Microfoundational Programs*

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Abstract
of

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The substantial questions of macroeconomics itself are very old, going back to the origins of economics itself. But professional self-consciousness of the distinction between macroeconomics and microeconomics dates only to the 1930s. The distinction was drawn quite independently of Keynes, yet Keynes's *General Theory* led to its widespread adoption. The question of the relationship of microeconomics to macroeconomics encapsulated in the question of whether macroeconomics requires *microfoundations* was not raised for the first time in the 1960s or '70s, as is sometimes thought, but goes back to the very foundations of macroeconomics. There are in fact at least three microfoundational programs: a Marshallian program with its roots directly in Keynes's own theorizing in the *General Theory*; a fixed-price general-equilibrium theory, which includes some work of Patinkin, Clower, and Barro and Grossman; and the more recent representative-agent microfoundations, starting with Lucas and the new classicals in the early 1970s. This paper will document the development of each of these microfoundational programs and their interrelationship, especially in relationship to the programs of general-equilibrium theory and econometrics, whose modern incarnations both date from exactly the same period in the 1930s.

1. Three Programs

At least since the early 1980s with the ascent of the new classical macroeconomics, only macroeconomic models with explicit microfoundations have been regarded as fully acceptable.[1] Typical graduate textbooks – and, increasingly, undergraduate textbooks – open with dynamic optimization problems that are meant to connect the ordinary microeconomics of the consumer and firm to the behavior of aggregate data and to classic macroeconomic concerns such as the business cycle, growth, inflation, and interest rates (see *inter alia* Romer 1996, Blanchard and Fischer 1989, Barro 1984). How did microfoundations become the *sine qua non* of sound macroeconomics? There are many ways to tell this story – and, indeed, it has been told before. Here I will tell it from the perspective of the currently dominant practice. This is an exercise in economy rather than in Whig history. The story features neither triumph nor inevitable progress; rather it seeks to know why current practice is the way it is; and, as a result, it omits or minimizes alternative paths, including heterodox programs, such as post-Keynesian macroeconomics, and heterodox criticisms, such as those lodged by the Austrian school, as well as mainly pointing to certain aspects that are already well discussed elsewhere.[2]

Lucas's well-known article "Understanding Business Cycles" (1977) exemplified a widely accepted understanding of the emergence of modern microfoundations. In Lucas's telling, modern macroeconomics began with John Maynard Keynes's *General Theory of Employment, Interest, and Money* (1936). Keynes, according to Lucas, rejects a dynamic analysis of business cycles in favor of a static account of output determination

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[1] See Hoover (1988) for an account of the new classical macroeconomics and the role of microfoundations in it.
[2] See Harcourt 1977; Horowitz 2000; Weintraub 1977, 1979; Janssen 1993; Hartley 1997.

(pp. 215-216); he rejects equilibrium theory (p. 219); and individual optimization – at least in the labor market (p. 220). Keynes's theoretical strategy gives a boost to the nascent program of aggregative econometric modeling:

> The decision on the part of the most prestigious theorist of his day freed a generation of economists from the discipline imposed by equilibrium theory, and . . . this freedom was rapidly and fruitfully exploited by macroeconometricians. [p. 220]

Lucas is, of course, aware that many Keynesian economists did consider the microeconomic basis for various components of the Keynesian model – the consumption function, the money-demand function, the investment function, the Phillips curve, and, in Lucas's own work with Rapping, the labor-supply function – but these exercises fell short of incorporating the discipline of the optimization problem into the general-equilibrium framework (Lucas 1981, pp. 2-3; Lucas 2004, pp. 20-21; Lucas and Rapping 1969, 1970). The microeconomics of the various functions mainly served to suggest a list of regressors to "explain" their target variables. The regressions themselves were, in effect, merely rules of thumb – decisions rules for particular stable environments. Lucas (1977, pp. 220-221) did not deny that macroeconometric models constructed in this manner could well mimic the behavior of the actual economy, but appealing to the main theme of his famous article "Econometric Policy Evaluation: A Critique" (1976), he argued that the regressions would not isolate the invariants in the economy and that conditional forecasting (policy analysis) requires such invariants.

Lucas (1980, p. 286; 1987, p. 108) was willing to excuse the theoretical choices of Keynesian economists as the product of the exigencies of the Great Depression and the absence of appropriate tools. But economists after the development of the Arrow-Debreu contingent-claims framework can no longer be excused. New classical microfoundations

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begins with the optimization problems of individual agents. These are incorporated into dynamic general based in the contingent-claims framework. Dynamics in an uncertain world requires the formation of expectations. Rather than taking expectations to be exogenously given or based on arbitrary rules of thumb, the rational-expectations hypothesis assumes that self-interested agents will somehow find expectations that are consistent with a true model of the economic process. They may make expectational errors, but they will not make systematic errors. The rational-expectations hypothesis, because it incorporates – implicitly, at least – the whole model of the economy imposes consistency restrictions across the various equations. No part of the system is independent from the other parts.

In Lucas's account, modern microfoundations begins with the new classical revolution of the 1970s, the opening shot of which was the introduction of rational expectations into otherwise standard macromodels (e.g., Lucas 1972b; Sargent and Wallace 1975, 1976). While many economists find the perfectly competitive general-equilibrium model too perfect and seek to capture some features with a Keynesian flavor by introducing realistic barriers to smoothly functioning markets, even these new Keynesians accept the main lines of Lucas's story and support a nearly identical view of the nature and necessity of microfoundations (see, for example, Blanchard 2000). Call this common view the *mainstream narrative.*

There is – as there usually is in Whig histories – some truth to this story. But in telling a story of linear progress, the mainstream narrative misses a more complicated and more interesting story and misrepresents key elements. The microfoundations of macroeconomics was a problem long before the new classical revolution and long before

the term "microfoundations" was current. Indeed, Keynes himself had a distinct

approach to microfoundations. As I reconstruct the development of microfoundations, it

comprises a prehistory and three distinct microfoundational programs. One program,

which I associate with Lawrence Klein was mainly concerned with accessible data. The

data was aggregate, and Klein wanted to know that its behavior was compatible with the

economic behavior of individuals. Call this the *aggregation program*. A second

program was theoretical. Taking macroeconomics to describe (theoretically or

econometrically) robust features of the economy, it asked whether a fully disaggregated,

general-equilibrium model could generate those features as a characteristic of the normal

operation of the system. Call this the *general-equilibrium program*.

The aggregation and general-equilibrium programs seek *non-eliminative*

microfoundations. If they were perfectly successful, we would nonetheless continue to

use macroeconomics. In contrast, Lucas advocates *eliminative* microfoundations:

> If these developments succeed, the term "macroeconomics" will simply disappear
> from use, and the modifer "micro" will become superfluous. We will simply
> speak, as did Smith, Ricardo, Marshall, and Walras, of *economic* theory. [Lucas
> 1987, pp. 107-108][3]

For reasons that will become clearer presently, we shall call the currently dominant,

eliminative microfoundations the *representative-agent program*. I do not wish to argue

that the three programs are entirely separate. There are many connections between them.

Yet, a key thesis in this account is that the representative-agent program provides a

plausible account of microfoundations only by systematically ignoring important

elements of the prehistory and other microfoundational programs.

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[3] Smith and Ricardo, of course, spoke of *political economy*, not economics.

2. A Bibliographic Map of the Microfoundations Literature

The notion of a microfoundations for macroeconomics presupposes notions of both microeconomics and macroeconomics. Macroeconomic issues – for example, the relationship between money and the aggregate price level – are ancient and no less venerable than microeconomic issues. While he did not use the modern terminology, when Keynes distinguished between

> the theory of the individual industry or firm and of the rewards and the distribution between different uses of a *given* quantity of resources on the one hand, and the theory of output and employment *as a whole* on the other hand [Keynes 1936, p. 293]

he drew a recognizable micro/macro distinction. The introduction of the terms "microeconomics" and "macroeconomics" nonetheless made the distinction more palpable and easier to keep straight. These terms seemed to have been coined by Ragnar Frisch.

It is well known that Frisch (1933, p. 172) distinguished between *microdynamics* and *macrodynamics*, employing these terms in essentially the same sense as we now use "microeconomics" and "macroeconomics." The earliest usages of the modern terms to be found in JSTOR are due to Tinbergen (1936, p. 177) writing in *macroéconomique* in French, and to Fleming (1938, p. 333) writing the English term as "macro-economic."[4] It is likely that Tinbergen and Fleming were using terminology that was already use in oral exchanges, very likely in the early meetings of the Econometric Society (see Louçã 2007, pp. 35, 190ff). Frisch is probably the ultimate source. In a set of widely circulated, mimeographed lectures, Frisch (1933/34) uses the Norwegian adjectives

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[4] Tinbergen (1938, p. 10) also uses "macroeconomic."

mikroøkonomiske and *macroøkonomiske* in a senses synonymous with "microdynamic" and "macrodynamic."[5]

While the micro/macro distinction gained currency over time, usage developed surprisingly slowly. Figure 1 plots the articles in JSTOR that use some term in the microeconomics or macroeconomics family as a proportion of all articles published in 97 economics journals.[6] There are few uses before the end of World War II. Growth in the use of "microeconomics is fairly steady, finally stabilizing around 10 percent only in the 1990s. "Macroeconomics" shows a similar pattern, although its growth is faster and stablizes at something over 20 percent a few years later. I conjecture that the much higher usage of "macroeconomics" is essentially the result of microeconomics being regarded by many (as perhaps implied in Lucas's pleas for eliminative microfoundations cited above) to be what economics *really* is. There are many fields regarded as microeconomic; but given that these fields have independent names (industrial organization, consumer theory, labor economics, etc.) one need mention microeconomics mainly when one needs to draw a contrast with macroeconomics. But "macroeconomics" is the name of a field with few subdivisions that do not also employ its name – adjectivally, at least.

