface lists the economists who most influenced the study and it reads like a who's who of the neoclassical synthesis: Keynes (on the "substitution between money and goods"), Hicks (for providing the "most up-to-date formulation of the theory of general economic equilibrium"), and Samuelson (for the "dynamic theory of stability of economic equilibrium"). Key to Lange's analysis in *Price Flexibility* is Samuelson's version of the Walrasian tâtonnement (T). Prices which obey (T) exhibit price flexibility (p. 2). The purpose of his analysis was to investigate the relationship between price flexibility in this sense and "employment and economic stability" (p. 1). The book was thus an attempt to combine Walrasian general equilibrium theory – particularly the stability analysis of the Walrasian tâtonnement – with a Keynesian analysis of unemployment and economic stability (in the macro sense). Although the argument was far from tight, the often-repeated theme in the book was that "flexibility of factor prices fails to assure full employment of factors of production" (p. 51) unless a number of additional conditions are satisfied. For Lange, like Keynes, full employment was a rare event in a competitive market economy: for Lange this even applied to Walrasian general equilibrium.

Lange also used Walrasian theory to make Keynesian theoretical points in his paper on Say's Law (Lange 1942). He makes the distinction between Walras's Law (valid in a general equilibrium system) and Say's Law (invalid in such a system). Again the Walrasian formalism was being used to make

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Keynesian political–economic points. Since this section is getting quite long, I will simply note that many others who participated in the theoretical literature of the neoclassical synthesis – Alvin Hansen (1949), Abba Lerner (1944), Don Patinkin ([1956] 1965), James Tobin (1958, 1969) and others – also combined elements of general equilibrium theory with elements of Keynesian economics and did so using much the same formula as Lange (though often with more moderate politics).[19] The Walrasian model formed the theoretical backbone – with a strong emphasis on stability analysis – and the Keynesian influence entered on the money/interest and policy sides.

Some of the argument presented in this section, regarding the close relationship between Walrasian stability and the neoclassical synthesis, has been presented in Roy Weintraub's *Stabilizing Dynamics* (1991). Weintraub argues that prior to the neoclassical synthesis terms like "equilibrium" and "stability" had a variety of different meanings – the discourse was not stabilized – with different economists and texts using the terms in different ways. He argues that one of the driving forces behind the ultimate stabilization that took place during the 1950s and 1960s (basically that "dynamic" meant that the system was specified explicitly in terms of differential or difference equations and "stability" meant convergence to equilibrium as $t \to \infty$) was the effort to reconcile the idea of general equilibrium with ostensibly "disequilibrium" phenomena of involuntary unemployment. As he explains:

> The literature associated with Frisch, Tinbergen, Hicks, and finally Samuelson was associated with understanding the conditions under which an equilibrium would be stable, so as to permit the conjunction of equilibrium theorizing and unemployment analysis. (Weintraub 1991: 123)

This means that the neoclassical synthesis played an essential role in stabilizing dynamics (in general equilibrium theory and in economics more generally).

> The mathematization of equilibrium and stability, the papers from Samuelson on through Arrow and Hurwicz, stabilized that discourse … The restriction of 'dynamic' to 'dynamical system,' and the construction of 'stable' to 'locally stable equilibrium motion of a dissipative dynamical system,' permitted concurrence … on the meaning of the claim that unemployment was a disequilibrium position associated with a 'usually' stable competitive equilibrium. The neoclassical synthesis was literally unthinkable before the availability of the mathematization of equilibrium and stability. (Weintraub 1991: 125)

Although my argument is not inconsistent with Weintraub's, I emphasize a different aspect of the story. There were no "stabilized dynamics" before the neoclassical synthesis. The sequential tâtonnement of Walras was quite

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different from the (more Keynesian accommodating) tâtonnement process of the later literature (T) and discussion of the tâtonnement had all but disappeared from the Walrasian literature by Pareto's most mature work. The "price adjustment mechanism" was not a significant part of general equilibrium theory in the period immediately preceding the work of Hicks and Samuelson; it *became* a significant part of the Walrasian research program during the 1940s and it became so in part because of the neoclassical synthesis and the concerns of Keynesians economics.

In closing this section I would like to emphasize how *important* the topics of stability and Walrasian dynamics were to general equilibrium theorizing during the heyday of the 1960s. For example, Arrow and Hahn's *General Competitive Analysis* (1971) – the canonical summary of the literature – dedicated far more pages to stability than any other topic. The book had fourteen chapters (and a number of mathematical appendices); there was one chapter on consumer choice, one chapter on production theory, and one chapter on existence, but there were *three* chapters on stability analysis (two on the traditional tâtonnement and one on alternative ways of modeling general equilibrium dynamics). Add to this the fact that there was an entire chapter on "The Keynesian Model" and it becomes clear how important stability analysis and its connection to Keynesian economics was for Walrasian microeconomics during this period. The purpose of this section has been to show that that would not have been the case if the Walrasian economics of the day had not been a product of co-evolution with Keynesian economic theory.

