The near emptiness of general equilibrium theory is a theorem of the theory.—Christopher Bliss, “Oil Trade and General Equilibrium”

In the 1970s, Hugo Sonnenschein, Rolf Mantel, and Gerard Debreu established a series of results regarding the properties of aggregate excess demands arising in Walrasian general equilibrium models of the Arrow-Debreu-McKenzie type. They showed that, despite the usual assumptions made on preferences, endowments, and technology, the aggregate excess demands were arbitrary, save that they satisfied Walras’s Law and continuity (Sonnenschein 1972, 1973a, 1973b; Mantel 1974, 1976, 1977; Debreu 1974). Although some other authors made striking additions to this characterization (especially Alan Kirman and Andreu Mas-Colell and their collaborators—Kirman and Koch 1986; Mas-Colell 1977; McFadden, Mas-Colell, Mantel, and Richter 1974), we can call this body of work smd theory after the initials of the three originators.

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SMD theory has important implications. It shows that good behavior at the microeconomic level is consistent with arbitrary behavior at the macroeconomic level. Therefore, the many economists who looked for microfoundations of systematic aggregate phenomena could not find them in these general equilibrium models. SMD theory also means that general results on, for example, uniqueness (Mas-Colell 1977; Ingrowth and Israel 1990, ch. 11), stability (Sonnenschein 1973a; Ingrowth and Israel 1990, ch. 12), comparative statics (Kehoe 1985), econometric identification (Diewert 1977; Stoker 1984a, 1984b), imperfectly competitive general equilibrium (Roberts and Sonnenschein 1977), and microfoundations of macroeconomics (Rizvi 1994b), are unobtainable on the basis of the commonly accepted Walrasian microfoundations. The general equilibrium research program, which has been running since the 1950s on the fumes of a huge variety of existence theorems for its general results and on particular case after particular case in other areas, has finally run out of gas.

The growing realization that this is the case has led to a variety of responses in recent economic writing. This article explores one of these—the distinctive contributions of the European economists, Werner Hildenbrand, Jean-Michel Grandmont, and their collaborators. In doing this, I want to more generally describe a change in attitude among economists. This shift is not uniform, certainly, but it is occurring among a significant number of practitioners and will have to be dealt with by their detractors. The new attitude is characterized by an empirical turn, a modesty concerning goals, an openness toward using a variety of methods and data, and—above all—an abandonment of strict microfoundations (which were based as much as is possible on individualistic assumptions). My argument is that these changes have been precipitated by a clearer understanding of the importance of SMD theory.

Section 1 sketches the history of attitudes toward SMD theory and to microfoundations, and demonstrates that initial skepticism regarding SMD theory has turned into a growing if grudging acceptance. This acceptance is inversely related to adherence to strict microfoundations. Section 2 looks at Hildenbrand’s and Grandmont’s work on market demand. This work signifies a very large change in what is the appropriate method for dealing with aggregate economic phenomena. The implications of this change are discussed in section 3, after which I conclude the paper.
1. Attitudes toward SMD Theory and Microfoundations

During the 1970s and into the first half of the 1980s, the acceptance of the importance of the SMD results remained weak to skeptical (Rizvi 1994b, 361), outside of a small group of general equilibrium theorists such as Herbert Scarf (1973) and Egbert Dierker (1974). The belief in microfoundations—that all statements about the macroeconomy should be derived from microeconomic data considered individually—remained correspondingly strong in this period. E. Roy Weintraub, in his book on microfoundations, could accurately state that “even those few economists who argue that current microeconomics does not generate macroeconomics have been extremely shy in their attempts to convince their colleagues of the seriousness of their concerns” (1979, 5). There seemed to be consensus around Allan Drazen’s view that “explanations of macroeconomic phenomena will be complete only when such explanations are consistent with microeconomic choice theoretic behavior and can be phrased in the language of general equilibrium theory” (1980, 293). These views were so common that by 1986 James Tobin lamented their ubiquity and their exclusive use in publication, peer review, and hiring (350).