While the issue of the relationship of macroeconomics to microeconomics is simultaneous with the introduction of the terminology, given the slow diffusion of these terms, it is hardly surprising that "microfoundations" first appears more than twenty years

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[5] I thank Olav Bjerkholt for his information and a copy of the relevant parts of Frisch's lectures.
[6] Data gathered in May and June 2009. The two families of search terms are:
  macroeconomics family: "macroeconomic" or "macroeconomics" or "macro economic" or "macro economics" (the JSTOR search engine treat hyphens as blanks; so these terms cover both adjectival and nominal uses);
microeconomics: "microeconomic" or "microeconomics" or "micro economic" or "micro economics";

later. The earliest use recorded in the JSTOR archive is due to Sidney Weintraub (1956, p. 854) where he refers to "microeconomic foundations." A year later he entitles an article "The Micro-Foundations of Aggregate Demand and Supply," and he refers to "micro-economic foundations" in the text (Weintraub 1957, p. 455).

The diffusion of microfoundations is displayed in Figure 2, which plots the number of articles among 97 economics journals in the JSTOR archive that use a family of microfoundation terms as a proportion of all economics articles and as a proportion of all macroeconomics articles.[7] The apparent boom in microfoundations at the end of the 1950s (viewed against macroeconomics articles) is an artifact of small numbers. There are relatively few macroeconomics articles, and almost all of the microfoundational articles are simply citations of Weintraub's 1957 article and do not discuss microfoundations *per se*. The real boom appears in the early 1970s. It coincides with the first book to carry "microfoundations" in its title, *Microeconomic Foundations of Employment and Inflation Theory* (Phelps 1970). E. Roy Weintraub's survey article (1977) and Lakatosian history (1979) no doubt reinforced the boom, increasing self-consciousness about microfoundations among macroeconomists.

Depending on the base of comparison, the data look somewhat different: steady when viewed against all articles and declining until the 1990s when viewed against macroeconomics articles.   I conjecture that the difference in the behavior of these data after about 1976 reflects the naturalization of the microfoundational world view. Increasingly in macroeconomics articles, microfoundations is not discussed, but the

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[7] Data gathered in May and June 2009. The *microfoundational* family of search terms is: "microfoundation" or "microfoundations" of "micro foundation" or "micro foundations" of "microeconomic foundation" or "microeconomic foundations" or "micro economic foundation" or "micro economics foundations."

theory is developed consistently with the strictures of the microfoundational program, hence the fall when the base is macroeconomics articles; while explicit discussions of microfoundations remain a steady topic of special interest, hence the level path when the base is all articles.

To get a further handle on the filiation of microfoundational ideas, Table 1 displays the number of articles that use terms in the microfoundational family and various economists, some of whom are mentioned in the mainstream narrative in section 1 and others of whom will be introduced in due course. Total occurrences of single authors appears on the main diagonal; while co-occurrences appear in the off-diagonal cells. For the moment, the most significant points are the dominance individually, and in terms of co-occurrences, of Robert Lucas and Edmund Phelps. Lucas not only contributed to *Microeconomic Foundations*, which Phelps edited, he also credits Phelps's "island model" with providing the key to his own appreciation of the microfoundations of the labor market, extended to a wider macroeconomic framework in his "Expectations and the Neutrality of Money" (1972a; see also 1981, p. 7).

As well as tracking people, we can also track concepts. Table 2 shows the number of articles that use terms in the microfoundational family plus another term – or (sometimes) two – linked to pertinent concepts. Most of the concepts listed in Table 2 are evident from the mainstream narrative – others will be considered in due course. Consistent with Figure 2, most of the discussion of microfoundations occurs in the later period, although the patterns are qualitatively similar before and after 1970. The largest entries after 1970 refer to "expectations," "Keynes or Keynesians," "labor," and, substantially fewer, to "general equilibrium." These patterns reflect the close connection

of microfoundations to the new classical macroeconomics and the "rational-expectations revolution." They are consistent with the mainstream narrative in which the application of rational expectations to labor markets in the context of the Phillips curve (which is itself mentioned in 579 articles) was the opening gambit. Since the importance of the Lucas critique is a key element in the mainstream narrative, one surprise is the strikingly small number of articles mentioning terms in the Lucas-critique family in connection with microfoundations,. Nor does this reflect a paucity of references to the Lucas critique itself. Terms in the Lucas critique family are, in fact, mentioned in 506 articles after 1970 irrespective of whether the microfoundations family is itself mentioned.

3. The Prehistory of Microfoundations

While the term "microfoundations" did not achieve currency until well after the distinction between microeconomics and macroeconomics had become a key organizing element in the structure of the discipline, the relationship of macroeconomics to microeconomics was an issue from the beginning. Two of the three microfoundational programs that I will identify antedate "microfoundations." And there is a history of microfoundations that antedates any coherent programs.

FRISCH

In his article "Propagation and Impulse Problems," Frisch wrote:

> micro-dynamic analysis is an analysis by which we try to explain in some detail the behaviour of a certain section of the huge economic mechanism, taking for granted that certain general parameters are given . . . The essence of this type of analysis is to show the details of the evolution of a given specific market, the behaviour of a given type of consumers, and so on.
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> . . .

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. . . macrodynamic analysis, on the other hand, tries to give an account of the fluctuations of the whole economic system taken in its entirety. Obviously in this case it is impossible to carry through the analysis in great detail. Of course, it is always possible to give even a macro-dynamic analysis in detail if we confine ourselves to a purely formal theory . . . Such a theory, however, would only have a rather limited interest. In such a theory, it would be hardly possible to study such fundamental problems as the *exact time shape* of the solutions, the question of whether one group of phenomena is lagging behind or leading before another group, the question of whether one part of the system will oscillate with higher amplitudes than another part, and so on. But these latter problems are just the essential problems in business cycle analysis. In order to attack these problems on a macro-dynamic basis so as to explain the movement of the system taken in its entirety, we must deliberately disregard a considerable amount of the details of the picture. We may perhaps start by throwing all kinds of production into one variable, all consumption into another, and so on, imagining that the notions "production," "consumption," and so on, can be measured by some sort of total indices [Frisch 1933, pp. 172-173]

As with Keynes's analysis in the *General Theory*, which was being developed at the same time, the fundamental distinction that Frisch draws is between the operation of parts in isolation and the characteristics of "the whole economic system taken in its entirety." Macroeconomics is not identified as the economics of aggregates. It is pragmatic, not conceptual considerations, that warrant the use of aggregates ("some sort of total indices"). We must sacrifice details and stick to the bird's-eye view because detailed models would not be tractable and detailed data would not be available.

Despite appearing in a volume in honor of Gustav Cassel, Frisch's article does not advocate a general-equilibrium approach in the sense of model that stresses mutual dependence above all else. Dynamics – change rather than coordination – are his main concern. This is not to say that interdependence is ignored; for, indeed, it is precisely that interdependence, rather than the bird's-eye view of aggregation, that distinguishes macrodynamics from microdynamics. Frisch fleshes interdependence out, not as individual actors in an economy-wide auction as in Walras's vision of general

equilibrium, but as a "circulation in and out of certain sections of the system" or, as he puts it, as "*Le Tableau Économique*" (Frisch 1933, p. 173-175).

Frisch does not address the issue of microfoundations except to the degree that the relationship of macroeconomics to microeconomics is implicit in his definitions. We can see, nonetheless, that his vision is not one of the micro as more fundamental than the macro or the macro as simply a dispensable representation of the micro. Yes, the micro, the "sections" of "the economic system taken in its entirety" are constitutive; but the dynamics of the micro, where dynamics are the desiderata of Frisch's economics, are themselves the special case "taking for granted that certain general parameters are given." Any association of macroeconomics with aggregates is merely a byproduct of the limitations of our analytical capacity and of the data.

KEYNES

Keynes's did not adopt Frisch's coinages – "macroeconomics" or "macrodynamics." Yet in drawing a distinction between the theory of the individuals, firms, or industries, taking resources as given, and a theory of the determination of "output as a whole" – a phrase that recurs frequently throughout the *General Theory* – Keynes makes the same distinction as Frisch (Keynes 1936, pp. 26, 27, 40, 43, 281, 285, 294, 322). And Keynes shares Frisch's concern for dynamic economics. In his *Treatise on Money* (1930, pp. 120), Keynes introduced the idea of a monetary economy explicitly as part of a dynamic analysis, and he carries the idea into the *General Theory*, in which money and expectations about the future play a key part in real outcomes. Despite his modern reputation, promoted in large measure by the mainstream narrative, Keynes is generally

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explicit about microeconomics and its connection to the theory of output as a whole. Where Frisch had sacrificed microeconomic detail for explicit dynamics, Keynes draws macrodynamics somewhat impressionistically, while providing a wealth of microeconomic detail.

It is an underappreciated element of Keynes's approach in the *General Theory* that it respects the heterogeneity of individual agents to a degree rarely found in macroeconomics. In place of Frisch's *Tableau Économique*, Keynes introduces a set of accounting conventions, closely analogous to modern national accounts, which were first developed at roughly the same time by Colin Clark, Simon Kuznets, and Richard Stone. Since the accounts are measured in monetary terms the incomes, expenditures, and products of disparate workers, consumers, and firms can be added up unproblematically in a common unit. Naturally, behavioral relationships must in some sense be formulated in "real," rather than monetary, terms. Despite – or, perhaps, because of – a deep knowledge of index numbers, Keynes does not appeal to a price index as a deflator, but reëxpresses monetary quantities in wage units (the ratio of the monetary quantity to the typical wage rate for manual workers), in effect adopting a relative social standard for real value rather than deflating by the price of a basket of disparate goods with somewhat arbitrary weights (Keynes 1909; 1930, book II). (Keynes's measure of value is not far from Smith's or Malthus's "labor-commanded" standard of value.)

Whereas in practical cases Frisch adopts the coarser-grained bird's-eye view when discussing behavioral relationships, Keynes nearly always refers to individual actors and declines to bury their behavior in aggregates. In most cases, he accounts for individual behavior using the usual Marshallian tools of utility or profit maximization.

Keynes's account of the investment decision of an individual firm is, as he

acknowledges, Fisher's intertemporal analysis (Keynes 1936, ch. 11, esp. p. 140).

Fisher's "internal rate of return" is Keynes's "marginal efficiency of capital" – namely,

that rate of discount that makes the expected future profit stream of an investment equal

to its supply price. The decision to invest, then, is a matter of comparing the marginal

efficiency of capital to the available alternative investments and financial asset returns.

