In Defense of Formalization in Economics

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These days, it seems, a lot of people are dissatisfied with a lot of economic analysis. Numerous articles and books have been written detailing complaints. Numerous sessions at conventions take up the issue. One often sees and hears words such as “malaise,” “disarray,” and “crisis” applied to describe the current state of affairs. In perhaps the strongest rebuke of received economic theory, yet, one of its founding fathers, none other than J.R. Hicks [8, p. 365], repudiated his own work by altering his identity: “Clearly I need to change my name,” he wrote in 1975. “Let it be understood that Value and Capital (1939) was the work of J. R. Hicks, a ‘neoclassical’ economist now deceased; Capital and Time (1973) – and A Theory of Economic History (1969) – are the works of John Hicks, a non-neoclassic who is quite disrespectful toward his ‘uncle.’” Much of Hicks’ subsequently published work appears under the newly-assumed name of its author.

On the list of objectionable facets of economic analysis singled out for special complaint, the formalization of economic reasoning is often at or near the top. Witness, for example, Prest’s advice [28, p. 131] to the aspiring graduate student of economics: “The suggested Ph.D. thesis title ‘n solutions to n – 1 problems’ is a very promising theme for an academic career devoted to High Theory. Nowadays this is the super-highway to eminence and professional acclaim. And you will very quickly discover that your standing and stature will rise in geometrical proportion to the irrelevance and obscurity of what you say and write. Abraham Lincoln was wrong here: “you can fool all of the people all of the time – without much effort, either.” I want to argue in this paper, however, that formalization per se is not the problem, and that the real difficulty lies in the nature of the questions that economists tend to ask and in the assumption-content of the analyses constructed to provide the answers. Indeed, formalization is, for many of us, a powerful and appropriate means of analytical expression whose avoidance would only weaken our incisiveness and create more problems than its non-use would resolve. Even the argument of Prest’s advice relies on a formalization that relates “standing and stature” proportionally to spoken or written “irrelevance and obscurity.”

It should be noted at the outset that although it certainly has contact-points with both areas, what follows is neither an essay on methodology or epistemology. There are a variety of methodologies and epistemologies in economics and most make room for formalization in analysis, using and interpreting their analytical structures in a variety of ways. However, the present discussion accepts the dominant methodology and epistemology in economics today which is based on a separation or dichotomy between reality and thoughts about reality and which, at least epistemologically, results in rationalism or empiricism or some combination thereof. Also accepted are the currently existing standards of scholarly discourse that have emerged from this background. Thus the issues considered here have not so much to do with the general implications of methodology and epistemology for formalization and vice versa, but are concerned, rather, with certain kinds of things that can be said of formalization given the methodology and epistemology employed.

Let me begin by defining the concept of formalization and discussing its properties, its purpose, and what it does.

The aim of analysis is to explain or clarify worldly phenomena. Analysis proceeds by organizing and exploring the thoughts one has about those phenomena. The explanations that are achieved may or may not carry with them an ability to predict. In economics there are a number of routes such organizations and explorations may take. To analyze by example requires the examination of highly detailed specific instances or case studies to illustrate the phenomena under consideration. Another form of analysis, storytelling, is the giving of a narrative account of the phenomena in which “... fact, theory, and values are all mixed together in the telling.” But the most common
method of analysis in economics involves the creation and study of models. A model of something – call the thing T – is a construct having enough in common with the observable facets of T that insight into T can be obtained by studying the model. Albert Einstein and Leopold Infeld [5, p. 33] gave the following illustration for physical models: Imagine you are presented with a watch and asked to explain how it works, but are not allowed to remove its cover. One way to proceed is to obtain appropriate springs, gears, and what not, and build a “model” of the watch whose behavior duplicates the observed behavior of the original. You could then give an explanation of how your model behaves, and say that the original watch works analogously to, or as if it were, your model. Clearly, there are many different models, and hence explanations, that could be built. But all explanations function by identifying something in the model (here, the movement of the model’s “hands”) with what is observed (the movement of the hands on the original watch). In economics, of course, models are usually not physical things. Rather they are mental constructs based on assumptions, abstract concepts, and relations among variables. But they function in much the same way as in the Einstein-Infeld example. Thus, in economics, model-building is different from description and from analysis by example in that it essays to provide an explanation of the thing being scrutinized; it is different from storytelling in that it focuses on relations among variables rather than on narrative. Note, however, that particular economic analyses may combine examples, storytelling, and model-building, as well as other forms of argument in their structures.