Matters started to change slowly in favor of SMD theory’s acceptance in 1982. In that year, Wayne Shafer and Hugo Sonnenschein codified the arbitrariness results in a chapter of the Handbook of Mathematical Economics, and the SMD results and their robustness should have been apparent to a wide audience of economists. SMD theory began to be mentioned in macroeconomic contexts following this publication (Rizvi 1994b, 361, n. 2). Things came to a head at the end of the 1980s with the publication of Alan Kirman’s 1989 article on the “intrinsic limits” of general equilibrium theory, and the process of dealing with the SMD results continues to this day. By now, however, even such erstwhile stalwarts of general equilibrium theory as Christopher Bliss compare that theory to theology (1993, 227).

Moreover, it can be argued that a great deal of the methodological pluralism in microeconomics since the mid 1980s has resulted from the theoretical dead end made obvious by the SMD results. For example, there is evidence that the turn to game theory around 1985 was directly influenced by the realization that SMD theory blocked progress in the general equilibrium mode (Rizvi 1994a, 2–6). Some authors, such as Donald Saari (1991) and adherents of the Santa Fe School, have embraced the
complexity that SMD theory derives, and they have not sought regularities at the aggregate level (Rizvi 1994b, 370–73). The European work on the "law of demand" as well is directly inspired by the SMD results, and it is to this work that we now turn.

2. Market Demand from Distributional Restrictions

Werner Hildenbrand and Jean-Michel Grandmont have been interested in demonstrating conditions under which the "law of demand"—the inverse monotonic relation between price and quantity demanded at the aggregate level (or more precisely, that the changes of price and of quality demanded point in opposite directions)—holds true. In his Alfred Marshall lecture called "Facts and Ideas in Microeconomic Theory," Hildenbrand considers the law of demand to be "fundamental and indispensable," since it is needed to do comparative statics, "which is the ordinary job of the economist" (1989, 252). However, both authors understand that SMD theory demonstrates that this cannot result from general equilibrium theory as ordinarily construed (Hildenbrand 1989, 252; 1994, preface; Grandmont 1992, 2). Thus, for both economists, a substantial change in approach is required to proceed.

Their approach contrasts with the standard approach in getting definite results at the aggregate level. The usual approach was to consider individual data and to place more and more restrictions on them—for example, the assumption of homotheticity in the area of preferences—until regularity resulted at the aggregate level. Grandmont describes this as putting "constraints on the range of the agents’ allowable characteristics" and rejects its use as being too restrictive (1992, 3). Hildenbrand also rejects the usual approach, in quite forceful terms, on epistemological grounds. For him, assumptions such as homotheticity at the individual level cannot serve as a basis for monotone market demand since "individual preference relations are not observable and we have no criterion to judge assumptions on preferences as 'reasonable'"; he asks, rhetorically, "Do good looking indifference curves describe typical behavior?" (257). He continues, "For this reason, the axiomatic individualistic approach—which consists in making ad hoc assumptions on individual preferences or demand functions in order to derive properties of the market demand function—cannot, in my opinion, lead to a satisfactory foundation of the Law of Demand. An economic foundation of the Law must, as much as possible, be based on observable entities" (257).
Grandmont also considers that an empirical basis for market demand is preferable. Moreover, it is possible since the approach of wanting to “impose qualitative and quantitative restrictions on the shape of their distribution” can in principle be verified (1992, 3). Before turning to the empirical results that Hildenbrand discusses, let us consider the sort of restrictions Grandmont and Hildenbrand have in mind. There have been two main approaches (Grandmont 1992, 6). One is to assume some minimal sort of rationality at the individual level, and then combine this with observable distributional restrictions at the macro level. The other is to assume no rationality at all, only satisfaction of budget constraints, but this approach has fewer testable implications.