What is distinctively Keynesian, however, is that the marginal efficiency of

capital of the economy as a whole is not constructed by aggregating the investment

opportunities of disparate firms to construct an investment schedule in which the

aggregate of those projects that are just barely profitable at the market rate of interest

define the margin – a construction wrongly attributed to Keynes in some early

macroeconomics textbooks. Instead, for Keynes "[t]he greatest of these marginal

efficiencies [of individual projects] can be regarded as the marginal efficiency of capital

in general" (Keynes 1936, pp. 135-136; cf. LeRoy 1983). Here the investment project of

a *particular* firm is the marginal efficiency of capital for the economy as a whole.

Unlike the textbooks, Keynes is not describing a static equilibrium, but supplying

a causal account of the forces that drive the economy. Implicitly, Keynes is identifying

the causes of economic dynamics – a concern that had been more explicit in the *Treatise*

(Keynes 1930, p. 120; see also Hoover 2006). An individual firm's marginal efficiency

of capital can be identified with that of the economy because the individual firm is

embedded in a financial system that connects heterogeneous firms through the common

denominator of money.

The case of liquidity preference is similar. An individual must decide whether to hold money (clearly short, interest-bearing bills on Keynes's definitions) or (long) bonds. If one expects market interest rates to fall, then it is profitable to go long, and vice versa. The interesting point in this context is that Keynes does not construct an economy-wide liquidity-preference schedule by assuming that each individual has a well-defined, stable demand-for-money function and adding them up at each conjectured interest rate. Rather he envisages ranking individuals according to their subjective normal rate of interest – that is, the rate to which they refer when judging whether market rates will rise or fall. If asset holdings are stable, then the market rate of interest must be the rate at which there are sufficient people who believe rates will rise to hold the available stocks of money and sufficient people who believe rates will fall to hold the available stocks of bonds. Again, the heterogeneity of individuals is preserved. In fact, Keynes argues that the stability of financial markets and the efficacy of monetary policy depends on that heterogeneity (Keynes 1936, p. 196-199; also 158-161). And, again, liquidity preference for the economy as a whole is associated with the causal factors that drive itsdynamics rather than with the conditions of static equilibrium.

Keynes's analysis of the labor market and consumption present harder cases. Keynes's account of labor demand follows directly from the optimization problem for the firm – hire labor up to the point that the product real-wage equals the marginal product of labor (Keynes 1936, p. 5). The problem arises with his denial of the second "fundamental postulate" – that is, his claim that the real wage can exceed the marginal disutility of labor (Keynes 1936, pp. 5-6). The analysis of chapter 2 of the *General Theory* has puzzled critics (friendly, as well as hostile) from the beginning. Leontief

(1936), for example, was an early critic. He accused Keynes of violating the orthodox theory of economic choice (p. 94 ff.) and reminds him that monetary neutrality (homogeneity of degree one in prices) is not an axiom of that theory, but a theorem (p. 91). For his part, Keynes (1936, p. 9) was prepared to believe in money illusion in practice, but was at pains to deny that his system depended on it essentially.

I have argued at length elsewhere that the best interpretation of Keynes's labor market analysis is not that workers value a particular money wage; but that, in addition to valuing what their money can buy, they also value their relative economic position, which is indicated by relative wages (Hoover 1995).[8] Firms in this story understand the sources of workers' utility and, thus, incorporate relative wages into their production decision in the manner of modern efficiency wage models. Unlike some modern models, in which efficiency depends on the real wage, firms have a disincentive to reduce *money* wages, since any reduction – unless it is coordinated across the economy – will reduce efficiency. In such a model, the real wage can exceed the marginal disutility of labor without violating homogeneity.

As with Keynes's analysis of investment and liquidity preference, this analysis of the labor market depends essentially on the heterogeneity of workers. And it explains what many have found to be a puzzling feature of Keynes's analysis of unemployment. Despite their heterogeneity, Keynes might have aggregated individual labor supply schedules to produce a labor-supply curve, relating some aggregate wage rate to the total labor forthcoming in the economy. Full employment would then be – as it often is in textbooks – the intersection of the labor supply and demand curves, and involuntary

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[8] "the struggle for money-wages is . . . essentially a struggle to maintain a high *relative* wage . . ." (Keynes 1936, p. 252).

unemployment would then be defined as occurring when the market wage is higher than

the equilibrium wage. But Keynes does not define unemployment in that manner.

Instead, he writes:

Men are involuntarily unemployed if, in the event of a small rise in the price of wage-goods relatively to the money-wage, both the aggregate supply of labour willing to work for the current money-wage and the aggregate demand for it at that wage would be greater than the existing volume of employment. [Keynes 1936, p. 36]

What Keynes has done is to propose a thought experiment that defines the situation in

which men are involuntarily unemployed. Why?

Despite references to "the current money-wage," this test does not require a single

money-wage or any aggregation of wage rates. Rather, whatever the actual structure of

money wages, any rise in the price of any good purchased by workers will decrease their

real purchasing power while increasing the demand for labor on the part of firms. Any

situation in which those reactions can result in higher employment is a situation of

unemployment. Keynes has once again appealed to notions that do not require collapsing

the heterogeneity of economic actors into aggregates and has singled out a specific

feature (here, the price of a wage-good) that is relevant both to individual decisions and

to a systemic characteristic – unemployment.

The case of the consumption function appears at first to be somewhat different,

since Keynes does not present a maximizing account of consumption choice, but the

famous "fundamental psychological law" (Keynes 1936, p. 96). This too may reflect

Keynes's appreciation of heterogeneity in that an intertemporal optimizing choice of

individual consumption patterns would require complete disaggregation of consumption

goods both in the current and future period in the manner of the later Arrow-Debreu

contingent-claims framework. It is not just that solving such a problem is formally

(much less practically) difficult for the economist; it does not seem to be something that individual agents could approximate in their own behavior. The alternative is to aggregate and optimize over time in the manner of Keynes's younger colleague Frank Ramsey (1928). But that would not show the respect for heterogeneity implicit throughout Keynes's theory. Instead, Keynes sees the consumption decision as two-stage choice: divide current resources into those to be consumed now and in the future (saving); then allocate current consumption expenditure over particular goods in the usual utility maximizing manner. Seen this way, consumption expenditure for the economy as a whole is just the sum of individual consumption expenditures.

Keynes does not give up altogether on individual optimization. Much of chapters 8 and 9 of the *General Theory* concerns factors that might affect the propensity to consume, putting to rest the common textbook notion that Keynes's consumption is the simple linear function of current income alone. The factors include changes in real purchasing power (measured in wage-units), as well as windfall capital gains and losses, changes in rates of time-discounting, and expected future income – factors anticipating the considerations of the later permanent-income/life-cycle hypothesis.

The multiplier, whose value depends on the marginal propensity to consume, is also a reflection of heterogeneity. It is another system characteristic that transcends the individual optimization problem and takes as it is background an economy in which differentiated agents engage in trade.

Taken together, Keynes's analysis of the fundamental components of aggregate demand and supply display a firm connection between microeconomic choices of firms and individuals and the macroeconomic outcomes. In every case, the causal mechanisms

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are driven by individual agents. The values of variables salient for the macroeconomic outcomes are traceable directly to individuals. Where aggregation is necessary (for example, in the analysis of consumption and the multiplier), it takes form of the addition of homogeneous monetary values rather than relying on the arbitrary weighting of a price index. The outcomes for the economy as a whole clearly *emerge* out of individual behaviors. The characteristics of the system – for example, that output is not given, as it is assumed to be for individual allocations – are distinct from the characteristics of individual markets and individual optimization, but are not disconnected or mysterious. *Emergence* is perhaps the most characteristic feature of Keynes's account of the relationship of microeconomic to macroeconomic behavior.

4. Hicks and the General-equilibrium Program

To a large extent, Hicks, as much as Keynes, belongs to the prehistory of microfoundations. The transition between Hicks and the first of the three microfoundational programs that I wish to consider is so seamless that it makes sense to consider them together. What I shall say about Hicks's own microfoundations and indeed much of the general-equilibrium program of microfoundations is schematic, omitting most of the details. Weintraub (1979) provides an excellent history, and there is no need to repeat it.

Hicks was deeply engaged in writing his masterwork, *Value and Capital* (1939/1946) when Keynes's *General Theory* first appeared. As Weintraub (1979, p. 55) observes, it can be seen as an attempt to construct a Walrasian macromodel. The main lines of Hicks's approach are familiar. He begins with individual optimization: a

restatement of the theory of subjective value, drawing heavily on Pareto and the device of

indifference curves; and an analogous restatement of the theory of firm. He situates the

individuals in a Walrasian general equilibrium. He sees Keynesian problems as arising in

a dynamic framework. Frisch noted

> it is always possible by a suitable system of subscripts and superscripts, etc., to
> introduce practically all factors which we may imagine: all individual
> commodities, all individual entrepreneurs, all individual consumers, etc . . . [Frisch,
> 1933, p. 172].

Whereas Frisch noted a possibility, Hicks – mainly verbally, to be sure – sketched out the

detailed formal theory, moving beyond a Walrasian static, general equilibrium to a

general equilibrium in which time is broken into distinct periods, commodities are dated,

and not only do decisions today affect decisions tomorrow, but expectations of the future

affect decisions today.[9]

Hicks's vision (as well-described by Weintraub) does not find a gulf between

microeconomics and macroeconomics. Rather the characteristically macroeconomic

features that Keynes had emphasized – the unemployment of labor and capital as a result

of deficient aggregate demand, the non-neutrality of money, and the efficacy of monetary

and fiscal policies – arose because the Walrasian model of perfect coordination was not

an adequate model of the world. A dynamic model – one in which expectations,

incomplete markets, and adjustment processes were central features – could capture the

main Keynesian insights. The dynamic model was based in individual optimization and,

thus, was completely compatible with microeconomics.

Like Frisch, Hicks connected macroeconomics to dynamics. But Frisch thought

that detailed macrodynamics – a macrodynamics with explicit representation of all agents

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[9] Hicks (1939/1946, pp. 2-3, sees his approach as that of Walras modified by Marshall.