With these ideas in mind, formalization in economics may be defined as the development and analysis of relations among variables that constitute part (or all) of an economic model. The relations themselves may be, to a greater or lesser degree, “appropriate” or “relevant,” and may or may not identify or be associated with directions of causality thought to be inherent in the phenomena under investigation. Thus the purpose of formalization in economic analysis is to explain – some would add predict – observable economic phenomena. Formalization is neither description nor analysis by example or storytelling in the same way that model-building, as indicated above, is not. The term “formalization” is also used, on occasion, to denote the end result of the process of formalization just described, i.e., to indicate the relations themselves. The name “formalism” is given to the special case of formalization in which the properties of the relations, as well as the implications of these properties, are fully spelled out.

Four observations concerning this notion of formalization should be noted. First, it is implicit in the definition of formalization given above that there should exist, in formalizations that are meaningfully relevant to the economic reality under examination, acceptable accordances between the model of which the formalization is a part and the observed facets of the phenomena being explained. Although I will not attempt here to describe what constitutes “acceptable accordances” between a model and reality, most assuredly each economist has some idea of that which is acceptable to him, if not the acceptability standards of the economics profession at large. Furthermore, these accordances are crucial to the acceptability of both the model proposed as well as the formalizations it contains. Without such accordances, any analysis based on the model or any of its individual formalizations would be regarded as having little significance and relevance for the phenomena under investigation.

Second, formalization does not require that the variables under consideration be capable of measurement. In the absence of measurement, a variable’s values can be specified as distinct and discrete verbal descriptions. The variable itself is the thing that can take on as values any description in the appropriate collection of verbal descriptions. Relations among such unmeasured variables can be defined in set-theoretic terms and used in model-building in much the same way (except, of course, that numbers are not available) as relations among measured variables. Thus, without measurement, systems of simultaneous functional relations can be constructed and, under certain conditions, their solutions expressed as functions of parameter values. And in the same context, systems of dynamic, periodic, functional relations can be analyzed for their stability properties. A lack of ability to measure, then, is no barrier to the pursuit of formalization in economics.

Third, all formalizations that make a meaningful attempt to throw explanatory light on economic actuality are constructed through a
process of abstraction. Reality is sufficiently complex that it cannot all be included in the specification of a formalization. Things have to be left out. This is so in defining variables, quantitative or not, as well as in characterizing relations. Thus the contents of, or the results derived from, formalization are, at best, approximations of real or observed phenomena that are relevant only under “certain conditions” and cannot have universal applicability. Exceptions necessarily arise. To handle each significant exception requires either a separate formalization addressed specifically to the exception itself, or a generalization of the original formalization that gave birth to the exception. The former leads to “localism” in analysis; the latter to formalizations that assert less and less about the real-world phenomena in question.

Fourth, formalizations, like models, are analogies or metaphors. The distinction between analogy and metaphor is subtle; the following statement of their divergence will serve for present purposes. When modeling the observed behavior of a firm, for example, to say that the model is an analogy is to think of the firm as behaving “something like” that described in the model. But to take the position that the model is a metaphor means that the firm’s behavior is thought of as being described or captured, with a high degree of literalness, by the model’s behavior. The difference is that in the first case the model itself (apart from its variables and relations) is an approximate similarity or likeness; in the second it is an “exact” replication. But in either case, the formalizations involved liken observed reality to mental constructions in which the former is conceived of in terms of the latter. In this way those constructions serve as both instruments of thought and as devices for communicating in an intelligible manner. Their force lies in that they focus thoughts in precise ways. In that they provide standards for judging real-world behaviors, and in that they transfer the sense of one person’s vision to another.