Hildenbrand takes the first approach (1983, 1989, 1994). The minimal rationality corresponds to his “ideas,” the distributional restrictions to “facts.” Although Hildenbrand is quite skeptical of untestable rationality assumptions, he assumes that individual demands derive from preference maximization or satisfy Weak Axiom of Revealed Preference only for “lack of a better alternative.” In any case, Hildenbrand’s approach is empirically based only to some extent, although he is very clear about this “limitation.” Yet given the Weak Axiom, the individual Slutsky-substitution matrices are each negative semidefinite. The mean of these individual substitution-effect matrices, which refers to the aggregate level, is then also negative semidefinite (Hildenbrand 1989, 252). There is thus no impediment to downsloping aggregate demand here for substitution-effect reasons. The problem arises with the matrix of the means of the individual Slutsky income-effect matrices, which cannot be assumed in general to be positive semidefinite. At this point, Hildenbrand relies on an argument from John Hicks’s Revision of Demand Theory, which claims that the heterogeneity of consumer characteristics implies that individual income effects will balance one another at the aggregate level, leaving the well-behaved substitution-effect to predominate (Hicks 1956, 144; Hildenbrand 1989, 258–59). Thus regularity might be expected to occur at the macro level, because of aggregation, and which could not be expected at the individual level.

Hildenbrand investigates this idea formally and empirically. For instance, he demonstrates that the mean demand of a continuum of consumers will be downsloping in prices if the distribution of incomes is independent of prices (this is a strong assumption, of course, but Hildenbrand has used it consistently) and if the density of the income distribution is downsloping—that is, wealthier consumers are fewer than poorer
consumers (Hildenbrand 1983). Empirically, these hypotheses could be violated without violating the result, since the aggregate income-effect matrix or related matrices can be estimated directly (Hildenbrand 1989, 266). Using the United Kingdom’s Family Expenditure Survey for 1969–83, Hildenbrand shows the matrix to be positive semidefinite for all years for expenditures (not quantities) of nine commodity groups such as housing, food, clothing, and footwear. In Hildenbrand (1994), he reports similar results for French consumption.

Thus, for Hildenbrand, the Law of Demand is a hybrid of theory and facts. On the one hand, “it rests on standard theoretical, yet more or less ad hoc and untested, hypotheses on individual behavior” and, “on the other hand, it rests on observable regularities of consumption patterns of large populations” (1989, 275).

Grandmont’s (1987, 1992, 1993) approach is less amenable to empirical testing, but it is a more radical departure from standard theory in that it forsakes individual rationality altogether. Grandmont is inspired by Hildenbrand’s theoretical demonstrations of downsloping aggregate demand, calling Hildenbrand (1983) an “outstanding methodological achievement in that it shows by way of example the applicability of the distributional viewpoint to demand analysis” (1992, 3). In regard to rationality, Grandmont recalls Gary Becker’s (1962) demonstration that if choices are uniformly distributed on the budget plane (!), then demand is downsloping, equally distributed among commodities, and of the Cobb-Douglas type. This is clearly too special, but Grandmont preserves Becker’s idea that consumers’ choices simply respect their budget constraints and need not be derived from constrained maximization. Grandmont then describes a metric on the space of consumers’ characteristics, and using it, he can say what it means for those characteristics to become more dispersed. Grandmont’s results are that increasing dispersion among consumers results in downsloping aggregate demand, and also yields other desirable aggregate properties. These properties are all the result of aggregation itself, based on a particular distribution of characteristics among individuals in society. Nothing so well-behaved is going on at the micro level.

3. Implications of the New Approach

An important feature of these approaches is that they are no longer based on strict microfoundations. Societal characteristics are used to un-
pin the analysis of other macroeconomic regularities (Kirman 1989, 138). Very little attention is paid to restricting the nature of individual characteristics—quite a lot to their distribution in the population. Without realizing it, perhaps, these authors have rediscovered the probabilistic approach that was popular in other social sciences many decades ago. In general, Hildenbrand and Grandmont pay little attention to the antecedents of their approach. However, both Hildenbrand and Grandmont refer to Becker (1962). Recently, Hildenbrand (1995, 329), in a reply to critical review by Hendrik Houthakker of Market Demand, pointed out that Houthakker himself (in Houthakker 1955) used the distributional approach in an analysis of the production function (to demonstrate that it would be Cobb-Douglas if the distribution of firms had a particular shape) and that this was crucial in influencing his own work. Edmond Malinvaud (1993) also refers to Houthakker 1955 as “one of the first applications of the [distributional] approach” (125) and mentions Pareto’s law of income distribution and Schumpeter’s admiration for it (129–30). A more inclusive list of antecedents in the economics of the post–World War II period would need to include Karl Borch (1953) and Benoit Mandelbrot’s (1961) work on the Pareto distribution (Rizvi 1994, 371–72). It seems that the distributional approach enjoyed a brief resurgence among European expatriate economists in the United States in the early post–World War II period. Nancy Wulwick (personal communication) has also brought to my attention work by Basmann, Battalio, and Kagel (1976)—published in Europe and largely overlooked—which is very much concerned with a distributional approach to macro level regularities. Indeed, such an approach was indicated by Rutledge Vining over fifty years ago:

Distributions of economic variates in as large groups as can be obtained should be studied and analyzed. . . . That is to say, statistical economics is too narrow in scope if it includes just the estimation of postulated relations. Probability theory is fundamental as a guide to an understanding of the nature of the phenomena to be studied and not merely as a basis for a theory of the sampling behavior of estimates of population parameters, the characteristics of which have been postulated. In seeking for interesting hypotheses for our quantitative studies we might want to wander beyond the classic Walrasian fields and poke around the equally classic fields once cultivated by . . . Lexis, Bortkiewicz, Markov, and Kapteyn. (Vining 1949, 85)
Claude Ménard (1987) has investigated why the classic probabilistic approach did not become popular in economics, and its revival also needs to be explained. At all events, it is clear that the taboo against using preexisting societal regularities in the investigation of other social phenomena is being lifted in economics. In addition, the use of a continuum of agents to justify the price-taking assumption in general equilibrium theory—a large concern of Hildenbrand’s in the 1970s—may have made working with distributions in the present context easier for a number of economists brought up on general equilibrium theory.

The distributional restrictions can be investigated both empirically, as Hildenbrand has done, and theoretically. Both Kirman (1989) and Grandmont (1992) have called for the study of elements of social cohesion that are not based in market interactions, but which can give rise to the distributional restrictions envisaged in market-demand theory. This call is again quite a change as it puts nonmarket interactions among agents at the forefront, instead of as exceptions to the analysis. Kirman and his coworkers have pursued this approach, building on H. Föllmer (1974). In addition, S. Abu Turab Rizvi and Rajiv Sethi (forthcoming) show how the complicated interaction of imitation, habit formation, and the pursuit of novelty in consumption might lead to relatively simple limiting behavior of consumption in the aggregate.

The writings on market demand also signal a shift in economists’ attitudes toward rationality. Essentially, rational behavior is employed only grudgingly—as by Hildenbrand (1994, 10–14)—if at all. It is considered either unscientific or irrelevant to the goal of establishing aggregate regularities. In Grandmont’s models, rational behavior is not postulated at all. This, of course, creates some “embarrassment of riches” for him, as it suggests that individual “rationality” may not be as necessary as some economists would like to believe to the construction of a “sound macroeconomic theory” (1992, 6–7). Grandmont expands on this idea in a passage that shows how big the change wrought by SMD theory really is:

The present analysis raises again some (old) questions about the actual status of the postulates of individual “rationality” in the Walrasian paradigm. It suggests in particular that such postulates might not be as necessary as some would like to believe to the construction of a sound quantitative macroeconomics. An alternative research strategy would be indeed to actually reverse the traditional Neoclassical research pro-
gramme, and to try to obtain some form of aggregate rationality as in the present paper (but surely not the existence of a "representative" consumer!) by relying more on particular features of the distribution of behavioral characteristics among the members of the system under consideration. (Grandmont 1992, 33)

The literature on the criticism of methodological individualism and rational behavior is, of course, quite immense; and even general equilibrium theorists have been contributing to the criticism of this "touchstone of accepted economics" (Arrow 1994, 1, in his Ely Lecture). But it is striking that the main thrust in Hildenbrand's and Grandmont's rejection of the use of individual rationality is that it is neither necessary nor sufficient to aggregate economics: not sufficient, because of the SMD results, not necessary because of distribution-based approach. Their change seems much less due to criticisms and doubts about the rationality approach at a methodological or philosophical level than it does to its lack of fruitfulness.

