

Axiomatics in Economics*

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As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality.

A. Einstein [29, 270]

I. Preliminaries

A correspondent who heard me talk last year about “Economics as an Inductive Science” interpreted me as saying “axiomatic theories are useless.” Since I neither said nor intended to say anything of the sort, I answered him to that effect, and shortly after received a second note to inform me that in any event my published address [13] did little but disparage neowalrasian theory, a charge I had no wish to deny. So his question was “Why did you so disparage neowalrasian analysis?,” and my answer was: “Because I think neowalrasian¹ theory has been generally deleterious to the advancement of economic science.” Although that remark is dated, the sentiment it expresses has not changed. I know my 1993 address struck some as concerned more with generalities than specifics; so I have no one but myself to blame if what I considered *refutations* were regarded by some as *fulminations* [57, 96]. In the discussion that follows, I focus on specifics, hoping thereby to persuade readers that my unsupportive attitude towards neowalrasian theory deserves to be emulated by all thoughtful economists.

II. Is Axiomatics Useful?

I want first to dispose of the canard that I have anything but respect for axiomatics. Countless examples demonstrate that axiomatization of disciplines rooted in demonstrative reasoning

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1. Why do I write “neowalrasian” rather than “neo-Walrasian”? Because I refer to a body of doctrine that begins with the “numeraire” model in *Leçon 12* of Walras [68], jumps to Pareto and Slutsky (1892 and 1915), continues through Hicks and Allen (1932), to Hicks [33, 85], to *Value and Capital* [31], then through Samuelson [54] to Arrow-Debreu [2] and Debreu [17]. By the end of the chain, and probably no later than 1939, any but inspirational connection with Walras [68] is absent; so I regard the word “neowalrasian” as an *impersonal* noun that requires no capitalization.

can enrich understanding, clarify basic concepts, uncover contradictions, eliminate ambiguities, and reveal hidden assumptions. These advantages are uniformly well-confirmed for hypothetico-deductive disciplines in which the concept of truth is formal. In these disciplines we do not have to deal with questions of truth [29, 264–65]:

The axioms serve as hypotheses or assumptions, which are entertained, considered, explored just for the purpose of discovering what other propositions they imply. [. . .] . . . the formal validity of any mathematical system expressible in the form of a complex hypothetical proposition can be assessed without any reference to the truth or falsity of either axioms or theorems.

But for disciplines rooted in plausible reasoning, truth is of paramount importance; the aim is not to state and prove theorems but to persuade others that proffered empirical conjectures are sufficiently plausible to deserve closer scrutiny.

The two types of reasoning—the first involving demonstrative, the second shaded inference—are extensively discussed in Polya’s *Mathematics and Plausible Reasoning* [52]; but perhaps the simplest way to think about them is to adopt terminology proposed by Synge [63, 16–17] in an informal book about general relativity theory. There Synge used the term R-World to refer to “. . . the real world . . . of immense complexity in which we live and move and have our being,” and coined the term M-World, “. . . with M standing for model or mathematical. . . .” to refer to conceptual worlds invented by theorists. Synge’s distinction is ancient, going back at least to Plato’s story of cave-dwelling prisoners who, seeing only shadows cast by R-World objects, would base their ideas about reality on shadow images [51, 312–13].

As a personal matter, I have long believed that in dealing with M-Worlds, axiomatization is useful as well as safe [9, 4–8, 105–12, 143–46] and I have not changed my mind, even though my faith in formalization has been sorely tested by re-reading (i) Polya [52] on “Induction in Solid Geometry,” (ii) Lakatos [40] on “Proofs and Refutations,” (iii) N. Bourbaki² [8, 296–346] on the history of set theory, (iv) Debreu [19] on “Theoretic Models,” (v) McCloskey [45], Katzner [37], Leamer [41] and Solow [58] on “Formalization in Economics,” and (vi) a monograph on “Formalization” by Velupillai [67]. My opinion continues to be that axiomatics, like every other tool of science, is no better than its user, and not all users are skilled. Subject to that caveat, I stand by my initial claim: axiomatics is a valuable tool for taming M-World models.