– was simply too hard to implement in practice. He thus made the pragmatic decision to

reformulate macrodynamics in terms aggregates. Hicks reacted in what seems at first

blush a more principled manner. Hicks's

> method of analysis . . . enables us to pass over, with scarcely any transition, from
> the little problems involved in detailed study of the behaviour of a single firm, or
> single individual, to the great issues of the prosperity or adversity, even life or
> death, of a whole economic system. The transition is made by using the simple
> principle, already familiar to us in statics, that the behaviour of a group of
> individuals, or group of firms, obeys the same laws as the behaviour of a single
> unit. . . . The laws of market behaviour, which we have elaborated for those
> tenuous creatures, the representative individual and the representative firm, thus
> become revealed "in their own dimensions like themselves" as laws of the
> behaviour of great groups of economic units, from which we can readily evolve the
> laws of their interconnexions, the laws of the behaviour of prices, the laws of the
> working of the whole system. [Hicks 1939/1946, p. 245]

Hicks's theoretical rationale for the assumption that what is true of the individual is true

of the group – at least in part, a denial of Keynes's emergent properties of the economy as

a whole – is found in what would latter be referred to as his "composite-commodity

theorem,"

> the very important principle, used extensively in the text, that if the prices of a
> group of goods change in the same proportion, that group of goods behaves just as
> if it were a single commodity. [Hicks 1939/1945, pp. 312-313; cf. pp. 33-34]

On the one hand, the composite-commodity theorem provides a set of conditions

under which the aggregate can be treated as an individual. On the other hand, it says

nothing about how likely those conditions are to be found – even approximately – in real

world cases. Hicks does not address the applicability of the theorem to the real world.

Subsequent developments in aggregation theory, however, suggest that its range of

applicability is exceedingly narrow. Gorman (1953) showed that an aggregate utility

function could take the same form as individual utility functions only in the case of

identical, homothetic preferences or, equivalently, when Engle curves are linear and

parallel. A series of results, sometimes referred to as the "Sonnenschein-Debreu-Mantel" result shows that under a range of conditions market excess-demand functions exist, but their shapes do not properties imposed by the axioms of rational choice on individual supply and demand functions (see Kirman 1992 for an exposition). Individual relations may give rise to definite aggregate relations, but aggregate relations do not bear any simple analogical relationship to individual ones. Subsequent microeconomic theory itself undermines Hicks's optimistic appeal to the composite-commodity theorem.

One reaction to these results is to acknowledge that aggregation is too difficult and to stick to formal general-equilibrium models in which all agents are named as individuals. Another reaction is to interpret the composite-commodity theorem to provide an existence result: there is some way to formulate a model much simpler than a disaggregated general-equilibrium model that can be used to explore various aspects of the economy in which aggregation itself is not the key feature.

The literature that forms the core of Weintraub's (1979) history of microfoundations takes the first path. His is a history of the general-equilibrium microfoundational program, which is conceived as showing how Keynesian problems can arise directly from the interactions of individual agents. The presuppositions of this path explain Weintraub's contention that the Arrow-Debreu-McKenzie model does not provide microfoundations, if by that we mean "a bridge between two distinct bodies of knowledge"; rather in one variant or other, it encompasses both microeconomics, identified with successful coordination, and macroeconomics, identified with coordination failure (Weintraub 1979, pp. 71, 75).

The two paths are not always kept separate. Patinkin's *Money, Interest, and Prices* (1956/1965) is structurally analogously to Hicks's *Value and Capital* in that it starts with individual agents, incorporates them into a general-equilibrium model, and then appeals to the composite-commodity theorem to justify attention to highly simplified systems when addressing Keynesian problems. In chapter 13, Patinkin explores the systemic implications of a failure of the labor market to achieve the Walrasian equilibrium, setting the stage for both the investigation of Walrasian disequilibrium theory, eventually leading to the investigation of non-*tâtonnement* processes and more aggregative general disequilibrium models of the 1970s and 1980s.

In a series of papers, Backhouse and Boianovsky (2005a, b, c) document the rise and disappearance of the aggregative general disequilibrium approach to microfoundations and its relationship to Patinkin's chapter 13. Robert Clower (1965) provided the seminal contribution with the observation that such essentially Keynesian mechanisms as the consumption function cannot arise in a Walrasian general-equilibrium model. The supply and demand schedules generated in such models are all notional, in the sense that they ask, what would an agent wish to do if he could buy or sell as much as he likes at a particular set of prices. Contrary to the Keynesian consumption function, an agent's spending decision is not conditioned on income in the Walrasian model, since income is not one of things, such as the price vector, given in the thought experiment; it is instead an endogenous outcome of decisions conditioned on prices. The Walrasian model assumes that prices are coordinated *deus ex machina* with an "auctioneer" or "recontracting" serving as the avatars of the god in the machine. In a world without such a mechanism, Clower observes, agents will respond to price signals in ways that will not

necessarily clear markets. The excess supplies and demands that they face provide additional constraints to their optimization problem. Thus, the expenditure of a worker who cannot sell as much labor as he notionally wishes to do will be constrained by the income that he can actually raise, giving rise to a consumption function.

While Clower's account is largely confined to the individual, he sees implications for the economy as a whole. These are worked out informally in Leijonhufvud's (1968) reinterpretation of Keynes's *General Theory*. What is most interesting in this context is the Barro and Grossman's "A General Disequilibrium Model of Income and Employment" (1971) and Malivaud's *Theory of Unemployment Reconsidered* (1977) which popularized the non-Walrasian models as aggregative, general disequilibrium models with representative agents.[10] While not explicitly attending to the issue of aggregation, these models follow the second path from Hicks's composite-commodity theorem to its natural conclusion.

One path from Hicks essentially became the domain of economic theorists – generally regarded as microeconomists, even when they addressed coordination failures – and hardly affected mainstream macroeconomics. The second path ended in a historical dead end – general disequilibrium (or fixed-price) models with representative agents. Backhouse and Boianovsky (2005a, b) speculate that one reason for the disappearance of general disequilibrium microfoundations was that Barro became convinced that price stickiness (for example, from contracts) could be regarded as an optimal, *equilibrium* outcome of a sufficiently complex Walrasian model. The economy was thus Walrasian at

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[10] Whether these should be seen as "disequilibrium" models or as "equilibrium" models with additional constraints is a semantic question that appeared from time to time in the literature, but which need not detain us.

the core and was better represented, even when using simplified models, by market-clearing, general-equilibrium models.

While there may be a good deal of truth to this story as a part of Barro's biography, I want to suggest that a more important reason is found in genealogy of the general-disequilibrium models as the product of the second path from Hicks's composite-commodity theorem: they are special theoretical cases, toy models built to display certain principles, but not models that have any claim on the real world. They were thus a poor basis for econometrics. It will be easier to appreciate this claim after we have consider the second microfoundational program.

5. Klein and the Microfoundations of Macroeconometrics

Post-World War II macroeconometrics derives in nearly equal measure from Tinbergen's pioneering models of the Dutch and U.S. economies and from Keynes's *General Theory* as seen through the lens of the aggregative formalizations of Hicks's (1937/1946) and Modigliani (1944).[11] Although Tinbergen and Keynes make strange bedfellows, given Keynes's (1939) hostile reception review of Tinbergen's book on econometric modeling, Lawrence Klein in his *Keynesian Revolution* (1947) and subsequent efforts at applied macroeconometric models was able to forge a common program out of contradictory roots. Since Kleinian macroeconometrics is the *bête noir* of the mainstream narrative, it is perhaps surprising that Klein lays out a consistent microfoundational program:

> A problem which has never been adequately considered by Keynesians is the derivation of a theory in terms of communities of individuals and groups of commodities from a basic theory in terms of individuals and single commodities. In modern economic terminology this is the problem of passing from micro to macro economics, i.e., aggregation. [Klein 1947, p. 56]

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[11] On Tinbergen's econometrics, see Morgan (1990, ch. 4).

While Klein's aggregation program is quite different from the general-equilibrium program, deriving from Hicks, it starts in much the same place. Klein (1947, p. 57) begins with individual agents solving optimization problems, ideally situated in a Walrasian equilibrium. (Formal derivations of each of the key Keynesian functions are provided in an appendix.) Klein, like Frisch, sees a detailed general-equilibrium account as impractical; for the purposes of econometric modeling, individuals have to be replaced by aggregates. But unlike Frisch, he is not willing simply to take his aggregates off the shelf uncritically; and, unlike Hicks, he is not willing to restrict his concerns to special cases suitable for theory. Klein argues by analogy that macroeconomic functions share the properties of microeconomic functions, provided one has used the "appropriate aggregation methods" (Klein 1947, p. 58).

But what are the appropriate methods? Klein explored this question in detail in two papers published in *Econometrica* in 1946. Klein is not content with the special case of Hicks's composite-commodity theorem, as prices will not stay constant in a dynamic economy with significant income effects – the distribution of income matters. Klein, who contrary to our interpretation in section 2, sees Keynes's theory as a theory of the relationship of aggregates, criticizes Keynes for his unjustifiable assumption that aggregate schedules are stable without accounting for distributional factors (Klein 1947, p. 57).

Two approaches to aggregation were current in the 1946 discussion. First, one could try to work out the implications of individual behavior for the existing price indices and other aggregates in the national accounts, which are generally weighted sums of individual data. Klein sees this as a nearly impossible task. May (1946) and Pu (1946)

proposed aggregation schemes of this form that worked on the condition that all

equilibrium conditions were fulfilled (see Janssen 1993, ch. 5 for further discussion).

Klein (1946b, p. 303) rejected their approach on the grounds that it was wrong to

presuppose equilibrium and that an aggregate had to be suitable in or out of equilibrium.

Klein argued that a more fruitful approach was to take both microeconomic theory

and Keynesian economic theory as given and to work out aggregates that made them

compatible with each other. The criterion of compatibility was that analogous marginal

conditions held at both the individual and the aggregate levels (Klein 1946a, p. 94). The

criterion is fulfilled if the conditions of Hicks's composite-commodity theorem hold, but

only then (p. 95). Common index numbers fail to fulfill the criterion (pp. 100-102).

Klein's aggregates would look very different from the atheoretical aggregates: "we

cannot know in advance the form of the aggregates but must accept those forms which

satisfy a mathematical requirement." (Klein 1946b, p. 311).[12] In particular, the

appropriate aggregates would not be weighted sums. As Klein remarks: "[t]here is no

reason to assume . . . that there is something sacred about a sum" (Klein 1946b, p. 310).

The ultimate justification for an aggregation scheme (or any scientific theory) is not that

it falls into familiar patterns, but that it yields useful results that contribute to human

happiness.