3.3 Reversibility, Path-Dependency, and All That

This topic is related to the stability discussion in the previous section, but it can be separated from the way that Walrasian tâtonnement dynamics jelled together with Keynesian notions of disequilibrium and unemployment. One feature of Walrasian models (of any sort) and Keynesian models (of the IS-LM sort) is an absence of path-dependencies, irreversibilities, reference-dependence, hysteresis, endowment effects, or any other properties where the path or initial position influences the characteristics of the equilibrium (or which equilibrium is) reached. Disequilibrium adjustment can be characterized in both models, but the process/mechanism by which the equilibrium is reached has no impact on the resulting equilibrium position.

The story on the microeconomic side is fairly familiar. In recent years a vast amount of empirical evidence from experimental and behavioral economics suggests that such path-dependencies and irreversibilities are pervasive features of human choice (in laboratories and in markets) – see

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for example Camerer and Loewenstein (2004), DellaVigna (2009), Kahneman (2003), Kahneman, Knetsch, and Thaler (1990), Knetsch (1989, 1992), or Thaler (1980) – but such effects are entirely absent from Walrasian choice theory. For Walrasian theory, the individual consumer has well-ordered preferences (and thus a well-behaved ordinal utility function) and chooses the most preferred bundle (maximizes utility) from the affordable set. The consumer is assumed to have fixed preferences, infinitely fast computational ability, and to move instantaneously to the optimal bundle – as Nicholas Georgescu-Roegen once put it, the agent's behavior is like a "bird" that drops down instantaneously on the optimal bundle, rather than like a "worm" that actually moves through the choice space in real time to arrive at the optimal choice (Georgescu-Roegen 1968: 255). Of course, if the behavior were worm-like, the particular path taken might matter to the final choice (path-dependency) and reversing the parameter change that initiated the choice might not return the consumer to the initial position (irreversibility). Obviously, this feature is common to many neoclassical-based models of individual choice and is not restricted to Walrasian choice theory, but – and here is the point – such path-dependencies and irreversibilities *were* common features of many of the demand theories the Walrasian program was competing against during the 1930s and 1940s.[20] Many of the different competing approaches to choice/price theory listed above in the discussion of interwar pluralism, were motivated by the idea that economic agents do not have stable and reversible preferences, infinitely fast computational abilities, act instantaneously, and so forth. These issues, present in the recent literature on experimental and behavioral economics, disappeared with the ascension of the Walrasian version of choice theory. The question is: How does all this relate to Keynesian economics?

The fact is that Keynesian economics – at least in the form it came to take during the neoclassical synthesis – was also characterized by hermetic separation of the equilibrium position from any dependency on, or influence from, the process/path by which that equilibrium is reached. In "What was Lost with IS-LM?" Roger Backhouse and David Laidler (2004) discuss a number of problems associated with the passage of time and related issues that concerned macroeconomists during the interwar period, but disappeared from discussion once the profession came to accept the IS-LM framework. As they explain: "All of these matters had received widespread attention in the interwar literature, but the wholesale adoption of the static IS-LM framework form the 1940s onward led to their falling into neglect" (Backhouse and Laidler 2004: 31). At the same time that the Walrasian program was rising to dominance in microeconomics and thus facilitating the profession's dismissal of many of the issues of time and path that

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concerned microeconomists during the interwar period, the rise to dominance of textbook Keynesianism facilitated a similar dismissal of time-related issues within macroeconomics. Micro and macro both stabilized around theoretical frameworks where the "dynamic structure of the world plays no role in determining the equilibrium toward which the economy converges" (Backhouse and Laidler 2004: 32).

So given the argument in the preceding paragraphs, it does seem that neoclassical synthesis micro and macro were very similar on the issues of path and time – and perhaps defeated competitors that allowed for path-dependency and irreversibility on both the micro and macro side – but how does this show that Keynesian ideas influenced Walrasian theorizing? To answer this, notice how the literature on the Walrasian tâtonnement is based on an entirely different strategy for answering the question of *how the competitive market* reaches equilibrium than Walrasian theory employs when answering the question of *how the individual economic agent* reaches equilibrium (optimal choice). In the case of the individual economic agent (intra-agent equilibrium), Walrasian theory makes equilibrium instantaneous and avoids all issues associated with time, path, initial position, irreversibility, or the dynamic process of "getting there." The Walrasian consumer does not grope around in, or converge to, the optimal bundle by moving through the choice space; they are essentially always in equilibrium. One can of course do comparative statics exercises in such models and compare one equilibrium to another, but no time passes (even logical or virtual time) between the two equilibrium positions, the change does not affect either equilibrium, and reversing the initial parameter change would simply take the consumer back to the original point. Notice how different this is from the Walrasian tâtonnement.