It is hard to overemphasize the nature of the change to theory made by an abandonment of rationality assumptions. In particular, one point has not been sufficiently emphasized, although it has been alluded to (Hildenbrand 1989, 251): the utility basis for economics traditionally serves two purposes: it motivates choice behavior and it provides an indicator of welfare. When Hildenbrand, Grandmont, and others abandon rationality assumptions, they also lose the welfare or normative basis of neoclassical economics. This is why Gustav Cassel's approach of beginning with individual demand functions as being elementary (to which Hildenbrand 1994 returns) was not popular. None of the "invisible hand" results—the two welfare theorems—can be demonstrated in a nonoptimizing framework. Thus the distribution-based approach is more subversive than it at first might seem. This observation also indicates why the distribution-based approach has not met with universal acclaim.

Specifically, a divide seems to be forming between those American theorists who understand SMD theory but have not committed to a systematic approach to aggregate economics in response to it, and the European economists who are willing to jettison large chunks of received theory. Some Americans seem quite reluctant to give up this theory. Arthur Lewbel (1994b), in an illuminating exposition and critique of Hildenbrand's Market Demand, begins with a long list of authors on demand theory to which Hildenbrand does not refer, emphasizing the renegade
aspects of the book. He also repeatedly points out Hildenbrand’s reluctance to use behavioral (rationality) assumptions. Lewbel’s attitude carries over from his earlier description of Hildenbrand’s work as making insufficient reference to traditional aggregation theory. Specifically, Lewbel charges that Grodal and Hildenbrand (1991) “fail to acknowledge how much their work borrows from the exact aggregation literature” (Lewbel 1994a, 129). Here, Lewbel specifically mentions his own work (Lewbel 1991) as being not adequately cited. Similar tensions surface in a review by Houthakker (1995) of Hildenbrand’s book. The review is quite revealing since it is followed by a reply by Hildenbrand (1995) and a rejoinder to that by Houthakker. Houthakker chides Hildenbrand for “some sweeping assertions concerning past research that need examination” (327) and also for rejecting the usual methodology of economics, which he asserts is up to the task posed by demand analysis (327–28). Hildenbrand’s response makes it clear that he is undeterred by these criticisms and, what is more, that he is currently working on developing his work into a “dynamic theory of market,” which explicitly takes the time-dependence of demand into account.

The new approaches discussed in this article are indeed a far cry from the choice-theoretic, general equilibrium approach of fifteen years ago. Not surprisingly, the attitude toward macroeconomics has also changed in the European economics milieu. Evidence for this can be seen in Malinvaud’s 1989 presidential address to the European Economic Association, “Observation in Macroeconomic Theory Building.” This address shares the characteristics of the recent views of Grandmont and Hildenbrand: an empirical turn to economic writing (Malinvaud 1989, 218), a yearning to be practical and relevant (205), a broadening toward what might be an acceptable observation base for economics (216–18; this includes panels of national data, expectations surveys, evidence of disequilibrium, historical approaches, experimental data, and socioeconomic structures), all combined with an avowed sense of the modesty of the whole exercise.

Malinvaud begins with an acknowledgment that “it was repeatedly found with uneasiness that the properties derived from pure theory are too inconclusive” and describes the twin crises caused for macroeconomics by the disputes over the role of rationality in model building and the challenges made against the probability approach of its allied econometrics (1989, 207, 206–13). He wants to avoid a macroeconomics that has “become a detached intellectual play” (205) and feels that the methodological strictures of strict microfoundations—that one should recognize
“only constraints, behaviour and equilibrium conditions” (207)—went too far; “Clearly, such a stand is too dogmatic, even if it is induced by the normal teaching of microeconomic theory. . . . If we can correctly identify elementary behaviour, this is perfect; but when we cannot, we have to stick to observing results of a more complex sort, and we should not impose on the data an unfounded preconception” (207). He now favors eclecticism over rigor: “But the notions of proper concepts, true models and true facts cannot and should not be interpreted strictly; somewhat tentative notions or constructs are not ruled out. This open-mindedness and eclecticism is again consistent with the view that one tries to benefit, as much as is possible, from present knowledge, even from that part which is not hard and fast” (209). Malinvaud’s assessment of the strict microfoundations impulse is quite harsh:

The same general idea of a discipline that theoreticians ought to impose on lousy macroeconomics often occurs in less extreme form. In particular it was often stated that disequilibrium models were not admissible as long as the rationale for sticky prices had not been elucidated, or as long as the price and wage dynamics had not been derived from the study of elementary behaviour behind it. The indisputable impact that such claims had in the profession reveals a kind of methodological perversity about which one ought to reflect. (212)