In practice, Klein's strategy has proved to be overoptimistic, since the

Sonnenschein-Debreu-Mantel theorem demonstrates that there is no guarantee, as Klein

supposes, that aggregates exist such that maximizing for a community function yields the

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[12] Nelson (1984) discusses these aggregation strategies from the point of view of methodology or
philosophy of science. A more recent, though theoretical example, is provided by equation defining the
aggregate price level in Blanchard and Fischer (1989, p. 376**)** which is an aggregate price level related to
underlying prices set by monopolistically competitive firms according to a highly nonlinear function
nothing like the typical Laspeyres or Paasch indices. See Hoover (forthcoming) for a discussion.

same results as maximizing disaggregated individual functions, even if aggregates are

"properly measured" (Klein 1947, p. 199; 1946a, p. 93). The theorem lay in the future,

and Klein was not to know. As an econometrician, he would, as others did, model the

economy using published aggregate data. The real force of his approach to

microfoundations was the intellectual reassurance that it offered:

> If we want to simplify mathematical models of general equilibrium into a small
> number of equations, it is useful to know that operationally significant concepts
> exist which justify such simplifications. It is only in models of macroeconomics
> that we can see through all the complex interrelationships of the economy in order
> to form intelligent judgments about such important magnitudes as aggregate
> employment, output, consumption, and investment. [Klein 1946a, p. 108]

Beyond the theoretical solace of believing that appropriate aggregation was

feasible in principle, Klein also appeals to microeconometrics, to budget studies that

show that distribution effects are small and might be neglected without much loss (Klein

1947, pp. 59, 194). He also argues that in practice ordinary published aggregates will

correlate so highly with properly computed aggregates "that one set can be substituted for

the other" (Klein 1946b, p. 311).

Empirical practice is paramount in Klein's view, and his approach to

microfoundations does not suppose that we build up from secure microdata, but that we

start with the available data and a feasible macroeconomic model and constantly work to

disaggregate it and to elaborate it in the direction of a complete Walrasian general-

equilibrium model. This is the strategy behind the program that developed through

various simpler models (Klein 1950; Klein and Goldberger 1955) and ultimately into the

famous Brookings macroeconometric model of the U.S. economy with its more than 150

equations (Duesenberry *et al.* 1965). Whether explicitly engaged in macromodeling or

not, much of the macroeconomics from the 1950s to the 1970s can be seen as the

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implementation of Klein's microfoundational program. Each of the Keynesian functions was analyzed at a microeconomic level and its implications for a feasible macroeconometric model considered.

We could easily examine investment, demand for money, labor demand or supply, or the Phillips curve in the same spirit, but let us instead consider two analyses of the consumption function – the relative-income hypothesis of James Duesenberry (1949) and the permanent-income hypothesis of Milton Friedman (1957). Duesenberry was later a collaborator with Klein on the Brookings Model; while Friedman was skeptical of this type of modeling. But both Duesenberry and Friedman represent methodologically similar efforts to ground macroeconomic relationships in microeconomic relations. That they differ on other methodological issues illustrates how widespread their common approach to microfoundations was.

Both studies start with the observation, due to Simon Kuznets, that simple versions of Keynes's consumption function are difficult to reconcile with empirical facts about consumption (Duesenberry 1949, pp. 1-2; Friedman 1957, pp. 3-6). A linear version of the consumption function in which the average propensity to consume exceeds the marginal propensity to consume predicts that the average propensity to consume will fall over time and that it will be lower for richer than for poorer people. Kuznets found that the average propensity to consume in aggregate was higher than the marginal for short spans but was constant over longer periods; while in budget studies the average propensity to consume is higher for individuals with lower incomes.

To reconcile these apparently contradictory findings, Duesenberry proposed that relative socioeconomic position both through time and across individuals matters. If

income is steady for any individual, more or less independently of its level, the average propensity to consume will be constant. But there is habit persistence induced by treating current income as a reference point. If income falls, the first response will be to maintain socioeconomic status by maintaining the familiar consumption pattern: consumption falls less quickly than income. If income rises, the first response will again be to continue in familiar patterns: consumption rises less quickly than income. An enduring increase in income, however, raises the reference point and the average propensity to consume readjusts to its typical rate. A similar thing happens – perhaps more slowly – with an enduring fall in income.

The budget studies are explained by the fact that they are point-in-time studies. The value of consumption to an individual depends, in part, on "keeping up with the Jones." Poorer people have to have a higher average propensity to consume to maintain comparability with richer neighbors. But "a rising tide lifts all boats"; so that an increase in aggregate incomes that leave income distribution relatively stable raises consumption proportionately (Duesenberry 1949, ch. 3).

Duesenberry's analysis is predicated on the assumed consistency of microeconomics and macroeconomics. Kuznets's facts themselves combined the microeconometrics of budget studies with the macroeconometrics of time series. Duesenberry grounds his analysis in ordinary microeconomic consumer theory with the added hypothesis that preferences are interdependent.

Friedman's permanent-income hypothesis explains the same set of facts without the assumption of interdependent preferences. Like Duesenberry, Friedman supposes that the "long-run" average propensity to consume is constant. He explains variations

around it by drawing a distinction between permanent income – essentially the income flow from the annuitized value of one's expected human and nonhuman wealth – and temporary windfall deviations between measured and permanent income. The permanent-temporary distinction involves both the idea that even predictable variations in income will be smoothed (wealth not income is the spending constraint) and that the idea that consumers must form expectations of uncertain future income flows to estimate wealth.

In budget studies, the poor (as measured by current income) show a higher measured average propensity to consume because some are only transiently poor because of windfall losses of income. Since such a loss has only a small effect on wealth (roughly the interest rate times the loss), their permanent income is hardly impaired and they maintain the higher level of consumption that it justifies. Symmetrically, some of the rich are only transiently rich and base their consumption on their lower permanent income. These transients raise the average propensity to consume of poorer groups and lower the average propensity to consume of the richer groups.

The time-series data is explained by the fact that over any shorter period of years temporary shocks to income may be dominantly positive or negative, shifting people on average above or below their permanent income and raising or lowering the average propensity to consume in exactly the same way as in the cross-sectional account; while over a longer run of years, positive and negative temporary shocks will average out, so that measured income and permanent income nearly coincide.

On Duesenberry's account, the short-run consumption function is the real result of a more complex socioeconomic process than contemplated by textbook consumer theory,

but just as grounded in the canons of microeconomic rationality. On Friedman's account, the short-run consumption function is a statistical illusion – an epiphenomenon.

As with Duesenberry's analysis, Friedman bases his hypothesis squarely in microeconomic, utility-maximizing consumer theory. Friedman argues for the superiority of his approach over Duesenberry's mainly on the basis of the microeconomic budget studies (Friedman 1957, ch. 6). Neither provides a direct derivation of the aggregate relationship from the individual relationships. Microfoundations in this program is not a matter of deductive certainty but of apparent consistency. Anticipating the Lucas critique, Duesenberry (1949, p. 72) sees the object to find fundamental invariants. In the manner of Klein, he argues that, in principle, invariant relationships at the individual level deductively imply invariant relations at the aggregate level, though the aggregates relations are not necessarily simple sums of the individuals ones. But in practice it comes down to empirical facts.

Neither Duesenberry nor Friedman see the adaptation of hypotheses to facts as undermining the authority of microeconomic theory. Duesenberry (1949, p. 13), for instance, argues that utility theory has an empirical basis, and it is empirical observation that warrants the notion of interdependent preferences. Friedman's notion of wealth follows from general principles of intertemporal optimization; whereas the constancy of the ratio of consumption to permanent income is a contingent fact, consistent with, but not implied by, consumer theory. Without the microtheoretical analysis it would be impossible to conceive of the notion of permanent income or to propose its measurement or to test the hypothesis of the constant permanent average propensity to consume.

The role of microeconomics in supporting the various Keynesian aggregate

functions has been stigmatized as merely suggesting the variables to be included in a

regression. The two analyses of the consumption function demonstrate suggest

otherwise. Duesenberry's microanalysis suggests a functional form that includes a

ratchet effect – the contextual calibration of the reference level of income. And while

Friedman's microanalysis does suggest a new variable, permanent income, this variable

is not an observable; and the theory suggests the strategy for its construction. What is

more, the theory explains the nature of the relationship between current income and

consumption while, at the same time, giving a reason to regard it as nonstructural. The

relationship between microeconomics and macroeconomics in these two cases is not one

of logical implication, but the microecononomics nonetheless does serious work to

support the macroeconomics.

The commitment to microeconomics is not merely theoretical – a well formed

aggregate function is consistent with microtheory – but empirical. Duesenberry argues

for microeconometric testing:

> we ought to operate on the following principles. First, every hypothesis ought to be
> stated in terms of the behavior of individual firms or households, even when we are
> only interested in the aggregate results. . . . Second, in so far as it is possible, we
> ought to test our hypotheses against data which indicate the behavior of individual
> households or firms. [Duesenberry 1949, p. 75]

Elaborating and testing the microeconomic basis for the consumption function

and the other Keynesian functions is an essential part of Klein's top-down

microfoundational approach. The target – though it may never be reached – is the

completely disaggregated model:

> In contrast with the parsimonious view of natural simplicity, I believe that
> economic life is enormously complicated and that the successful model will try to
> build in as much of the complicated interrelationships as possible. That is why I

want to work with large econometric models and a great deal of computer power. Instead of the rule of parsimony, I prefer the following rule: the largest possible system that can be managed and that can explain the main economic magnitudes as well as the parsimonious system is the better system to develop and use. [Klein 1992, p. 184][13]

The Brookings model is the most perfect expression of Klein's approach. In its

1965 version the model includes thirty-two sectors and more than 150 equations

(Duesenberry *et al.* 1965). The modelers, however, understood the tradeoff implicit in

real-world data. On the one hand, aggregation leads to imprecision; on the other hand,

data are sparse and disaggregated data have low signal-to-noise ratios (Duesenberry and

Klein 1965, p. 7-8) The tradeoff and the problems of tractability imply that modelers

must start with highly aggregated systems and disaggregate them wherever, and to the

degree that, it is feasible to do so. Consumption, for instance, is broken down into five

categories; firms into five categories; investment in four industrial groups (Duesenberry

and Klein 1965, pp. 7-8; Suits and Sparks 1965, p. 203; Jorgenson 1965, p. 56). The

microeconomics is never far from mind. For example, just as in the case of the

consumption function, Jorgenson (1965, p. 40) maintains that a major failing of earlier

aggregative analyses of investment was a failure to apply microeconomic theory to

determine not only which factors were important but *how* those factors should be

functionally related.