As an example of formalization in economics, consider the often-criticized general-equilibrium model. This model focuses on the notion of equilibrium and, in the tradition of methodological individualism, is built up by making assumptions about the preferences, technologies, and behaviors of fictitious, individual agents. Its purpose is to explain and clarify the observed, simultaneous, interacting behavior of real agents in an economy. Having accepted the questions this model addresses, and having accepted the equilibrium approach and the assumptions upon which the answers provided by the model are to be based, the economist has no choice but to pursue the relevant formalizations and inquire into the existence, uniqueness and stability of equilibrium in the model. An easy way to see why this must be so is to focus attention on a single, real-world market in isolation. Imagine that one were to observe the market during a particular period of time. In that case, one could see that so much of the commodity was traded at such-and-such a price or, in other words, one would observe a single point in commodity-price space. Subsequent observation in the following period would yield a second point. In building a model to explain how these points came to be seen, the economist could assume (1) that there exist two distinct market demand curves each passing through one, but not the same, observed point, and (2) that there exists only one market supply curve passing through them both. Then, since each observed point is identified as a market equilibrium point in the model, each could be explained as analogous to, or as if it were, the outcome of the interaction of supply and demand. The economist could also assert that the movement from the first point to the second occurred because of a “shift” in demand. Clearly, equilibrium must exist in the model for this explanation to work. If, moreover, the equilibrium in the model were not unique, then the explanation would be incomplete; it would allow the observed point to be identified with many equilibria, each with its own properties, and thus the reason for the movement between the two points would become clouded. Finally, when the observed point changes from the old to the new, the equilibrium in the model would have to adjust accordingly. But if the latter equilibria were not stable, then whatever dynamics there were in the model could prevent the new equilibrium from being reached and, in that circumstance, the explanation given for the observed movement from the one point to the other would break down. An alternative interpretation of reality would be to locate observed points along time-paths that converge to equilibria in the model rather than to identify them specifically as equilibria. But in either case the questions of existence, uniqueness, and stability have to be explored because that is the
only way to be sure that the model can be linked to the real world.

A similar argument applies to the full general-equilibrium model with many goods and many agents. And in spite of the fact that considerable resources and energy have already been devoted to the investigation of the existence, uniqueness, and stability questions in this context, satisfactory answers are available only in the case of existence. The problem is that, although sufficient conditions for uniqueness and stability are known, these conditions, contrary to the tenets of methodological individualism, are expressed as restrictions on aggregated, that is market, excess demand functions. Furthermore, it is not clear if it will ever be possible to give uniqueness and stability conditions that are stated with respect to the preferences or behavior of the individual agents. Therefore, if methodological individualism is to be maintained, even more resources and energy will have to be diverted to the mathematical analyses of general-equilibrium models. And if this quest fails, then without even questioning the realism of their assumptions or the relevance of the conclusions derived from them, general-equilibrium models will have to be discarded because they are unable to provide viable explanations of that which can be observed.

II

Justifications for the use of formalization in general, and mathematics in particular, by economists (and others) go back quite some time. Cournot [3, pp. 2-5] and Jevons [12, pp. xxi-xxv, 3-5] believed that certain forms of reasoning in economics are mathematical in character. Walras [31, pp. 47-48] thought that to be scientific is, in part, to be mathematical. More recently, Suppes [30] provides seven reasons why formalization is desirable: It aids in the clarification of conceptual problems and the building of logical foundations, in the bringing out of explicit meanings of concepts, in standardizing terminology and methods, in permitting the development of a general vision without obstruction by inessential details, in allowing the attainment of a greater degree of objectivity, in setting out the precise conditions required for the analysis to be considered, and in obtaining the minimal assumptions necessary for statement of the analysis. Debreu [4, p.275] cites linguistic convenience and the ability to obtain deeper understanding and analytical extensions that might not otherwise be possible. Mathematical reasoning, according to Gorman [6, p.273], is important in economics because it helps in determining “... what implies what. It is from the solid basis of such knowledge that one can make imaginative leaps into the unknown.” For Solow [29, p.33], mathematical reasoning is preferred because, ceteris paribus it is more exposed: “... what you see is what you get.” And Weintraub [33, pp.178-179] argues that, since we comprehend our economic world by creating mental structure, and since doing mathematics is creating mental structure in its “purest form,” mathematics is naturally important and relevant in economic analysis.

All of the benefits of formalization contained in and implied by these justifications can be observed in the example of general-equilibrium analysis described in the previous section. Indeed, the general-equilibrium analogy or metaphor created through formalization is powerful, elegant, and appealing, and has led to what are considered by many to be significant results. In addition, formalization in this case can be seen as no less than the means by which the logical implications of the general-equilibrium model are worked out far enough so that its viability and usefulness can eventually be appraised. Because of its immense complexity, such a goal is probably not attainable for the subject-matter of general-equilibrium analysis without formalization.

What, then, is the problem? Why is there so much dissatisfaction? My contention is that the unhappiness arises from two main difficulties with current economic analysis in general. The first has been characterized by Hutchison [10, p. 88] as a “crisis of abstraction” brought about by oversimplification and the consequent irrelevance with respect to reality.” Hutchison traces the crisis back to that classical model-builder, David Ricardo. To build a model, as pointed out earlier, Ricardo (like everyone else since) had to abstract from reality and introduce simplifying assumptions. And the most significant of these was his postulate of complete and perfect knowledge. Throughout our history, says Hutchison [10, p. 79], “This postulate has probably been the most important and pervasive single simplification, bearing more logical weight than any other, in the whole range of economic theorizing, analysis, or model-building.” It is also the main source of the crisis of abstraction. “The most criticizable and
unrealistic feature of ‘..... Economic Man,’ .....’ to use Hutchison’s words again [11, pp. 2-3]. ‘..... is not his materialism, or selfishness..... [but rather] his omniscience,’” and this has nothing to do with the expression of that omniscience in terms of formalizations. Thus the thing that is causing discontent here is the assumption-content of the analysis and not the employment of formalization as a method of explanation.