When I consider intellectual analysis of R-World phenomena, further elaboration is in order. Here I am reminded of a failed attempt by Woodger to axiomatize embryology [72] that Slobodkin apparently had in mind when he remarked [57, 96]: “. . . formalism never worked well in biology.” Next, I am reminded of a failed attempt by John von Neumann to axiomatize Feynman’s quantum electrodynamics [29, 174]; [67, 48–50]. In his 1964 Messenger Lectures at Cornell, Feynman argued [24, 55–56]:

Mathematicians are only dealing with the structure of reasoning, and they do not really care what they are talking about. [. . .] You state the axioms, such-and-such is so, and such-and-such is so. What then? The logic can be carried out without knowing what such-and-such words mean. If the statements about the axioms are carefully formulated and complete enough, it is not necessary for the man who is doing the reasoning to have any knowledge of the meaning of the words [. . .] In other words, mathematicians prepare abstract reasoning, ready to be used if you have a set of axioms about the real world. But the physicist has meaning to all his phrases. That is a

2. Nicolas Bourbaki is the pseudonym of a consortium of mathematicians (described by Boyer [7, 674] as “non-existent” and “polycephalic”) that have produced a dozen or so volumes of formalist mathematics since 1939 under the title *Éléments de Mathématique* [70, 249–56; 17, 103].

very important thing that people who come to physics by way of mathematics do not appreciate. Physics is not mathematics, and mathematics is not physics. One helps the other. But in physics you have to have an understanding of the connection of words with the real world. It is necessary at the end to translate what you have figured out into English, into the world. . . . Only in that way can you find out if the consequences are true. This is a problem which is not a problem of mathematics at all.

In physics, axiomatization seems to have been usefully carried out only in long-settled areas of mechanics [60, 291–304]. In the social sciences, instructive examples of mathematical modeling, including a nice one in economics, may be found in Kemeny and Snell [38]; for a short list of uses of axiomatic method in economics, see Hildenbrand [34, 3–4]. It would be easy to continue in this vein, but without going into detail, we would learn little that is not already obvious: namely, every empirical science provides ample material to construct intellectually provocative models of myriad M-Worlds. Such models, whether in sociology, biology, physics, economics, meteorology, ecology, or evolutionary genetics, must be judged primarily on the basis of logical consistency and only secondarily on the basis of consilience with R-World evidence [64, 164; 42, 34]. Bearing in mind that “reality” never matches what we “cave prisoners” might judge it to be, any other attitude would be absurd. It is not meaningful to ask of a formal model whether it is true or false, only whether it is more or less useful than another model for a particular purpose.³ All M-World theories are *wrong* because none exactly describes the R-World, but some M-World theories are wrong in more interesting ways than others. More to the point, a good theory can never be harmed and may be helped by formalization; so by and large I favor axiomatics in disciplines rooted in plausible (inductive) as well as demonstrative (deductive) reasoning. All the same, let me end this section with a cautionary word from Will Baumol [4, 380]:

Axiomatization is one of the mathematician's very powerful and fruitful methods. [. . .] However, it must be recognized that while mathematical statements are always explicit, they are often not transparent. The literature abounds with axioms whose meaning is in dispute or which turn out to mean something other than what their author intended. [. . .] None of this is meant as a criticism of the axiomatic method. It amounts only to the trite injunction that powerful weapons should be used with . . . caution.

III. The Pygmalion Syndrome

We come next to a central issue concerning axiomatics and economics: How do we decide whether a particular M-World model deserves to be taken seriously by economists interested in subject-matter problems? This is a tangled issue [69, 107–112]. Let us start with Plato's account of cave-dwelling prisoners [51] who know nothing beyond what they can learn by viewing shadow images of real objects. At the end of his story, Plato adumbrates a phenomenon that Syngé [63, 18] calls the *Pygmalion Syndrome* after the legendary sculptor who carved a statue of such surpassing realism that Venus gave it life [28, 145–47]. The Pygmalion Syndrome may be described as the tendency of theorists in empirical sciences to confuse the R-World of experience with M-Worlds of their own imagination. On the whole, Syngé [63, 17] thinks this is a good thing for physicists. On the whole, I think it is a good thing for economists also, because the Pygmalion Syndrome

3. Perhaps the best account of these issues is to be found in the introductory chapter of Feller's *Probability Theory and Applications* [23, 2–3]).

helps keep them focussed on subject matter. But there are more substantial reasons that I shall address later. Here, it is more purposeful to highlight a comment by John Hicks [31, 23]:

Pure economics has a remarkable way of producing rabbits out of a hat—apparently *a priori* propositions which apparently refer to reality. It is fascinating to try to discover how the rabbits got in; for those of us who do not believe in magic must be convinced that they got in somehow.

Hicks speculates briefly about probable sources of factitious “rabbits” and then goes on to other matters. I have no quarrel with Hicks’s speculations [32, 367–68], but my own feeling is that most of the “rabbits” found in economic theory derive from Pygmalion Syndrome. As supporting evidence I call to mind the ubiquitous use of terms from everyday language to designate ideas which, in economic theory, often connote subtle and ambiguous concepts, e.g., *market*, *firm*, *rational*, *competitive*, *price*, *optimal*, *efficient*, *equilibrium*.