The tâtonnement is also timeless in the sense that no trade takes place until the equilibrium price vector (p^*) is reached, but the variables are tracing out paths in "time." This "time" has a natural direction; it makes no sense to talk about "reversing" the dynamic system (T). In (inter-agent) equilibrium the motion simply stops – $dp_i / dt = 0$ for all $i = 1, 2, \ldots, n$. Since stability implies $\lim_{(t \to \infty)} p(t) = p^*$, and no trade takes place until p^* is reached, this process may take a very long "time." It is analytical (or virtual) "time" and not real time, but there is still a big difference between this notion of "getting to" equilibrium and the instantaneous choice of the Walrasian agent. The "behavior" of the Walrasian auctioneer is conceptually quite different than the "behavior" of the Walrasian agent and in particular, the tâtonnement (T) allows for "stickiness" or "disequilibrium" in a way that is inconceivable for the Walrasian agent. If, contrary to the Walrasian models of the neoclassical synthesis, the equilibrium in the Walrasian market were

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modeled in the same way as the equilibrium of the Walrasian consumer there would be no tâtonnement "adjustment"; the competitive price system would always be in equilibrium. In fact, in the late 1970s when the New Classical macro of Robert Lucas (1981) replaced Keynesian macro this is exactly the way general equilibrium was discussed. There was no "adjustment"; the economy was always in equilibrium in the same way that the Walrasian agent is always in equilibrium. As Kevin Hoover explains, general equilibrium in the Lucas model means that "self-interested economic agents successfully maximize their utility or profits subject to constraints on their budgets and, crucially, on available information" (Hoover 1988: 42). The Lucas model is strictly Walrasian in that the representative agent does what Walrasian agents have always done. No tâtonnement is needed. As Lucas himself says: "the idea that an economic system in equilibrium is in any sense 'at rest' is simply an anachronism" (Lucas 1981: 287). Yes, an anachronism of the neoclassical synthesis.

So, in the end, it seems that this discussion of individual versus market behavior has left us at a point similar to where we were at the end of section 3.2. The conception of "dynamics" that stabilized in Walrasian general equilibrium theory during the 1950s was in part driven by a desire to find a notion of multi-market competitive equilibrium that was consistent with a version of Keynesian unemployment. What this section has added is that the stabilization was not only about equilibrium and stability in a competitive market, but also about the characterization of the behavior of the individual economic agent. Walrasian economics overcame (or circumvented, or suppressed, depending on your point of view) the path-dependency and irreversibilities that were a concern of a number of non-Walrasian theories of demand during the interwar period, and it also overcame these same issues concerning time and position in the theory of market adjustment, but the two "solutions" were quite different; and different in part because of *Keynesian concerns on the market side*. Later, freed from these Keynesian concerns, New Classical Walrasians such as Lucas endorsed a more consistent Walrasian view where equilibrium in agents and markets meant essentially the same thing.[21] It seems reasonable to conclude that the combination of instantaneously optimizing agents and tâtonnement adjusting competitive markets that characterized microeconomics during the period of Walrasian high theory – present in Walrasian theory neither before, nor after, the neoclassical synthesis – was a product of its co-evolution with Keynesian theory.

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3.4 Income Matters

As discussed in section 3.1, the "Walrasian" demand theory that emerged triumphant during the neoclassical synthesis descended more from Pareto than Walras, and it was put in essentially its final (calculus-based) form in Slutsky ([1915] 1952) and Hicks and Allen (1934). Out of all the various contributions and contributors during the half-century stabilization of demand theory, Slutsky ([1915] 1952) – translation published in 1952 – is generally considered to be the key development. Over the last decade there has been a substantial amount of historical research on Slutsky and we now know quite a lot more about his life and work (for example, Barnett 2004, Chipman 2004, Chipman and Lenfant 2002, Weber 1999a. 1999b), but the one question that does not seem to have been adequately answered is: Why has the Slutsky equation – the "fundamental equation of value theory" (Hicks [1939] 1946: 309) – had such a prominent position within microeconomic theory and economic education since the 1950s? The argument in this section will be that this is in part because of the neoclassical synthesis and the impact of Keynesian economics.

If one is thinking about which of the various theories of demand from the 1930s and 1940s would best "fit" with Keynesian macroeconomics, the Walrasian formulation has one obvious advantage: Walrasian demand functions have nominal income as arguments. If one is trying to meld consumer choice theory and Keynesian economics then one needs to be able to explain how changes in nominal income have real effects on individual behavior. Income matters in Keynesian economics and income must matter in any demand theory that is going to live comfortably with Keynesian theory.[22] This feature of Walrasian demand theory certainly gave it a big advantage over various demand theories that did not have such income effects; for example Hotelling (1932) with no income term at all, or various versions of Chicago price theory where income effects are compensated away (Friedman 1953). Although this explains why Walrasian theory made the short-list for the neoclassical synthesis – it fulfilled an important necessary condition – it does not provide much explanation for the particular emphasis on the Slutsky equation. But there is a synthesis-based story for that as well.

Perhaps at this point it would be useful to write down the Slutsky equation,[23]

$$\frac{\partial x_i^h}{\partial p_j} = S_{ij}^h - d_j^h \frac{\partial d_i^h}{\partial M^h} \qquad \text{(S)}$$

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and the main results: the own Slutsky substitution terms are strictly negative ($S^h_{ii} < 0$), the cross-substitution terms are symmetric ($S_{ij} = S_{ji}$ for all $i \neq j$), and the $n \times n$ matrix of Slutsky substitution terms $S^h = [S^h_{ij}]$ is negative semi-definite ($x^T S^h x \leq 0$ for all $x \neq 0$). The substitution terms (S^h_{ij}) show the change in the consumption of the good caused exclusively by a change in relative prices; the remainder of the expression is the income effect which shows the change in the consumption of the good caused by a price-induced change in real income.