In an essay on the macroeconomic implications of microeconomic theories, Malinvaud (1992) again considers this issue. He argues that the differences in perspective between microeconomics and macroeconomics invalidate any easy transposition from the micro to the macro level. For microeconomic “speculations” to be fruitful, they must be formulated in a macroeconomic framework and tested and validated against the data (12–13). He takes comfort in the fact that “As W. Hildenbrand’s (1989) research seems to show, aggregated relations could well appear commonly less complex than what certain segments of mathematical economics suggests” (18; trans. Elise Joosandr) while at the same time, he suggests that such an approach ought to take production factors more closely into account (20–21; Malinvaud 1993). In a recent article on rationality, Malinvaud (1995) dismisses the easy fiction of a representative agent. He considers Grandmont’s (1992) work to be “special but revealing” in its approach to aggregation, where “the emphasis must shift” to “bear in mind the observed properties of statistical distributions of
the individual characteristics of agents and of their behaviors. Werner Hildenbrand's recent book (1994) perfectly shows the efficacy of this method" (535; trans. Elise Josserand). In these ways Malinvaud's writings show a distancing from the exclusive focus on microeconomics of the past, a concern with empirical validity as opposed to deductive speculation, and a cautious but respectful attitude toward the work of Grandmont and Hildenbrand.

The relative autonomy of recent European writing on aggregate economics can be seen in a number of other ways as well. Several issues of Ricerche Economiche in 1993 are devoted to the new approaches to aggregation I have been discussing. This includes Malinvaud's (1993) essay on aggregation, in which he highlights the distributional approach (calling it the statistical approach) and gives it equal footing to other, more traditional approaches to aggregation. He indicates that contributions in this vein "are not only interesting in themselves, but also show the way for a very promising trend" (125). Roger Backhouse (1995) has argued along different lines for the distinctiveness of European macroeconomics. He also points out to me (personal communication) that European economists are becoming more self-aware of their distinctiveness, as is indicated for example by the special issue of Kyklos (1995, 48.2) devoted to European economics. While there certainly are American responses to SMD theory (such as, it can be argued, complexity approaches, experimental economics, and game theory), so far they do not concern reformulating macroeconomics. For example, as Jim Hartley has observed (personal communication), even when Joseph Stiglitz (1992) mentions SMD theory, he does so only in passing. The recent work by Dhananjay Gode and Shyam Sunder (1993), building on Becker (1962), seems also to be motivated by concerns other than macroeconomics. There is, however, an interesting exception to the rule of Europeans pursuing statistical approaches to market phenomena. I refer to Duncan Foley's (1994, 1996) work on statistical equilibria. He, however, derives statistical regularities from certain assumptions instead of imposing them as distributional restrictions. For instance, he supposes that there are many agents with each offer set, and that each action within an offer set is equally likely. This setup permits Foley to characterize equilibria statistically, as the outcome most likely to be realized. It is clear, therefore, that the center of gravity of the distributional approach to aggregate phenomena lies in Europe.
4. Conclusion

The thesis of this article, that it is much easier to understand contemporary work in economics by looking at the impact of SMD theory, can be encapsulated by contrasting Drazen’s comments in 1980 and Malinvaud’s in 1989, both quoted earlier here. Whereas Drazen felt that “explanations of macroeconomic phenomena will be complete only when such explanations are consistent with microeconomic choice theoretic behavior and can be phrased in the language of general equilibrium theory,” Malinvaud characterizes such a view as methodologically perverse. It is a significant comment on what causes change in modern economics that the economists mentioned here, who are now led to hold such “revisionist” sentiments about microfoundations, have probably long had doubts about the foundational problems with their earlier approach. Moreover, the very path they now take had been pointed out a number of times before, as instanced by Vining’s comments quoted above. Yet it seems clear that the factor that directly motivates the shift to the new work is that the attempt to uphold the earlier methodological strictures failed, and it was found that progress along those lines was not possible. The theoretical vacuum created by the SMD results then elicited the distinctive European approach to the relation between micro variables and macroeconomic regularities.

References