As already intimated, I think it makes sense to treat Pygmalion Syndrome as a blessing, though I suspect my reasons for so thinking may incline others to view it as a curse. My grounds for considering it a blessing are indirect; they derive from the fact that, like the discipline of rational hydrodynamics that I described at length in my 1993 address [13, 808–89], economic theory is an indiscriminate mixture of plausible argument from casual empirical hypotheses and formal reasoning from “first principles” of utility and profit maximization and all that. As Birkhoff demonstrates in his study of hydrodynamic theory [5], this kind of mixture—when modelled in a formal way—leads naturally to scientific paradoxes: apparent inconsistencies between theoretical results and conclusions suggested by fact or common sense [5, 4; 62, 16–17; 20, 337ff]. The emergence of such paradoxes is more often helpful than hurtful, because it opens the door to immanent criticism of model assumptions; scientific paradoxes—*alias* Hicksian “rabbits”—are of interest because their solution commonly involves the application of stricter standards of logical rigor [5, 3–5, 177–78; 42, 49].

Someone once said “It is better to be vaguely right than precisely wrong.” Perhaps so, as long as we are speaking vaguely (as a practical matter, economic theory, like creative thinking in any science, is often vague because it is informal and tentative). If we are speaking formally, however, then to be vaguely right is to be precisely wrong [58]. Indeed, it is this feature of *formal* models that gives them their *raison d’être*. If a provocative formal model contains an anomaly, it is unlikely to remain disguised for long, and once found it will be converted into a scientific paradox [12, 62–63, 71–75]. At some stage, stricter rigor aimed at resolving the paradox may direct attention to previously overlooked weaknesses in the underlying model, which may suggest new avenues of approach to familiar problems. Such must be our hope, for it is evident that no empirical science ever has been generated by axiomatic thinking. One has only to mention Copernicus, Galileo, Newton, and Einstein to see the absurdity of a contrary view. So unless axiomatics can be made to play a *critical* as contrasted with a *constructive* role, it is likely to be as little use to an empirical scientist as a broken saw to a carpenter. I suspect most economists regard paradoxes as foibles of our discipline; but because scientific paradoxes emerge only from theories that are not entirely devoid of empirical content, the thing to be regretted in contemporary economics is not the plenty but the paucity of paradoxes.

IV. Prophylactic Interlude

Subsequent discussion of Debreu [17] will be facilitated if we pause briefly to inoculate ourselves against unsuspected Pygmalion Syndrome infection. Debreu tells us in his preface [17, viii]:

The theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics [referring, presumably, to the “school” of Bourbaki]. [. . .] Allegiance to rigor dictates the axiomatic form of analysis where the theory, in its strict sense, is logically entirely disconnected from its interpretations. In order to bring out fully this disconnectedness, all the definitions, all the hypotheses . . . are distinguished by italics; moreover, the transition from the informal discussion of interpretations to the formal construction of the theory is often marked by one of the expressions: “in the language of the theory,” “for the sake of the theory,” “formally.” Such a dichotomy reveals all the assumptions and the logical structure of the analysis.

This makes axiomatics sound neat, clinical, and straightforward. How does it work out in practice?

Chapter 1 (“Mathematics”) is what we would expect: straightforward and clear. Chapter 2 (“Commodities and Prices”) is less transparent; at [17, 28] for example we are told “No theory of money is offered here, and it is assumed that the economy works without the help of a good serving as medium of exchange; then just five pages later [17, 33] we are asked to “Imagine that a certain good circulates as money. . . .” (shades of J-B. Say [56, 76–77]?). Since neither of the quoted passages is italicized, the apparent glitch may be overlooked. So we go on to “Dates and Locations” [17, 29]. Here we learn that “The interval of time over which economic activity takes place is divided into a finite number of compact *elementary intervals* of equal length. [. . .] Their common⁴ length . . . is small enough for all instants of an elementary interval to be indistinguishable from the point of view of the analysis. An elementary interval will be called a *date*. . . .” This passage contains one italicized phrase and one italicized word. Is it all part of the formal theory? Taking no chances, let us introduce an unusual word as a synonym for “elementary interval” and “date.” For the sake of the theory, call them both *oblivion*. For some reason, I find it difficult to make sense of the phrase “finite number of compact oblivia of equal length”; so my inclination is to forget about different dates and focus attention on economic activity that takes place during a single oblivion of indeterminate duration. This procedure makes later paragraphs [17, 33–35] on “Interest, Discount, and Exchange” curious reading (e.g., “Let t^1, t^2 be two oblivia such that $t^1 < t^2$ ”); but if theory and interpretation are “entirely disconnected,” we can let that pass. Chapter 7 on “Uncertainty” is perhaps a harder pill to swallow, but as a formal matter an oblivion, a flea, and an elephant may all be considered equally compact, divisible, and small.