Since the mainstream narrative stigmatizes large-scale macroeconometric models

as unidentified, nonstructural reduced forms that substitute correlation for causation, it is

noteworthy that the Brookings team gives significant attention to structural modeling,

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[13] It is worth noting that, although we have identified Friedman as fellow traveler with Duesenberry in seeking the microeconomic basis of individual Keynesian functions, Friedman dissented strongly and early from Klein's complexity-is-best notion and never supported large-scale macroeconometric modeling (Friedman 1940, 1951; see also Hammond 1996, pp. 67-68).

appropriate estimation methods, and to the different information that might be extracted from structural and reduced-form models (Fisher 1965). In this, they continue a line of thought that was already clear in the work of the Cowles Commission (Koopmans 1950; Hood and Koopmans 1953) and related to microfoundations in Duesenberry's work on the consumption function.

In trying to understand the microfoundational elements in Klein's macroeconometric enterprise, we are not offering a defense of its substantive achievements. But we do take note, first, of the fact that macroeconometric models of the same genre persist in government and private policy analysis today; and, second, that contrary to the mainstream narrative, such models were never entirely macroeconomic, but were situated by their creators in a clear microfoundational program.

6. The New Classical Macroeconomics and the Representative Agent

Despite the persistence of practical macroeconometric models in the tradition of Klein, the aggregation program of microfoundations has been eclipsed since the early 1980s by the representative-agent program, introduced by new classical economists but now the common property of all mainstream macroeconomists, new Keynesian as well as new classical. So, complete has been the victory of representative-agent program that few economists of a recent vintage have ever been instructed in the elements of the aggregation program – the IS-LM model or the microfoundations of the individual Keynesian functions. Microfoundations as a concept, in the minds of many, has come to exclude the earlier programs altogether. The general-equilibrium program of microfoundations has come to be seen as a respectable area of research, but one that is

essential *microeconomic*; while the aggregation program of microfoundations is falsely

characterized as an analysis of aggregates without any substantial connection to

microeconomics. Figure 2 and Tables 1 and 2 quantify the narrowing of the association

of microfoundations with the representative-agent program, showing how little the term

was employed before the advent of the new classical macroeconomics, how much it has

grown since, and the close relationship between discussions of microfoundations and the

particular issues emphasized by new classical economists.

      The publication of *Microeconomic Foundations* (1970), edited by Phelps, was the

watershed event in the establishment of the representative-agent microfoundational

program. There is an irony in this in that the sensibility that informed Phelps project and

many of the contributions to the volume was more the role of search, which involves

many agents, and heterogeneous information than it was the representative agent. But

Lucas and Rapping (1970) contributed a key paper, which has strong claims to be the first

paper in the new classical macroeconomics and the fount of the representative-agent

program. Most references to microfoundations in JSTOR in the years immediately

following its publication are to the Phelps volume. As Table 1 shows Phelps is

referenced as much in the context of microfoundations as Lucas.[14] The detailed pattern is

different, as shown in Table 3. Where Phelps is more frequently associated with

microfoundations than Lucas in the 1970s, references to Lucas reach parity in the 1980s,

and references to Phelps fall sharply behind in the 1990s.

      The premise of the Phelps volume was that the Keynesian account of the labor

market was inconsistent with standard neoclassical microeconomics. Real effects of

aggregate demand shifts in the *General Theory* depend on "esoteric non-neutrality"

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[14] References to Phelps are often not personal but to papers by other authors in the volume.

(Phelps 1970b, p 1; also Phelps and Winter 1970, p. 310). While this is an interpretation of Keynes's labor market that we rejected in section 3 above, it is probably the most common interpretation among economists since the *General Theory* was first published. In an important sense, the Phelps volume was conceived in much the same spirit as the microeconomic analyses of other Keynesian functions that dominated the aggregation program.

Lucas and Rapping's contribution was no different. Its central premise is that Keynes gave up too soon on perfect competition in the labor market (Lucas and Rapping 1970; also 1969). Lucas and Rapping generated real effects of demand shocks through expectational errors of the sort that Friedman (1968) and Phelps (1967) had made the centerpiece of their reconstructions of the Phillips curve. To this, Lucas and Rapping introduced intertemporal optimization – the intertemporal elasticity of substitution being the key factor governing the response to transitory shocks. The paper is typical of the earlier aggregative literature in that the "simple theory of a single household *suggests* an aggregate supply function" (Lucas and Rapping 1970, p. 265, emphasis added). The empirical investigation using aggregate data is not tightly connected to the microeconomic optimization problem.

Although Lucas and Rapping contributed to Phelps's volume, Phelps's direct influence on Lucas derived from what Lucas had learned at the conference of which the volume is a record. In one of his most famous papers, "Expectations and the Neutrality of Money," Lucas (1972a) moves beyond the single-agent optimization of Lucas and Rapping's articles and creates a model of many agents in general equilibrium. This is not a model in the spirit of Keynesian heterogeneity; all agents are fundamentally alike.

What differs is not the agents themselves, but the information available to them. Lucas appeals to Phelps's "island model" – agents are informationally isolated so that they know local information but can learn aggregate information only with a delay as travel between "islands" is not instantaneous (Phelps 1970b, p. 6). While Lucas assumes that information is incomplete, he also assumes that agents use it as efficiently as possible, adopting Muth's (1961) "rational-expectations hypothesis." The upshot of Lucas's model is that there is no genuine tradeoff between inflation and real output or employment implicit on common interpretations of the Phillips curve: money is neutral in the long-run and the short-run; the apparent short-run Phillips curve is a statistical illusion, positive demand shocks are associated with signal-extraction errors, so that they are correlated with positive movements of real output; but it is impossible to move systematically along the curve that traces out this correlation. Lucas's strategy is similar to Friedman's strategy in analyzing the consumption function – in their views, both the current-income version of the short-run Keynesian consumption function and the short-run Phillips curve were epiphenomenal.

Although Lucas frequently makes reference to general-equilibrium and to the Arrow-Debreu model, he applies very little of the technical work in general-equilibrium theory to his macroeconomic models. His references tend to be casual, in much the same way as Friedman (1968, p. 8) famously defines the natural rate of unemployment by reference to the Walrasian general-equilibrium model, while never actually deriving a natural rate in such a model. Lucas's "Expectations and the Neutrality of Money" to some extent serves a function not dissimilar to Klein's articles on aggregation. They provide a sort of theoretical reassurance: we could get the desired result out of a fully

articulated general-equilibrium model with heterogeneous agents; and knowing that we could, it is OK to short-circuit the process and to work with much simpler models. In "Some International Evidence on Output-Inflation Tradeoffs" (1973), Lucas also derives the epiphenomenal Phillips curve from many agents with differing information, but the real meat of the article is to justify an *aggregate* "surprise-only" aggregate-supply curve, the function that takes the place of the Phillips curve. It is only the aggregate relationships that are tested.

As was widely perceived at the time, the real "revolution" was the use of the rational-expectations hypothesis. Rational expectations were closely related to a form of general-equilibrium. The outcomes predicted by the model depended on the expectations held by the agents in the model and rational expectations were defined as those expectations that were consistent with those predictions. Thus, the model required a simultaneous solution of individual and systemic outcomes (e.g., the general price level) characteristic of general-equilibrium. But there was no necessary appeal to a *disaggregated* general-equilibrium of *heterogeneous* agents. Rational expectations themselves were a systemic characteristic, which despite Lucas's (1973) account of signal extraction, were not grounded in individual optimization behavior.[15]

Once a microeconomic rationale had been provide for the surprise-only aggregate supply function, most of the early new classical macroeconomics was devoid of direct reference to microeconomics. Lucas's (1972b) criticism of Friedman's interpretation of the natural-rate hypothesis joined a surprise-only aggregate-supply curve, aggregate demand based on the quantity theory of money, and rational expectations to generate both

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[15] Janssen (1993, p. 134) sees them as analogous to the price-vector in the Arrow-Debreu general-equilibrium model – a macro property for which no individual microeconomic account is provided, the "auctioneer" being merely a *façon de' parler*; see also Hoover (2009, p. 404).

the ineffectiveness of aggregate demand policy and an early version of the noninvariance or "Lucas critique" of econometric models. Sargent and Wallace's (1975, 1976) more famous papers on policy ineffectiveness were similar, replacing the quantity equation with an IS-LM representation of aggregate demand. Sargent's (1979) macroeconomics textbook emphasizes asset dynamics and rational expectations, but does not introduce an individual optimization problem until chapter 16 – a potpourri of new classical "topics." Few of the articles collected in Lucas and Sargent (1981) anthology of earlier new classical papers, *Rational Expectations and Econometric Practice*, either involve individual optimization or display explicit concern for microfoundations. As it title suggests the main focus of the early new classical macroeconomics was on the implications for rational expectations on econometrics – both the problem of estimating macroeconomic models with systemic ("cross-equation" constraints) imposed by rational expectations and implications of rational expectations for interpreting the evidence concerning the natural rate of unemployment (supporting claims for policy ineffectiveness). This strand of the new classical literature certainly paid no more attention – and, in fact, it would seem, rather less attention – to microfoundations than did the economists involved with the Brookings Model.

New classical microfoundations originate not so much in the rational-expectations hypothesis *per se* as in the interaction of rational expectations with the intertemporal optimization, starting with Lucas and Rapping's (1969, 1970) investigations of the labor market. Rational expectations requires modeling complete systems, not independent parts. To do so in a fully disaggregated dynamic intertemporal general-equilibrium model is simply too hard. This is exactly the same problem that Frisch faced in 1933.

Lucas and Sargent (1981, p. xiv) are clear that simplification is essential; the dimensionality of the problem must be reduced through various devices. They do not mention the representative-agent model in this context, but it was already a standard device, especially in optimal growth theory, and had, as we have seen, been used in Barro and Grossman's (1971) general-disequilibrium model. Sargent (1979, p. 371, fn. 4) refers to the "standard device of 'representative' agents" as needing no special justification and by the time that Sargent's *Dynamic Macroeconomic Theory* (1987) appears – the representative agent model has become the workhorse of the new classical macroeconomics. The new Keynesian graduate textbooks (Romer 1996 and Blanchard and Fisher 1989) also fully incorporate the representative agent.