Notice that (S) is in many ways the perfect expression of the neoclassical synthesis. It decomposes observed changes in consumption into a micro part (the change based on only relative prices) and a macro part (the change based on a change in the purchasing power of money). According to the macroeconomic/monetary theory preceding (and following) the dominance of Keynesian macroeconomics we should keep these two things strictly separate: changes in relative prices are an issue for value theory while changes in the purchasing power of money are an issue for monetary theory, and a strict dichotomy should be maintained between these two types of economic theory. The Slutsky equation not only violates this strict dichotomy, it harmonizes micro and macro into one simple expression.

As the Frank Knight quote in the epigraph demonstrates, this was precisely the criticism of the Walras–Hicks–Slutsky demand theory raised by certain members of the Chicago school during the 1940s and 1950s and one of their main reasons for advocating an alternative – more purely micro – theory of demand. As Knight explains:

> The treatment of the Slutzky school adopts the assumption that the price of X varies under the condition that the prices of all other goods (and the consumer's money income) are constant. Hence real income must change … It throws together two distinct effects upon consumption, the "price effect" and the "income effect." The treatment then proceeds to separate these by means of an ingenious analysis. The cleverness of it all must be conceded. But it is called for only because of an initial confusion in the statement of the problem which is wholly unnecessary and should clearly be avoided … The "income effect" of Slutsky et al. is merely a particular case or mode of change in the purchasing-power of money, or the price level: and it is this problem as a whole that should be isolated and reserved for special treatment. (Knight 1944: 299)

> An approach to any particular price problem which "jumbles" effects of change in the purchasing-power of money (however caused) with effects of change in the relative value for purchasing different things is mere gratuitous confusion. (*ibid.*: 300)

Friedman also made similar (though less dramatic) comments in a number of works. For example, his paper on the Marshallian demand curve argued

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that "the separation of the theory of relative prices from monetary theory" was extremely important and it was one of "Marshall's basic organizing principles" which led him to use "a constant purchasing power of money as a means of impounding monetary forces" (Friedman 1953: 66). Friedman argued that Marshall, unlike Walrasian theorists, correctly "recognized the desirability of separating two quite different effects and constructed his demand curve so that it encompassed solely the effect that he wished to isolate for study, namely, the substitution effect" (*ibid.*: 64–5).[24] Similar discontent with the standard Walrasian–Slutsky distinction between substitution and income effects was expressed in Friedman's famous "Provisional Text":

> To summarize, general considerations suggest the desirability of having two functions. One function should be so defined as to summarize the forces affecting the demand for the commodity in question operating via relative prices. In this function, real income should be held constant. The other function should be so defined as to summarize the forces affecting the demand for the commodity in question via real income. In this function, relative prices obviously should be held constant. A function of this latter type is the Engel curve, which relates quantity demanded and real income. The ordinary demand function is intended to provide a function of the former type but does not do so, because changes in real income are not rigorously excluded. (Friedman 1962: 30)

So the bottom line is that the elevation of (S) to the fundamental equation of value theory, like the elevation of (T) to the fundamental equation of Walrasian dynamics, was a product of the neoclassical synthesis. Having nominal income as an argument in demand functions helped the Walrasian theory win out over its immediate competitors, but the context of the neoclassical synthesis – with its mandate for micro–macro harmonization – also helps us understand how the Slutsky equation came to be so important in demand theory. Of course, like all of the other topics discussed in sections 3.1–3.3, the argument is not that the Keynesian connection was the only reason for the prominence of the Slutsky equation, but it is an additional factor not discussed in traditional explanations of the equation's influence (for example, Chipman and Lenfant 2002). All things considered, (S) does seem to be an elegant way to demonstrate that "the broad cleavage between microeconomics and macroeconomics has now been closed" (Samuelson 1955: 360).

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4. EVERY GOOD THING MUST END (OR THE DOWN-SIDE OF CO-EVOLUTION)

This section will extend the argument about the influence of Keynesian ideas on the rise and character of Walrasian economics to the question of the Keynesian contribution to the fall of Walrasian microeconomics. The discussion here will be less detailed and more suggestive than the argument in the previous section. One reason is simply that this is already a long paper and a serious analysis of the fall would require a lot more space than is available here, but there are other reasons as well.

First, it is still an open question whether there really has been, or the degree to which there really has been, a "fall" in Walrasian microeconomics during the last few decades. Although many consider it to be obvious that the profession has moved on (for example, Colander 2000, 2006; Colander, Holt and Rosser 2004a, 2004b; Davis 2006, 2008; Rizvi 2003; and many others), it is also clear that Walrasian microeconomics is still an active research program[25] and it remains the standard framework in textbooks at every level.