It would be repetitious to cover the same ground again in reference to Debreu’s description of *elementary region* and *location*. For the sake of rigor, I replace both terms with the single word *limbo*. This procedure makes the concept of *nations* [17, 35] seem out of place, but what will be, will be. So let us backtrack to the notions of “commodity” and “price” [17, 34] where Debreu tells us:

A commodity is characterized by its physical properties, the date at which it will be available, and the location at which it will be available. The price of a commodity is the amount [Amount of what?] which has to be paid [Paid in what form, how, and to whom?] . . . for . . . one unit. . . .”

This is mysterious stuff, but it is mild by comparison with the statement in the next paragraph about price, debits and credits, etc.

I propose to cut through this thicket of misdirection by dropping the word “price” entirely, replacing it with the technical term *weight coefficient*, a real number u_i that I express in units of

4. Here Debreu informally introduces a powerful tacit assumption (standard in virtually all discrete-time economic analysis) that economic activities are perfectly synchronized (conducted in lock-step). This assumption is essential to formal analysis involving addition of vectors that are supposed to measure commodity quantities. Unless the time dimension of different “agents” is the same, such sums have no sensible interpretation [50, 866].

an imaginary unit, the *uff*,⁵ which is unlikely to be associated intuitively with any customary notion of “money” or “money price.” Debreu introduces a “price” vector that I replace by the *uff* vector, $u = (u^1, \dots, u^h, \dots, u^l)$. For linguistic convenience, I call the inner product of the *uff* vector and any row vector *uffage*. I adopt this procedure not as a whim, but to provide a “flag” (as in a DOS DEBUG procedure) to warn of possible interpretation error. Such flags would be redundant if rigorous theory were a practical possibility in a world afflicted with Pygmalion Syndrome; but we have to take the world not as it might be but as it is.

Early in Chapter 2 Debreu [17, 32] describes his universe of discourse informally:

It is assumed that there is only a finite⁶ number l of distinguishable commodities . . . indicated by an index h running from 1 to l . [. . .] . . . the quantity of any one of them can be any real number. [. . .] The space R^l will be called the *commodity space*. For any economic *agent* a complete plan of action . . . or more briefly an *action*, is . . . a complete listing of his inputs and of his outputs. With [an appropriate sign convention] an action is therefore represented by a point a of R^l .

So ([17, 35] “in the language of the theory” (after relevant substitutions):

*The number l of commodities is a given positive integer. An action a of an agent is a point of R^l , the commodity space. An *uff* vector $u = (u^1, \dots, u^h, \dots, u^l)$ is a point of R^l . The *uffage* of an action relative to a vector u is the inner product $u \cdot a$.*

In a related vein, I end the present section by recalling anomalies that were mentioned in [13, 808]. In the language of Debreu’s formal theory: There are *excess demands* [17, 80], but **there are no trades** [3, 324; 26, 309; 59, 16]; there is a *price system* [17, 33], but **there are no markets** [26, 52, 113]; there are *agents* and *actions* [17, 32], but **no events are observable** [3, 340]; there are *shares* [17, 78–79; 2, 270–71] in production, but **production does not occur** [3, 282]. I have been told that these and other apparent “anomalies” in neowalrasian theory are “just a matter of semantics.” I do not disagree; but I am bound to reflect that scientific theory is concerned with little else.

Debreu’s pioneering axiomatization of value theory is deservedly respected by those who know it;⁷ still the question arises: on balance, has Debreu’s *Theory of Value* helped or hindered the development of economics as an inductive science [53, 34–37; 26, 309]? Merely to pose this question is, according to some, tantamount to disparagement. Those who think thusly should make themselves familiar with the niceties of formal axiomatics [60, 247–49; 61, 273–74], and should also become acquainted with Syngé’s reason for not attempting a formal treatment of special relativity theory [64, 4]:

Anyone who tries to put a physical theory on an impeccable axiomatic basis soon realises that he has undertaken a major task, absorbing all his energy and leaving none for the body of the

5. I extract this term from Lewis Carroll’s nonsense rhyme “Jabberwocky” [10, 180]: “And, as in uffish thought he stood, The Jabberwock, with eyes of flame, Came whiffing through the tulgey wood, And burbled as it came!”