It is striking, however, that there is little to no explicit justification of the representative-agent simplification. This is borne out by Table 2, which shows that 380 JSTOR articles after 1970 display the co-occurrence of terms in the representative-agent family and terms in the microfoundations family. In comparison "general equilibrium" co-occurs with more than twice as often, and "expectations" and "labor" more than three times as often. Nor does the small co-occurrence reflect merely a low usage generally: in the same period, terms in the representative family occur in 3,842 articles altogether.

The fact that no significant defense of the representative-agent assumption *as empirically applicable* is found in the new classical literature bears out the significance of these data. The qualification is important, because the representative-agent assumption poses less of a methodological hurdle when what is wanted is only some case in which aggregation is warranted – essentially Hicks's justification for appealing to the composite-commodity theorem. Sargent (1979, p. 371, fn. 4) seems to have something

like Hicks's justification in mind when he says that the representative-agent assumption

merely removes the burden of having to carry around a variable expressing the number of

individuals in the model. So, for instance, if one wanted to demonstrate that aggregated

relationships were not invariant to changes in taste and technology in a toy model,

constructing the special case in which the representative agent is just $N \times$ the individual

agents is unproblematic. But there is never any discussion of the conditions under which

this assumption is warranted more generally. It is truly just assumed without comment.

So, in effect, the new classic macroeconomists sleepwalked into their most characteristic

methodological position.

It is easy to imagine that the justification is tractability. Lucas justifies the use of

perfect competition on just this basis:

> The case for the use of competitive theory in modeling business cycles would, if I
> were to develop it here, be based entirely on convenience, or on the limits imposed
> on us by available technology . . .[Lucas 1980, p. 293, fn. 11]

The point is meant to generalize. The essay from which the quotation is drawn,

"Methods and Problems in Business Cycle Theory" is an extended methodological

defense of drastic simplifying assumptions and the need to recognize the constraints

imposed by the technology of scientific investigations (esp. section 1):

> The historical reason for modeling price dynamics as responses to static excess
> demands [as opposed to dynamic optimization with individual agents] goes no
> deeper than the observation that the theorists of that time did not know any other
> way to do it. [Lucas 1980, p. 286]

Presumably, the same justification would be offered in support of the use of a single

representative agent.

The advocates of representative-agent microfoundations face the same barrier to

complete disaggregation that Klein and advocates of the aggregation program faced. The

difference is that Klein took the data as the binding constraint: disaggregate as far as the data permit, looking for a general consistency with microeconomic theory. In contrast, Lucas and the representative agent program takes theory as the binding constraint: work out the theory in a tractable special case and disaggregate as far as the technical advance of microeconomic theory permits, looking for a general, nearly impressionistic, consistency with the available data.

While the cases appear symmetrical, they are not quite. The representative-agent program elevates the claims of microeconomics in some version or other to the utmost importance, while at the same time not acknowledging that the very microeconomic theory it privileges undermines, in the guise of the Sonnenschein-Debreu-Mantel theorem, the likelihood that the utility function of the representative agent will be any direct analogue of a plausible utility function for an individual agent. Kirman's (1992 survey article on the representative agent, which highlights the lack of analogy, is well-cited; yet, it is striking that almost all of the citations are by critics of the representative-agent program; there is little evidence that advocates have even noticed the argument against their approach.

The priority of theory in the representative-agent approach is most striking in with in the case of calibration methods, first used with real-business-cycle models, but now widespread (Kydland and Prescott 1982, 1991, 1996).[16] Traditional methods of econometric estimation and hypothesis testing are eschewed because the mismatch between models and data nearly always results in rejection of the model. But accepting the verdict of the hypothesis test is to resolve the tension between microeconomic theory and the data in the wrong direction.

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[16] Hartley, Hoover, and Salyer (1998) provides a critical account of the real-business cycle model.

Recently, the representative-agent program has begun to push toward models with more heterogeneous agents (e.g., Krusell and Smith 2006). The move is analogous to the movement between early macroeconometric models and the more disaggregated Brookings Model. But in one sense the object is different. The object of representative-agent microfoundations is ultimately to eliminate macroeconomics – to derive all results from microeconomic theory. Distinctively macroeconomic phenomena on this view are, like Lucas's analysis of the Phillips curve, merely epiphenomena. In contrast, the object of the aggregation and general-equilibrium programs of microfoundations was to push towards an understanding of how genuine macroeconomic phenomena arise out of microeconomic behaviors. These programs are non-eliminative.

6. Microfoundations – Yesterday and Today.

The term "microfoundations" dates from the mid-1950s, but the awareness of microfoundations as a methodological program explicitly present in the consciousness of the economics profession really begans with Phelps's *Microeconomic Foundations* and the new classical assault on Keynesian orthodoxy – especially on Keynesian macroeconometrics – around 1970. The mainstream microfoundational narrative finds virtue in deductive rigor, the unity of economics, and the invariance of econometric relationships. The rhetoric of microfoundations draws on an image of a macroeconomics grounded in microeconomics – anything we want to know about the economy is derivable from the analysis of individuals, taking only their tastes and the constraints of technology and resources as given. While this vision of microfoundations is newly

articulated in the 1970s, it is an essential element of its rhetorical success that it claims to be returning to a sounder, pre-Keynesian conception of economics.

An historical irony is that the earliest surveys and histories of microfoundations (Weintraub 1977, 1979) and methodological investigations (Janssen 1993) played an important part in promoting the rhetoric of microfoundations, but in the main did not address the mainstream narrative. Instead, they mainly documented a distinct program in general-equilibrium theory. In doing so, they highlighted the fact that the relationship between microeconomics and macroeconomics was a central issue, starting when these distinctions were first drawn by Frisch in the early 1930s.

The mainstream narrative portrays macroeconomics as a retreat from sound microeconomics and as based in nonstructural associations among aggregated variables. This picture is vastly different from the visions of Frisch or Keynes. Explicitly for Frisch and implicitly for Keynes, microeconomics was the economics of a part of the economy, holding the remainder of the economy constant in the background; while macroeconomics was the economics of the economy as a whole. The distinction was inspired by Marshall. Macroeconomics is general equilibrium, provided that term is broadly conceived to refer to the properties of interdependent systems taken as a whole and not to Walras's or Arrow and Debreu's conception of such systems. This was clearly Frisch's conception of macroeconomics. Aggregation did not define macroeconomics for Frisch; it mattered only practically.

Keynes did not share Frisch's ambitions to develop either an applied econometrics or a *formal* economic dynamics. He was not, therefore, as constrained as Frisch by practical considerations, and he formulated his macroeconomics with essential

reference to heterogeneous individuals. Contrary to the characterization of Keynes

familiar from Lucas and the mainstream narrative, the individual played an essential role

in Keynes's analysis, even though he denied the vision of macroeconomics as having

been built upward deductively from self-sufficient, autonomous microeconomic units.

Microeconomics on this interpretation of Keynes is the economics of a part in the context

of the whole. Microeconomic parts are neither self-sufficient nor autonomous on this

view; microeconomics presupposes, and takes, macroeconomics as given.

Macroeconomic properties emerge out of and transcend the individual, and provide the

background to microeconomic decision-making. Keynes's conception of

macroeconomics, I believe, has not been widely appreciated and has not much influenced

mainstream macroeconomics.

Frisch used aggregates, but provided no account of the relationship of aggregates

to individuals. Keynes sidestepped aggregation problems by providing an account of

macroeconomics that was not meant to support econometrics. Both Frisch and Keynes

wished to provide dynamic, general-equilibrium accounts of macroeconomics, but neither

understood these accounts in terms of the Walrasian model.

In contrast, Hicks, who also wanted a dynamic macroeconomics, was a Walrasian

and was concerned with aggregation. Hicks's composite-commodity theorem isolates

stringent conditions under which aggregation works. One reaction to the theorem was to

accept that the conditions are too stringent and that, therefore, only the analysis of

general-equilibrium models in which every individual was specified would be acceptable.

Another reaction is to say that the theorem isolates a special case in which aggregation

works; and, by sticking to the special case, we can construct aggregate models to

investigate other issues. The first reaction is what generated the general-equilibrium

program of microfoundations, ably documented in Weintraub's history. The second

reaction inspired the large number of representative-agent models (e.g., optimal growth

models) that became workhorses of macroeconomic theory in the 1960s and 1970s.

In either its disaggregated or aggregated form, the general-equilibrium program of

microfoundations avoids the practical problems of empirical macroeconomics. Empirical

macroeconomics must come to terms with aggregation. Klein took the issue to be so

important that he defined the distinction between micro and macro in terms of

aggregation. Klein and others working in the aggregation program of microfoundations

took reassurance from the theoretical results of the general-equilibrium program that

suggested that micro and macro can be rigorously connected in principle. But their

central priority was empirical economics. Thus, microfoundations in the aggregation

program was a matter of working down from aggregated data to as much disaggregation

as practical, rather than working up from individual optimization deductively.

Economists in the aggregation program looked for consistency between individual

optimization and aggregate outcomes and between microeconometrics and

macroeconometrics, but they did not hope for a formal deductive path from individuals to

the aggregate. Later work on aggregation theory reinforced the view that ground-up

derivations were not feasible, providing support for Klein's initial strategy.

The new classical macroeconomics was initially a reaction to the aggregation

program – indeed, Lucas and other new classicals originally worked within the program.