Second, it might be argued that the profession has in fact "moved on," but the movement reflects more the "maturity" of the Walrasian research program than its failure (Colander 2000, Davis 2006). It may be that Walrasian general equilibrium theory is simply a "completed" research program; the task was to investigate the full implications of the assumption that all economic agents behave in a perfectly competitive manner and that task has now been completed. From this point of view economics is now post-Walrasian, but simply because the profession has successfully completed the research project that had been its main focus for the previous two hundred years.

Third, in addition to the question about the existence of the fall is a broader question about the future of neoclassical economics and rational choice theory more generally. Walrasian microeconomic theory is a particular version of neoclassical economics and to some extent utilizes rational choice theory in its characterization of individual agents. But the extensive literature in experimental and behavioral economics that finds repeated systematic empirical violations of rational choice theory and/or the stability and reversibility that is characteristic of preferences in neoclassical models, clearly has implications for the future of Walrasian microeconomics. If rational choice theory somehow comes to be replaced as the core organizing framework for the way that economists think about, model, and formalize individual behavior, then certainly Walrasian economics is in for an even bigger downturn than it has experienced during the

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last few years. Of course these issues are still in flux and that severely limits what one can say about the "fall" of Walrasian economics at the current time.

Finally, there is the question of the current status of Keynesian economics. Twenty-five or so years ago it was quite clear that Keynesian economics had fallen from grace within the economics profession (although not necessarily from policy makers and undergraduate instruction[26]), but now, with the recent financial crisis and world economic recession, this is less clear. It is possible there will be a Keynesian revival in macroeconomics and finance within the profession during the next few years, but then again, maybe not. The point is simply that we may be in a period of significant change within the discipline of economics – change that involves both Walrasian and Keynesian theory – and that presents a serious challenge to an attempt to provide an historical analysis of the fall of Walrasian theory.

Taking into consideration all that was said in the previous three paragraphs, I do think it is possible to say a few things about the "fall" of Walrasian microeconomics and its relationship to Keynesian theory and the neoclassical synthesis. First of all, whether Walrasian economics has taken, or will take, a terminal fall or not, it is clear that it has been demoted during the last few decades. Second, whether Keynesian economics stages a comeback or not, during the late 1970s and 1980s it definitely fell from the position it held during the heyday of the neoclassical synthesis. Not only does it seem reasonable to accept these stylized facts of the history of modern economics, it also seems reasonable to agree about some of the causes for both of these events. On the Walrasian side it is clear that the failure of *stability analysis* – starting in the 1960s with the counterexamples by David Gale (1963) and Herbert Scarf (1960), and exacerbated by the SMD results which opened the floodgates for more counterexamples[27] – and the associated failure to prove uniqueness raised serious challenges to the hegemony of the Walrasian program. As Alan Kirman explained, without "stability or uniqueness, the intrinsic interest of economic analysis based on the general equilibrium model was extremely limited" (Kirman 2006: 257). With respect to Keynesian economics the inflation and supply-side oil shocks of the 1970s were clearly empirical factors, and the rise of monetarism and the failure of the microfoundations project certainly contributed on the theoretical side.[28] It is not necessary to debate the details, or weight the relative significance, of any of these factors; for the purposes here all that is required is agreement that the Walrasian program has faltered during the last decade or so, the Keynesian program was seriously questioned by the 1980s, and that the things mentioned here on the theoretical and empirical side played some role in these negative developments. Given all this it seems quite clear that there are a few ways in

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which Keynesian economics – and the previous co-evolution of the two research programs – contributed to the decline of Walrasian microeconomics.

The main point of this section will be that Walrasian theory ran into trouble *at precisely the points where the Keynesian influence was most pronounced*. Consider stability first. Almost all of the serious theoretical problems associated with Walrasian microeconomics revolve around the *stability* of the tâtonnement adjustment mechanism (T). The problem is that the tâtonnement process is globally stable only when very restrictive additional assumptions are imposed on excess demand functions (and local stability is only slightly easier): these include gross substitutes, the Weak Axiom of revealed preference holding on aggregate excess demands, a dominant diagonal on the excess demand Jacobian matrix, and several others (see chapters 11 and 12 of Arrow and Hahn 1971). These assumptions are restrictive in at least four different senses. First, they are over and above what is implied by the standard assumptions on consumer and firm behavior (they are theoretically restrictive). Second, there is no obvious reason why the behavior they would require should actually be the behavior of consumers and firms in a competitive market economy (they are empirically restrictive). Third, they are much more restrictive than what is required for existence of competitive equilibrium (the main positive result of the research program). And fourth, they are only sufficient, not necessary, conditions. The goal of stability theory was to find out what stability implied – in the same way that Slutsky, Hicks, Allen, and others found out what income-constrained utility maximization implied – but all that could be found were a variety of different conditions that implied stability (not what stability implied). Of course, there were also problems with uniqueness, but in every special case where the tâtonnement process is globally stable the equilibrium price vector (p^*) is also unique (see chapter 9 of Arrow and Hahn 1971). And there were also problems with comparative statics, but again the problem is really about stability. The correspondence principle attempts to derive comparative statics *from* stability, but if the stability results are not available, or weak, or economically uninterpretable, then the comparative statics results inherit these same problems. As Arrow and Hahn explained:

> Thus what the "correspondence principle" amounts to is this: Most of the restrictions on the form of the excess-demand functions that are at present known to be sufficient to ensure global stability are also sufficient to allow certain exercises in comparing equilibria. It should be added that these same conditions also turn up in the discussion of the uniqueness of competitive equilibrium. All these restrictions share the characteristic that they are not

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> necessary for the task for which they were invented; they are only sufficient and this explains why the correspondence principle "isn't." (Arrow and Hahn 1971: 321)

As discussed in section 3.3, the stability of the system of differential equations (T) was not part of the original Walrasian model and by Pareto's *Manual* there was no discussion of multiple market stability at all. It was also not a part – and was considered one of the problems of the earlier theory – in the macro-Walrasian general equilibrium theory that came after the Keynesian fall from grace. As Robert Lucas (1981: 278–9) explains:

> Samuelson proposed a dynamic model of price adjustment in which the rates of change of prices offered in each market were related to the level of "excess demands" in all markets. Whatever the history or underlying objectives of this model of price dynamics … this theory introduced sufficient additional (to those needed to describe tastes and technology) parameters to the equilibrium system so that, given an initial shock to the system, a wide variety of paths were consistent with its eventual return to equilibrium.
>
> This introduction of additional … free parameters held out the promise that one could construct a theoretical system the stationary point of which was a general equilibrium in the neoclassical sense but whose movements, out of equilibrium, might replicate the "Keynesian" behavior captured so well by the econometric models … The objective of the enterprise was widely agreed to be "unification" of the two types of theories into which Keynesian ideas were translated in the 1930s and 1940s.[29]

The idea that the stability of the tâtonnement process should be one of the most important issues for Walrasian general equilibrium theory was a product of the neoclassical synthesis and the effort to unify Keynesian and Walrasian economics. It is clear from the contributions of those like Lange and Samuelson in the 1940s, and it is also clear from its rejection in the post-synthesis Walrasian macro literature of those like Lucas. The stability of the tâtonnement ultimately became a very serious problem for Walrasian economics, and it was a problem that developed right at the particular point in the theoretical edifice where the Keynesian co-evolution had left its greatest impact on Walrasian theory.

But this is not all there is to the story; there is yet another Keynesian aspect to the theoretical difficulties that developed within Walrasian general equilibrium theory. Not only was the stability of the neoclassical synthesis-inspired tâtonnement the main problem, "the stability problem" was itself a product of the *income terms* in the Hicks–Slutsky version of Walrasian demand theory. The effort to forge a seamless connection between Walrasian and Keynesian theory contributed to an emphasis on both tâtonnement dynamics and income effects; the income effects were the

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primary cause of instability in Walrasian models which in turn became the most important theoretical difficulty for the Walrasian program.

To see the problem, recall the discussion from section 3.4 above and the Slutsky expression given in (S). As noted there, the Slutsky matrix is negative semi-definite,[30] but consumer choice theory imposes no sign restrictions on the income effects (goods could be normal or inferior). As noted above, the standard way of proving global stability was to find a Liapunov function. Applying the Liapunov result from this period to the problem of Walrasian stability, we have that if there exists a function $V[p(t)]$ defined over the price path $p(t)$ generated by (T) with the following three properties (glossing over various mathematical complexities and assuming the numéraire good has been eliminated):[31]

a) $V[p(t)] > 0$ for all $p \neq p^*$,

b) $\dfrac{dV[p(t)]}{dt} < 0$ for all $p \neq p^*$,

c) $\dfrac{dV[p^*]}{dt} = V[p^*]$

then the equilibrium price vector (p^*) is globally asymptotically stable. One popular Liapunov function was:[32]

$$V\left[p(t)\right] = \frac{1}{2}\sum_{i=1}^{n} z_i^2\left[p(t)\right]$$

Computing the time derivative of this Liapunov function we have:

$$\frac{dV\left[p(t)\right]}{dt} = z^T[p(t)]JZ\left[p(t)\right]z[p(t)] \text{ for all } p \neq p^*,$$

where the right-hand side is a quadratic form of the excess demand Jacobian matrix JZ. *If* the matrix JZ forms a negative definite quadratic form, then the expression on the right-hand side will be negative for all nonequilibrium prices and equal to zero at equilibrium, which in turn implies the tâtonnement is globally stable.

But the matrix JZ – remember it is the Jacobian of the excess demand function (demand minus supply) – will consist of three separate parts,

$$JZ = S - M - F,$$

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where the S and M matrices are from the market demand functions – they are the market equivalents of the substitution and income effects in (S) respectively – and the F matrix is the supply function Jacobian (see Mukherji 1974: 247–8). We know the S matrix is negative definite from the standard Slutsky results on demand functions and we also know that the traditional assumptions on the production side of the Arrow–Debreu model guarantee that the matrix F is positive semi-definite (Arrow and Hahn 1971: 72). So both of these terms are signed the "right way" for stability. Neither the substitution effects on the demand side nor the supply side are a problem. The only problem – the only matrix that is not signed the "right way" by the standard assumptions – is the matrix of income effects (M above). This means that the full burden of all of the stability "problems" in general equilibrium systems rests with the income terms on the demand side of the market excess demand functions. The various conditions that have been demonstrated to be sufficient for global stability during the late 1950s and 1960s all amount in various ways to getting around these problematic income effects.