6. In a 1973 lecture [26], Frank Hahn appears to confuse “finite” with “small.” Otherwise, how can he discern an “empirical confrontation” [26, 52–3] between Arrow-Debreu and “the facts” only in an uncertainty model where “markets exist at every date”? The product of two finite numbers is finite. Suppose in a certainty model the number of commodities is 10^{17} . I doubt if the world will contain that many markets from the Big Bang to the Final Implosion. So what? The world will probably contain even fewer *uffs* during the same stretch of time. If books are to be written about “missing markets” (e.g., Hahn [27]) on the basis of Hahn’s “empirical confrontation,” I propose a volume or two on “missing” *uffs*!

7. For those who want to know it without drowning in it, I suggest Hildenbrand [34] or Debreu [18]. For paddlers in deeper water, Koopmans [39] is good reading, and Weintraub [69] is masterful. For a truly beautiful account by a master expositor, one should read Mas-Colell [44].

theory in which his real interest lies. The axiomatisation of physics . . . is a job for the specialist in axiomatics.

As Samuel Johnson is said to have remarked about a dog that played chess poorly, “The wonder is not that it is done badly, but that it is done at all.”

V. Axiomatics in the Neowalrasian Mode

Let us deal next with Debreu’s “axiomatic analysis of economic equilibrium.” Debreu’s stated objective [17, vii] is to explain:

. . . the prices of commodities resulting from the interaction of agents of a private ownership economy through markets. . . .

This statement of purpose is much like the analogous statement in Hicks’s *Value and Capital* [31, 7]:

This is a work on Theoretical Economics, considered as the logical analysis of an economic system of free enterprise. . . .

In both cases, we expect attention to focus on the “interrelation of markets,” and on trading phenomena generally; and that is what we find in Hicks. As for Debreu, let me quote Punzo [53, 36]:

The neo-Walrasian programme suggests a specific understanding of a model [vide Debreu Ch.2] as a plausible description of an actual economy, which is based on the extension to economics of the purely logical criterion of consistency of formal, axiomatic systems. As equilibrium and consistency are coextensive concepts, the necessity of providing explicit existence proofs derives from a specific use of ‘models’; but in ‘applied sciences’ or in a ‘discipline’ [33, 371–75], the content of such a proof is either negative (it tells us which models are useless) or inconclusive. When the latter is the case, as it often is, we do not know what to do. . . . [. . .] In this respect, *Value and Capital* and the general equilibrium literature do not seem to do the same sorts of things. In general equilibrium models are essentially conceived and exploited as logical constructions; to Hicks, models provide laboratories for testing concepts. The underlying reason for the absence of discussion of the existence issue in *Value and Capital* is . . . Hicks’s own conception of economic theory and of the role (and heuristic limits) of formal modelling. *Value and Capital* belongs to . . . the set of statements intended to interpret reality: therefore its validation rests upon criteria which are not logical in the sense required by formal logic.⁸

The whole of Punzo’s comment applies with full force if for Hicks’s *Value and Capital* we substitute Walras’s *Éléments* (though, to be sure, Walras’s practice [36, 71] and his preaching [36, 46] were not always consistent). However that may be, Punzo makes it clear that Debreu’s concern is with an M-World, not with the R-World. What was in Debreu’s mind is neither here nor there; from internal evidence in the *Theory of Value* I conclude that the “world” Debreu intended to model was his conception of the neoclassical economy portrayed in Hicks’s *Value and Capital*, which derives from Hicks’s conception of the “no arbitrage model” that Walras introduced at the end of Lesson 11 of the *Éléments* [46, 501]. We should not be surprised then to discover

8. Hicks says [33, 374–5]: “Existence, from my point of view, was a part of the hypothesis; I was asking, if such a system existed, how would it work?”

that Debreu's analysis bears little resemblance to anything in Walras, nor to find that its logical structure is that of *Value and Capital*.