The second difficulty that causes unease emerges from the kinds of questions that economists tend to ask. There is bound to be dissatisfaction with the application of formalization to answer what are considered to be the wrong questions. But then the problem is not really with the use of formalization. Of course, questions are set by context, and the context most common today is based on the notion of equilibrium. In other words, by postulating an equilibrium framework (this is not necessarily to employ a general-equilibrium model), economists commit themselves to certain kinds of questions, far broader than mere existence, uniqueness, and stability of equilibria. But it is even more significant that when an equilibrium framework is invoked, vast areas of reality are closed off from analysis. Most notable, perhaps, is the exclusion of historical time, the frame of reference for real time, as opposed to the logical or clock time in a mechanistic model. One is reminded of Marshall’s comment [22, p. 461] that:

“The theory of stable equilibrium ..... helps indeed to give definiteness to our ideas; and in its elementary stages it does not diverge from the actual facts of life, so far as to prevent its giving a fairly trustworthy picture of the chief methods of action of the strongest and most persistent group of economic forces. But when pushed to its more remote and intricate logical consequences, it slips away from the conditions of real life ..... [E]conomic problems are imperfectly presented when they are treated as problems of ..... equilibrium, and not of organic growth ..... “The Statical theory of equilibrium is only an introduction to economic studies ..... Its limitations are so constantly overlooked, especially by those who approach it from an abstract point of view, that there is a danger in throwing it into definite form at all. But, with this caution, the risk may be taken .....”

At any rate, these exorcisms have nothing to do with the presence or absence of formalization as such but they are, it seems to me, the second reason for so much discontent. The two difficulties or sources of dissatisfaction with economic analysis just described are, in part, transcended by what may be called the “angle of vision” that any scholar brings to his work. Angles of vision emerge from pre-analytical persuasions arising out of backgrounds and experiences, and they influence, in turn, the nature of the questions asked and the assumption-content of the analyses put forward to answer them. In this way, and in a very broad sense, all scholarly enterprise is inescapably contaminated by ideology, politics, culture, and values. Thus, not only are many attractions on formalization more properly understood as attacks on the questions being asked and on the assumptions being made, but also may explicitly or implicitly be raising issues concerning the rightness or wrongness of the angle of vision upon which these elements, as well as the formalizations themselves, are based.

There remains, nevertheless, the issue of whether formalization in economics has gone too far. Have we arrived at the point where economic analysis has become, to use Hick’s phrase [9, p. viii], a “good game” in which the pursuit of mathematical “fun” takes priority over the shedding of light on economic phenomena? Were this so, then the fear of Katouzian [14, p.168] that mathematical form and technique might come to determine the substance and content of economic knowledge, would be upon us. However, illustrating his thesis with respect to the development of general-equilibrium stability analysis, Weintraub [34, ch. 4, p. 46] takes the position that this has not yet happened. There exist, instead, a symbiotic relation in which both economic conceptualizations select the mathematical tools and the mathematical tools select the economic conceptualizations. Reassuring as this might be, it does not address Morishima’s belief [26, p.64] that the marginal productivity of using mathematical argument has declined, “..... not from the increase in the absolute quantity of mathematics at our disposal, but from the ever greater injection of mathematics into a fixed quantity of material.”
Nor does it preclude the notion that formalization goes too far when improper uses are made of it. That is, economic analysis becomes a good game when, for example, it engages in arguing out the logical implications of alternative formalized assumptions that can have no analogical or metaphorical relation to the phenomena under investigation. In such a situation the acceptable accordances between the formalizations being manipulated and reality have broken down, and further exploration of these formalizations (which actually cease to be formalizations in the sense defined earlier) is irrelevant to explanation and clarification.