So it seems that Keynesian economics must bear some responsibility for the fall, or at least faltering, of Walrasian microeconomics during the last few decades. Any neoclassically-inspired theory that could get along with Keynesian economics well enough to form a stable partnership would need to be able to account for unemployment and disequilibrium in an otherwise general equilibrium world and it would need to have nominal income matter to consumer choice behavior: thus (T) and (S). And yet these two aspects of the Walrasian theory of the neoclassical synthesis are right at the heart of the Walrasian program's later technical difficulties. The features of Walrasian theory that were most influenced by its co-evolution with Keynesian economics during the middle of the twentieth century were precisely the features most responsible for its decline at the end of the century.

5. CONCLUSION

This chapter has tried to make a fairly simple point. What Keynesian and Walrasian economics evolved into – what they *became* – when they stabilized into textbook macro and (advanced) textbook micro during the 1950s and 1960s, was, at least in part, a result of the fact they were joined together in, and co-evolved within the context of, the neoclassical synthesis. Even if it is assumed they each contained certain core concepts identifiable across time, there were particular features of each in the later period that emerged because those particular aspects had survival value for the synthesis they

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were both a part of, and thus, because of the presence of the other theory. Many historians of economic thought readily accept that textbook Keynesian economics in its heyday was what it was at least in part because of its compact with Walrasian economics. My point was simply that the synthesis involved adaptations by both research programs and that influence flowed both ways. The neoclassical synthesis was made, not found.

In section 2, I listed four ways that Keynesian ideas contributed to the eventual success of Walrasian micro over its immediate theoretical competition and/or influenced the content of the theory in its final form. Frankly, taken in isolation, none of these points would be very significant, but I believe that taken together they provide a substantial amount of new insight into how the co-evolution of the two research programs manifested itself on the Walrasian side. One of the main arguments in this section was that the equilibrium within mid-twentieth century Walrasian economics – unlike the Walrasian tâtonnement – was highly path-dependent. In section 3, I tried to show how the main theoretical problems of Walrasian theory at the end of the century – primarily stability, but also related issues such as uniqueness and comparative statics – emerged at precisely the point within the Walrasian program where the Keynesian imprint was most visible.

Although I believe the story told here is an important explanation of both the rise and the fall (or faltering) of Walrasian microeconomics, I noted repeatedly in the chapter that I was in no way attempting to provide the only, or perhaps even the main, reason that Walrasian economics took the particular path that it did during the twentieth century. There are many other forces at work in the life-history of Walrasian economics – other forces that, at particular moments, may have mattered more than the Keynesian connection. My point was simply that Keynesian theory mattered – something not generally recognized – not that it was the only thing that mattered.[33]

NOTES

1 This chapter was initially presented at The First International Symposium on the History of Economic Thought "The Integration of Micro and Macroeconomics from a Historical Perspective," University of São Paulo, São Paulo, Brazil, August 3–5, 2009. Later versions were presented at the Center for the History of Political Economy at Duke University, October 2009; The Amsterdam–Cachan Workshop on the History and Methodology of Economics at the University of Amsterdam, December 2009; The American Economics Association Meetings in Atlanta, January 2010; and the Western Washington University Economics Department, February 2010. I received helpful comments from numerous people at these various venues.

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3 In addition to books of course, the history of general equilibrium theory, and mid-twentieth century microeconomics more generally, has received extensive discussion in history of economic thought journals, Handbooks and Companions, and to some extent journals in economic theory. A number of *History of Political Economy* annual conferences have also focused on aspects of the story.
4 As Coddington put it: "what Hicks was supposed to have done was to have taken the pristine work of Keynes's *General Theory* and, via a kind of Walrasian sleight of hand, transformed the profound and intellectually subversive message into something innocuous, insipid and even lifeless" (1983, xi).
5 I owe this way of thinking about "identity" – in terms of individuation and reidentification – to John Davis (2003).
6 This was primarily an Anglo-American phenomenon, but given the historical context of the immediate post World War II period, it came to characterize the discipline in general.
7 Note the "neoclassical synthesis" here and throughout refers to the original neoclassical synthesis and not the "new neoclassical synthesis" of dynamic stochastic general equilibrium (DSGE) models (Clarida, Gali, and Gertler 1999, Goodfriend and King 1997). See the Duarte chapter in this volume for a detailed discussion of DSGE models.
8 I will follow tradition and use the term "neoclassical synthesis," but in fact the term "synthesis" does not really capture the relationship very well. A synthesis suggests two things coming together to form a third that is unique and different from each of the things that entered into it: like the synthesis of water from hydrogen and oxygen. But the neoclassical synthesis was not like this. Microeconomics and macroeconomics remained identifiable and distinct fields; they did not disappear as separate entities upon the formation of the neoclassical synthesis. The main point of this chapter is that although Walrasian economics had certain core conceptions that were identifiable over time, it also evolved and changed in response to, and because of, its contact with Keynesian economics. This seems much more like *co-evolution* than synthesis. Each program remains distinct – it retains its own genetic material and some aspects of its earlier behavior – but also changes in various ways because it has formed a partnership with another research program. I will argue that what Walrasian economics was in the 1960s was in part because of its relationship with Keynesian economics – and the interaction of the partnership with the environment in which these two sets of ideas competed and survived – and yet it always maintained a separate identifiable existence.
9 As Luca Fiorito explains, focusing on the (first level) strife of radically different research programs blinds us "to the 'second level' of pluralism which characterized US interwar economics, i.e. not just variety *among* schools of thought but also variety *within* each school. This was true of that variegated universe that was pre-WW II American neoclassicism …" (Fiorito 2000: 269). Also see Backhouse (2003) and Morgan and Rutherford (1998).
10 As historians of biology have noted about the Darwinian synthesis of experimental genetics and Darwinian evolutionary theory (during roughly the same time period as the neoclassical synthesis): "From the beginning, founders of the synthetic theory were concerned to make the modern synthesis appear as complete and coherent as possible" (Hull 1988: 204).
11 Including the other factors I have examined in previous research (for example, in Hands 1994, 2006, 2007, 2010a, 2010b; Hands and Mirowski 1998; or Mirowski and Hands 1998).
12 Only under very restrictive conditions can market demand be written as a function of the n prices and total income M = summationM^h. This is one version of the notorious aggregation problem in demand theory. See the Hoover chapter in this volume for a discussion of the aggregation problem in macroeconomics.
13 See Hands (2011).
14 See Shafer and Sonnenschein (1982) for a survey and Rizvi (1998, 2003, 2006) for more historical discussion.