As indicated earlier, Debreu postulates just two primitive economic concepts (concepts not described by more basic terms or by previously defined concepts): "agent" and "commodity." But Debreu's agents appear to be placeholders for "plans" or "thoughts" fathered by the wishes of a single "principal" that I shall denote by T and call the "theorist" (the terms "auctioneer," "secretary of the market," "coordinator," "umpire," and "demon," all found in pre-Debreuvian literature, appear to designate the same notion). What Debreu calls "agents," I would (in my own work) call trans[actors] to suggest *active* rather than *passive* participation in economic events. But because I am presently concerned with Debreu's model, I shall confine my discussion to his universe of discourse, A , which consists of a collection of sets called *actions*. To conform with Debreu's practice, I partition the set A into two disjoint subsets, a "producer" subset ${}^PA = \{y_1, \dots, y_q\}$, and a "household" subset ${}^HA = \{x_1, \dots, x_q\}$. (Debreu uses two index sets, "producers" being indexed from 1 to n , consumers from 1 to m ; but since any or all of the elements of the "producer" subset may be zero ("inactive") no point of substance is involved in supposing that the producer and consumer subsets have the same number of elements, namely q). In the language of Debreu's theory, actions are points of a Euclidean vector space, so our universe of discourse is just $A = \{x_1, \dots, x_q; y_1, \dots, y_q\}$. Now, the only action explicitly described by Debreu is indicated by the term *chooses* [17, 39, 43, 62, 65] which is part of his formal theory but is nowhere defined (cf. the comment in Bourbaki [9, 312] about the concept of "between" in Euclid). To plug this gap, I define *choose* as T selects for private contemplation, to reflect the fact that the plans of Debreuvian agents are simply the wishes of T. Accordingly, where Debreu writes, e.g., "the producer (consumer) chooses," I write (and mentally translate) "T selects." This procedure has the great advantage of exploding the neowalrasian pretense that Arrow-Debreu theory refers to the "decentralized actions of independent producers and consumers." In truth, there is just *one* decision unit, T, *there are no independent agents*, and the notion that the analysis deals with anything "decentralized" is an abuse of language. I conjecture that this pretense has been responsible for the almost universal delusion (shared even by such a careful scholar as Weintraub [69, 106–107] that the numbers discussed in neowalrasian theory have counterparts in R-World measures of production, sales, and price.

Preliminaries over, let us consider the "producer" actions ${}^PA = \{y_1, \dots, y_q\}$. Debreu introduces various assumptions [17, 39–42] to describe the *production set*, Y_i , of the i th action in PA . The action $y_i \in R^1 \in Y_i$ is referred to as (net) production or *supply* [17, 38] of the i th "producer." In much the same way, "actions" in the "consumer" subset ${}^HA = \{x_1, \dots, x_q\}$ are elements of a *consumption set* X_i ; the action $x_i \in R^1 \in X_i$ is called (net) consumption or *demand* [17, 51] of the i th "consumer." While we are on the subject, let us follow Debreu and assume that with each element of HA , there is associated an *endowment* vector $\omega_i = (\omega_{ij})$ [17, 78] representing *a priori given* quantities of commodities which we will suppose come from a meteor shower [43, 415]. Then we may define *excess demand* of the i th "consumer-producer"⁹ by $z_i = x_i - y_i - \omega_i$.

Debreu's first economic axiom [17, 43] is:

E1: *Given Y_i and the vector u , y_i is selected to maximize the inner product $u \cdot y_i$.*

I call this axiom "economic" because, apart from its role in later definition [44] of the *supply correspondence*, the only motivation for the phrase "maximize the inner product [*uffage*] $u \cdot Y_i$ "

9. This is a transposition of the term coined by Graaff [25] to describe his integrated account of the textually separated treatment in *Value and Capital*. See also Clower [14].

seems to be conventional dogma (specifically Hicks [31, 79]). Having earlier inoculated ourselves against Pygmalion Syndrome by choosing to work with *uff coefficients* rather than prices, the axiom EI should strike a sour note. If *uffage* were “money profit,” no economist would take a second glance at EI before passing on. But *uffage* is not money profit; in the language of the theory, *uffage* is a fiction, plain and simple. So how are we supposed to make sense of EI within the language of the formal theory?

Let us start by asking, Why do we deal with producer actions in the first place? I submit that we do so not because the subject is relevant to a logical theory of exchange value (consider Edgeworth’s “manna” model of prices [22, 277], Marshall’s “meteoric stones” model of rent [43, 415], or Patinkin’s manna endowment model in *Money, Interest, and Prices* [49, 8]); we do so because Walras, Fisher, Marshall and other founders of “neoclassical economic theory”—and more recently Joan Robinson, Edward Chamberlin, Hicks and Samuelson—taught us to do so. Since all these economists lived, breathed, and worked in capitalist money economies, they naturally viewed “the price system” as a mechanism that guides decentralized suppliers to make production choices. But the *uff vector* is not a “system.” It is arbitrary. If we merely want to define supply functions, without regard to possible economic interpretation, why not replace u in EI by the unit row vector? So we find ourselves with a paradox: we need EI as a way-station to “supply”; but as presently stated EI might be rephrased in many intuitively senseless ways, all consistent with other elements of Debreu’s formal theory.