They perceived a weakness in the Keynesian account of labor markets and sought to

repair it with a greater emphasis on dynamics and expectations. Their preferred approach

to expectations, the rational-expectations hypothesis, invoked a system property, which undermined Klein's piecemeal strategy of investigating different Keynesian functions separately. The analysis of complete systems is difficult, if they are at all complex. So, just as Frisch before them, they began to work with drastically simplified systems. And, just as Klein before them, they faced a tradeoff between theoretical tractability and empirical relevance. Whereas Klein had resolved the tradeoff by giving priority to empirical applicability and aspiring to move toward greater and greater disaggregated detail, the new classicals placed the priority on consistent theory. Whereas Klein had started with aggregates and hoped that their relationships would not prove to be fatally inconsistent with individual behavior; the new classicals started with the special case of the representative agent and hoped that its implications would not prove to be fatally inconsistent with data. The new classicals aspired toward an elaboration of their simplified theory – that is, toward a detailed theory of heterogeneous agents – but did not allow their results to be held hostage to achieving that elaboration any more than Klein allowed his results to be held hostage to disaggregation. Yet, there was an important methodological disanalogy. Klein was explicitly aware of the difficulties posed by aggregation. The new classicals treat it as a non-issue, showing no appreciation of the theoretical work on aggregation and apparently unaware that earlier uses of the representative-agent model had achieved consistency with theory only at the price of empirical relevance.

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Table 1
Microfoundations and Individual Economists

	Barro and Grossman	Clower	Lucas	Phelps	Weintraub
Barro and Grossman	214	82	113	101	14
Clower	82	267	103	62	38
Lucas	113	103	652	303	33
Phelps	101	62	303	653	30
Weintraub	14	38	33	30	142

Notes: entries are the number of articles in 97 JSTOR economics journals 1956-2009 that include terms from the microfoundational family (see main text fn. 7 for a definition) and each of the economists' names. Entries along the main diagonal are total occurrences; entries in off-diagonal cells are the co-occurences of corresponding pair. While the probability of assignment of an economist with the same surname as the target seems low, there is no effort to distinguish Sidney from E. Roy Weintraub.

Table 2
Microfoundations and Key Concepts

Search Terms	Through 1969	1970 and after
Expectations	1	1,344
Expectations and Labor	12	850
Rational Expectations	0	718
General Equilibrium	1	810
Keynes or Keynesian	16	1,048
Labor	19	1,330
Labor and Expectations	12	850
Lucas-Critique Family[1]	0	77
Phillips Curve	7	579
Representative Agent Family[2]	1	380
Representative Agent	0	153
Representative Consumer	1	107
Representative Firm	0	151
Representative Household	0	69

Notes: entries are the number of articles in 97 JSTOR economics journals 1956-2009 that include terms from the microfoundational family (see main text fn. 7 for a definition) and each of the search terms.
[1]*Lucas-critique family* = "Lucas critique" or "noninvariance" or "non invariance."
[2]*Representative agent family* = any of the terms listed separately as subcategories.

Table 3
**Phelps, Lucas, and Microfoundations**

	Lucas	Phelps
1970-1979	87	259
1980-1989	228	235
1990-1999	212	124
Total 1956-2009	652	653

Notes: entries are the number of articles in 97 JSTOR economics journals 1956-2009 or subperiods that include terms from the microfoundational family (see main text fn. 7 for a definition) and each of the economists' names.

Figure 1
The Diffusion of "Microeconomics" and "Macroeconomics"
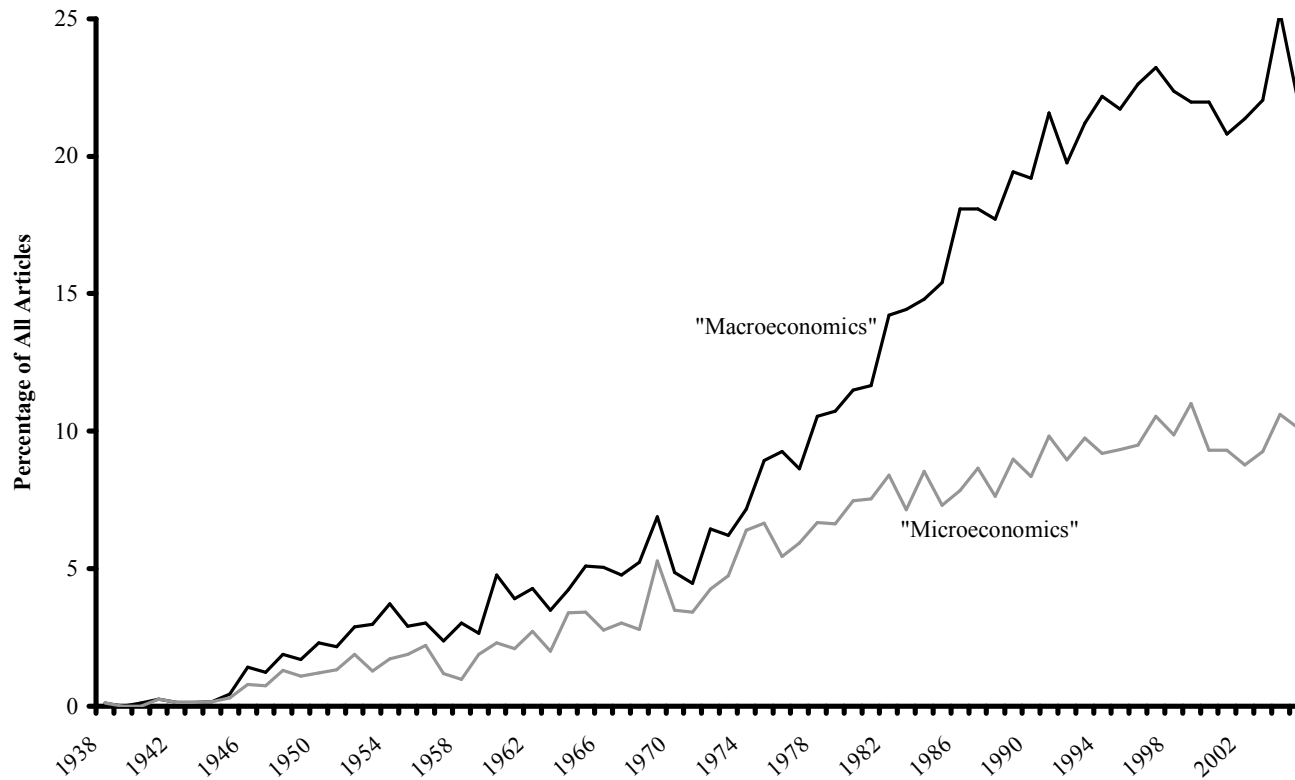
Notes: entries are the ratio of the number of articles in 97 JSTOR economics journals for each year 1938-2006 using either a term in the microeconomics family or a term in the macroeconomics family to the total number of articles. See main text fn. 6 for definitions of these families
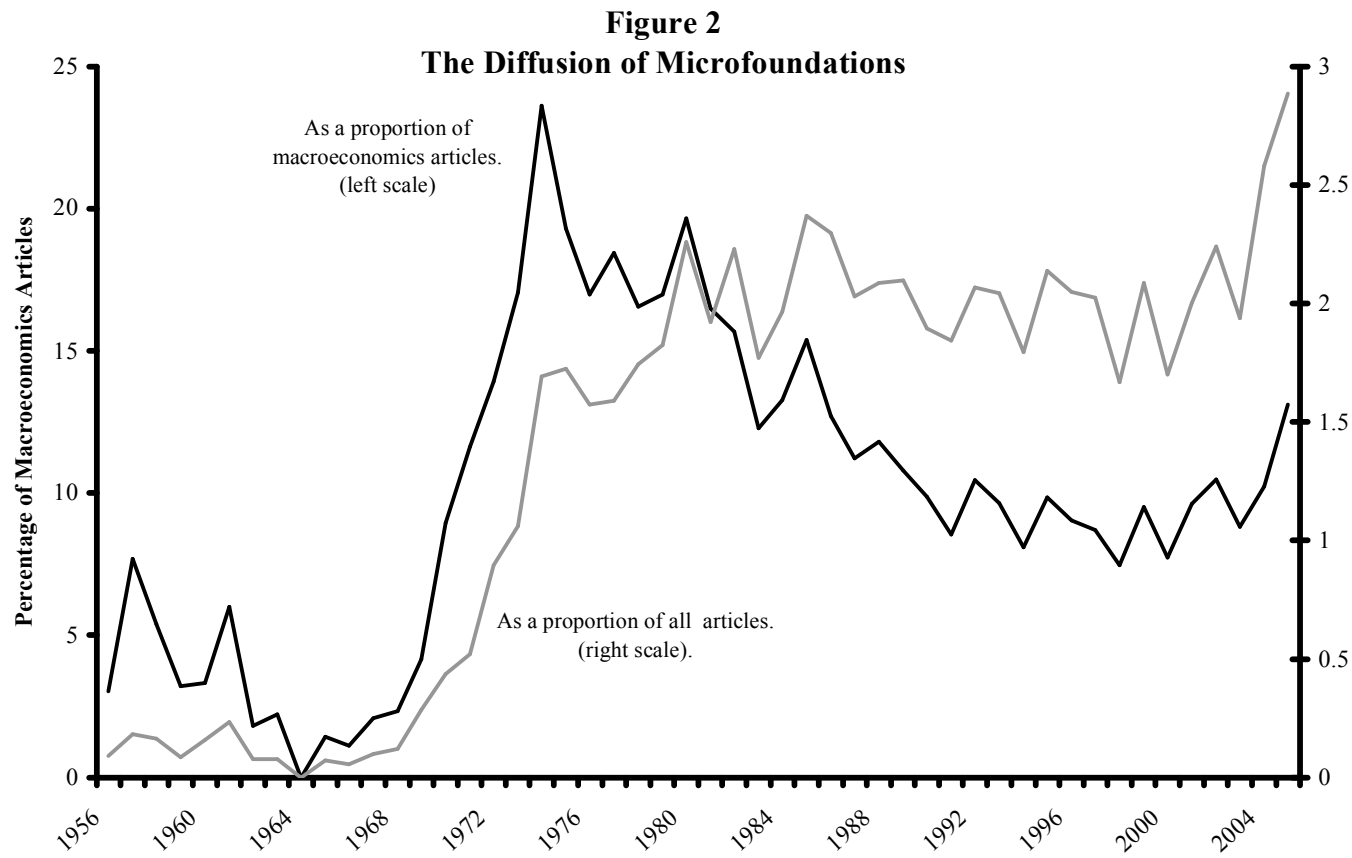
Figure 2
The Diffusion of Microfoundations

Notes: entries are the ratio of the number of articles in 97 JSTOR economics journals for each year 1956-2006 using a term in the microfoundational family to either all articles or to all articles using a term in the macroeconomics family. See main text fns. 6 and 7 for definitions of these families