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15 See Van Daal and Walker (1990) on the difference between Walras and Pareto on this matter.
16 My discussion of the tâtonnement of Walras (as opposed to the Walrasian tâtonnement) will focus primarily on his analysis of the pure exchange case. A detailed discussion of what Walras said about tâtonnement processes in general is not necessary for the task at hand. There were four (nested) models in the *Elements* – pure exchange, production, capital formation, and monetary theory – and the book went through five editions (counting the 4th definitive) and Walras offered different characterizations of the tâtonnement in different editions as well as for different models within various editions. In particular, the assumption of "no disequilibrium trading" or no trading at "false prices" was handled differently in various editions and models. The variation among editions is greatest in his analysis of production and capital formation, where his introduction of "tickets" ("*bons*") in the 4th edition provided a version of the "no trading at false prices" restriction for these models. Although there is some variation in his analysis of the pure exchange case, the core characterization offered in the 2nd edition remained basically intact in the later editions and that is the version of Walras's tâtonnement discussed here. Those interested in the details of how Walras's view of the tâtonnement changed across various editions and models can consult the various detailed discussions within the secondary literature (that is, Bridel and Huck 2002; Donzelli 2006, 2007; Jaffé 1967, 1980, 1981).
17 Uzawa (1960) noted that Walras's iterative process was a version of the Gauss–Seidel algorithm and proved that it converges under the assumption that all goods are gross substitutes.
18 Walras did not use the term "dynamic" for (any version of) his competitive price adjustment mechanism. For Walras, "dynamics" involved changes in the fundamentals of the analysis – tastes, technology, endowments, etc. – and the tâtonnement is not dynamic in this sense.
19 See Weintraub (1979, 1991) for more detail on these various economists (particularly Patinkin).
20 See Hands (2006, 2010a, 2011) for a more detailed discussion of, and evidence for, this argument and how it relates to various issues in the history of demand theory (integrability in particular) as well as to recent research in experimental and behavioral economics.
21 Consistent, that is, with respect to the symmetry of the behavior of the economic agent and the behavior of the competitive market. In other respects, Lucas's framework is much less consistent with Walras than Arrow and Hahn (1971) – his use of the representative agent being the most obvious inconsistency.
22 This was pointed out in Hands and Mirowski (1998, p. 366).
23 Symbolism follows (CCP), (ID) and (MD) above.
24 Note Friedman is only being quoted here to make the point about the Keynesian connection to the Slutsky equation and Friedman's criticism of it; it is not an endorsement of Friedman's interpretation of Marshall.
25 Consider for example recent developments in Walrasian general equilibrium theory such as the equilibrium manifold approach (e.g. Balasko 2009, Brown and Matzkin 1996, Brown and Shannon 2000, Chiappori and Ekeland 2004). See Rizvi (2006) for historical discussion of this literature.
26 See the various papers in De Vroey and Hoover (2004).
27 The literature here is quite extensive. See for example (Ingrao and Israel 1990; Kirman 1989, 2006; Rivzi 1998, 2003, 2006; Scarf 1981)
28 See the Hoover chapter in this volume for a detailed discussion of the microfoundations literature.
29 Think of the k_is in (T') as Lucas's "free parameters."
30 And will be negative definite if the numéraire row and column is eliminated as it normally would be for stability analysis.
31 See Arrow and Hahn (1971, ch. 11).

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32 Arrow and Hurwicz (1958).
33 As I indicated in note 11, I have discussed a number of other factors in previous research.

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