Fortunately the paradox is readily resolved by deeper analysis. By hypothesis, T conducts a thought experiment to select y_i . It is natural to hypothesize further that the choosing is not to be done arbitrarily, but rather that the thought experiment is to proceed *as if*¹⁰ T were a “producer” guided by coherent thought. In this situation, I suggest what is needed is a “coherent decentralization” procedure like that proposed long ago by Charnes and Cooper to help a U.S. manufacturing firm ensure that its independent divisions conformed to company goals [11, 294–95 and 305fn.]. Accordingly, I introduce a second economic axiom:

EII: *If y_i satisfies EI, and if u^* defines a state of mutually consistent actions, $\sum x_i - \sum y_i - \sum \omega_i = 0$,*¹¹ *then $y_i = y_i^*$; otherwise $y_i = 0$.*

EII requires that T select y_i as if the action contemplated will occur exactly as contemplated or will be voided. On this assumption, the *uff vector* may be treated coherently as indicating to T real rates of exchange of outputs for inputs: i.e., u serves as a surrogate “price system.” By this procedure, we obtain a motivation within the formal theory for *uffage* maximization. There remains one reservation: can T, acting in the role of vicarious producer, reasonably be presumed to “choose” as if EII were credible? In general surely not, unless T is assumed to act as if there existed a mystical logistical presence (an “invisible hand” or a Hicksian or Patinkinesque “automatically equilibrating market” [31, 120; 49, 14]) that ensures fulfilment of mutually consistent plans [48, 9–12]. I won’t make this supposition a separate axiom here, although I believe it ought to be so treated in the Debreu model. As an alternative (to make explicit the basis for regarding EII as credible), I shall later call an axiom similar to EII the *Beggar’s Fantasy Axiom* (The “beggars fantasy” refers to a Scottish adage: “If wishes were horses, beggars might ride”). Whatever we call EII, our analysis reveals that, if Debreu’s formal argument is to make economic sense, it must be presumed to contain an implicit assumption such as EII. As it stands, Debreu’s profit

10. The italicized phrase is Jaffé’s translation [36, 169] of “*comme cela*,” which is used by Walras *exactement* as it is used by contemporary devotees of “as if” methodology.

11. This corresponds to Debreu’s concept [17, 76] of *market equilibrium*.

maximization assumption is a Pygmalion Syndrome import from the R-World of market exchange; it is correspondingly at odds with Debreu's formal theory.¹²

Let us deal next with consumer actions ${}^H A = \{x_1, \dots, x_q\}$. Here Debreu devotes major attention [17, 50–61] to set-theoretic definition of preference sets and utility functions. This is relatively familiar material [71, 81–97]; 9, 102–16]; 47, 7–40); but Debreu adds numerous frills. For my purposes, the ordering, completeness, closedness and connectedness assumptions [17, 56, 72] that define a continuous utility function on X_i , here denoted $V_i(x_i)$, will suffice. The crucial assumption in Debreu's theory of the "consumer" appears at p. 62 in the form of the so-called *wealth constraint*. This constraint is motivated by the assertion that "The expenditure $p \cdot x_i$ must clearly be at most equal to the *wealth* of the i th consumer, a real number w_i ." Like many remarks that contain the word "clearly," the truth of this one is questionable; in fact it makes as little sense in the language of Debreu's formal theory as does the analogous *uffage* expression in EI—and for the same reason.

Since I effectively suppressed Debreu's partition of the action set A earlier by rewriting it as $A = \{x_1, \dots, x_q; y_1, \dots, y_q\}$, I now offer a unified consumer-producer [25; 14] version of Debreu's producer and consumer theories in the form of two axioms:

I. SELECTION AXIOM. For any *uffage* vector u , given $V_i(x_i)$ in X_i and y_i in Y_i , T selects x_i and y_i to satisfy $u \cdot z_i \leq 0$ and maximize V_i .

II. BEGGAR'S FANTASY AXIOM. If x_i and y_i satisfy I, and if u_i^* defines a state of zero excess demand, $\sum z_i^* = 0$, then $x_i = x_i^*$ and $y_i = y_i^*$; otherwise $x_i = y_i = 0$.

One might draw various inferences from this restatement of Debreu's theory, but here I mention only two. The restatement suggests that Debreu's theory of production is an expository artifact rather than part of the logical structure of his theory [1, 273–99]. A similar comment applies to the Arrow-Debreu concept of *shares* [2, 270–71; 17, 78–79].

VI. Conclusion

I end with a few ruminations, starting from the story of "The Emperor's New Clothes." When straight talk comes from a child, it is called "telling it like it is." When it comes from an ageing academic, it is more likely to be called "sardonic," "iconoclastic," or "curmudgeonly." Be that as it may, it is time someone "told it like it is" for Arrow-Debreu general equilibrium analysis.