In the end, the issue of whether formalization in economics has gone too far in either of these last two connotations is a judgement call. And if it has, the reason may have more to do with the sociology and psychology of our profession than with formalization itself.\* For if a scholar chooses to tackle a problem through the use of formalization as opposed to, say, analysis by example or storytelling, it may be all or in part because of the necessities and advantages, described above, that the employment of formalization entails. But in many cases it may also be because, as Prest's advice suggests, that is where the rewards of publication, recognition, support money, promotion, and tenure are. The scholar's angle of vision notwithstanding, even the selection of the problem to work on is subject to the same reward pressures. And the structure of these rewards tends to be set by the established standards of what constitutes relevant and significant questions, and what makes up the appropriate assumption-content of analyses which purport to provide answers. Clearly the existence of established standards provides a powerful rationalization for the continued use of formalization. I maintain that formalization for the sake of formalization alone has never been, and will never be, knowingly acceptable or confessed to by most economists. But this is not to say that we are immune to being fooled by the questions we ask and misled by our assumptions.

In another sense, formalization in economics cannot go too far. For as I have argued above, as soon as one commits oneself to the pursuit of the questions and assumptions of, say, general equilibrium analysis, then one has no choice but to follow the accompanying formalizations to their logical ends. As the French philosopher Granger said of axiomatization [7, p. 145], which is, of course, a special kind of formalization, "If the axiomatizations proposed by [...] the economist are of any use for the progress of [economic] science, it is not because they appear to ape the constructions of mathematics. It is because they offer to rational thought the sole means of escaping from the attractions of data derived from experience," and one of the few ways of extracting positive structures from the complex and confounding jumble of reality.

Notes
1. The author would like to thank Randall Bausor, Philip Mirowski, Stephen A. Rosen, and Douglas Vickers for their help.
2. For example, Katouzian [14], Klammer and McCloskey [20], McCloskey [23, 24], Ward [32], Woo [36], and the collections of essays edited by Bell and Kristol [1] and Wiles and Rooth [35].
3. For a history of how formalization entered economics and became widespread, see Mirowski [25].
4. This is not to say that there are not other means of analytical expression that are equally powerful and appropriate. But given current professional standards and our backgrounds, formalization is often the mode of choice.
5. See, for example, Katouzian [14].
6. [32, p. 180]. It is possible to think of storytelling as encompassing both description (including analysis by example) and formalization (including model-building). For example, one might tell a story as description that incorporates "stylized" or hypothetical facts. Alternatively, a story might make use of formalized elements as defined below, or employ both description and formalization in its telling. But in all cases, the distinctive feature of storytelling is narrative. It is in this sense that storytelling is taken here to be distinct from and independent of analysis by example and formalization.
7. Why you might choose specific gears, for example, or even why you might choose gears at all does not matter for present purposes.
8. It is not necessarily that these relations be expressed in mathematical form.
10. See, for example, Keynes [18, p. 290].
11. Katouzer [15].
12. A discussion of the kinds of things that are omitted appears in Katouzian [17].
15. In economics, metaphors are sometimes established implicitly through the use of the phrase "as if". Thus, for example, a particular consumer's observed behavior may be explained as if he were a constrained utility maximizer.
16. This is not to exclude the approximateness that comes about in the variables and the relations from the abstraction process described above.
17. McCloskey [23].
18. To provide but one illustration of the criticism it has received, note the following comment of Kaldor's [13,
p. 13): "... since Walras first wrote down his system of equations over 100 years ago, progress has definitely been backwards not forwards in the sense that the present set of axioms are far more restrictive than those of the original Walrasian model. The ship is no nearer to the shore, but considerably farther off, though in a logical, mathematical sense, the present system of derived tautologies is enormously superior to Walras' original effort."

19. There seems to be some misunderstanding in the literature on this point. McCloskey [24, p. 67], for example, incorrectly states, "the problem is that ....... general theorems on existence of equilibrium do ....... not relate to anything an economist would actually want to know." Subsequent discussion addresses the issue.

20. The assumption of a single supply curve is sufficient but not necessary for the construction of the model.

21. Note that demand curves, supply curves, and equilibrium points cannot exist in reality. They can only be present in models. Similarly, to prove that equilibrium exists and is unique and stable in a model can never imply that unique and stable equilibria exist in the real world. The error of thinking that such elements are a part of reality has been referred to by Machlup [21, p. 12] as the "fallacy of misplaced concreteness."

22. See Kirkman [19].

23. Note that this irrelevance for reality arises for a different reason than the potential irrelevance, described at the end of the previous section, of the general equilibrium model due to the possible lack of uniqueness and stability of equilibria within it.

24. The epistemological issue of how context is determined is not discussed here.

25. See Myrdal [27, p. vii-viii].

26. A model of the sociology of an academic profession such as economics has been proposed by Katouzian [14, pp. 119-123].

References