If we ignore the disingenuous "statement of purpose" in Debreu's "Preface" [17, vii] and look instead at the content of the book, it is obvious that its only aim is the same as that of Debreu's earlier joint paper with Arrow [2]: to present a more general and rigorous proof of the existence of a competitive equilibrium [46, 501]. Although as a general rule, I approve of efforts to generalize proofs of any proposition, the value of this particular venture (more accurately, series of ventures, as McKenzie's *Palgrave* essay [46] makes clear) seems dubious. As Mas-Collel has remarked [44, 175]:

. . . the originators of equilibrium theory (L. Walras, V. Pareto, F. Edgeworth, and many others) sought to verify the determinate character of the theory by applying a counting of equations and unknowns test. . . . This was good heuristics. . . .

12. Debreu's concluding italicized paragraph in section 5.4 [17, 76] may seem similar to the Beggars Fantasy Axiom; in any case, it appears too late to "save" the coherence of the formal theory.

Considering the mathematical tools available in the last quarter of the 19th Century, Mas-Colell understates the case (cf. Hicks [33, 278–9, 374–75]). It seems to me that, once we had the writings of Walras [68] and Edgeworth [21] in hand, the production of rigorous proofs of competitive equilibrium was purposeless. As Hicks observed [33, 279], existence proofs of varied kinds understandably will seem important “. . . for those who are concerned with the defense of ‘capitalism,’ to show the possibility of an arm’s length equilibrium (an ‘Invisible Hand’). . . .” This may or may not explain why such writers as Koopmans [39, 41–53], Samuelson [55, 469–70], and Arrow and Hahn [3] devoted so much effort to establish conditions under which perfect meshing of individual economic activities can be achieved through “the working of the Invisible Hand,” “the market,” the “price system” [15]), and other metaphorical prodigies. Whatever the explanation, it strikes me as a great waste of intellectual talent. The most rigorous neowalrasian existence proof shows only that a competitive equilibrium “exists,” not how such an equilibrium might be “found.” A mathematical analogy is apposite here. We know from the fundamental theorem of algebra that every algebraic equation has solutions in the field of complex numbers [16, 101, 269]; but the theorem does not tell us how solutions known to exist may be located, much less that such solutions can be calculated.

Misconceptions among economists about these matters are made more pardonable¹³ by the meretriciousness of the economist’s notion of “equilibrium.” In every branch of physical science, “equilibrium” refers to a “balance of forces” [3, 1; 54, 5] such as might be associated with an olive resting at the bottom of a cone-shaped martini glass; and the word misleadingly conjures up analogous images when it is used by economic theorists. Strictly speaking, however, the “equilibria” that neowalrasian theory shows to exist are more correctly called *solutions* [to a system of implicitly-defined algebraic equations]. So understood, the important achievements of neowalrasian equilibrium theory lose much of their apparent lustre, which should in any case adhere to the mathematical geniuses Gauss and Brouwer whose work underlies all modern existence proofs. I do not in any way mean to denigrate the intellectual excellence of neowalrasian proofs of competitive equilibrium; I intend only to suggest that this work is more accurately categorized as set-theoretic logic than economics.

I have the impression that Arrow-Debreu theory has played a direct role only in the writings of a select group of world-class mathematical economists (Mas-Colell being a name that springs instantly to mind); its main impact has been a consequence of the position it has come to hold (for good reasons or bad) as an “authority symbol” [65, 119–21]. In that respect, neowalrasian theory has played an important role in saddling the economics profession with the *neowalrasian code* [35; 12, 806], a development I judge analogous to “albatrossing” in Coleridge’s “Rime of the Ancient Mariner.”

I conclude on an optimistic note. As I hope I have already made clear, I find no logical flaw in any aspect of Arrow-Debreu theory; I argue, however, that as a foundation for applied economics, Arrow-Debreu theory is empirically vacuous and conceptually incoherent. I know there

13. Some economists are less forgiving than others. For example, Velupillai, in the synopsis of his Ryde Lectures [66, 5], remarks “‘Economic Theory in the Mathematical Mode’ (*pace* Debreu [18]) is replete with undecidabilities, uncomputabilities and incompleteness results—and the primitive ontology that goes with this ignorance. Yet with princely unconcern, economic theorists have been developing computable general equilibrium (CGE) models, recursive methods in economic dynamics, computing equilibria and theorizing about learning processes that converge to rational(sic!) expectations equilibria (REE). The CGE models are *not* computable; the so-called recursive methods . . . are, in general, not well founded in recursion theory; in computing uncomputable equilibria absurdities are compounded by conflating constructive methods with effective processes. . . .” Velupillai calls this a *scandal* in the sense in which that term is used by Bishop [6, 3].

are economists who count rigor more important than conceptual coherence [70, 268]. To them I say *De gustabus*. . . . As far as I am concerned, those who like neowalrasian theory are free to like and use it as they will; its logical consistency is not in doubt. Those who prefer conceptually coherent models of the R-World, however, I hope will join me in searching for better ways to advance economic science.

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