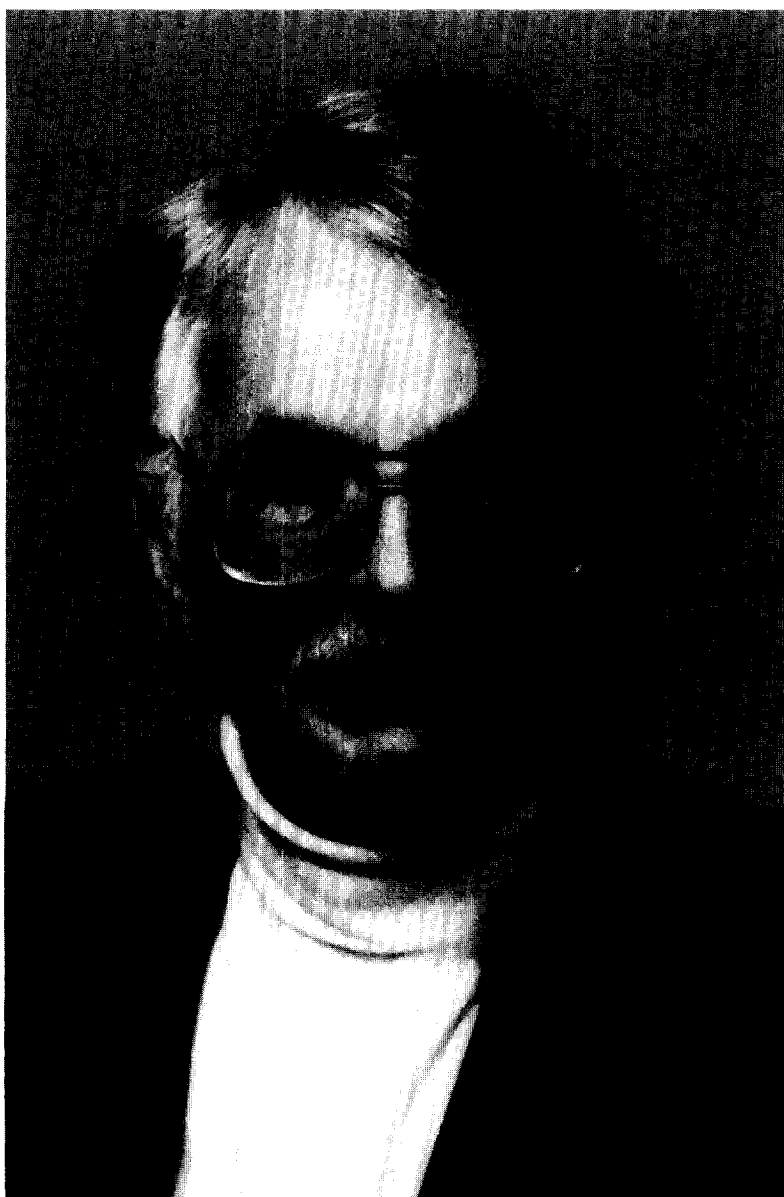


Economics as an inductive science

Clower, Robert W

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Robert W Clower

Economics as an Inductive Science*

ROBERT W. CLOWER
University of South Carolina
Columbia, South Carolina

Logic is the art of going wrong with confidence.

Anonymous [30, 197]

It seems appropriate to begin by recalling something from our founder, so I start with a passage from the *Wealth of Nations* where Smith writes about “Institutions for the Education of Youth” [48, 763]:

If the teacher happens to be a man of sense, it must be an unpleasant thing to him to be conscious, while he is lecturing his students, that he is either speaking or reading nonsense, or what is very little better than nonsense.

All who have taught macroeconomics at any time during the past forty years will know this feeling well; for macroeconomic theory has been riddled with Kuhnian anomalies [32, 202 ff.] since its inception. Strangely, few teachers of *microeconomics* seem embarrassed by the purely theoretical material they teach and which their students are required to read and regurgitate, although microtheory is at least as objectionable as macroeconomic theory. That we who teach any kind of “pure” theory ought to be as embarrassed with microeconomics¹ as with macroeconomics is the position I propose to argue here.

Before I proceed, let me emphasize that by “pure theory” I do not mean what most working economists, including, I imagine, our most recent Nobel Laureates, Douglas North and Robert Fogel, mean when they use the word “theory” without further qualification. Generally speaking, we mean by “theory” the fact-oriented creative mixture of intuition, casual empirical knowledge, and seat-of-the pants logic that is found in virtually all “applied economic analysis” and, indeed, in virtually everything called “economics” before 1950 [20, 284]. By pure theory I mean the axiomatically-based *neowalrasian* analysis of Arrow-Debreu [3], Debreu [15], Arrow and Hahn [4] and closely related offshoots that serve as a standard of “economic correctness” in all modern

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1. I have in mind particularly such self styled “New Keynesians” as Mankiw and Stiglitz. The former claims a link with Keynes because involuntary unemployment, monetary nonneutrality, and sticky wages and prices are acknowledged to exist [33, 565]. And, judging from his 1993 principles text [51, 682–83], Stiglitz would agree that by introducing ad hoc theoretical gimmicks into standard textbook theory Mankiw and other New Keynesians have “reincarnated” Keynesian Economics “. . . into a body with firm microeconomic muscle” [33, 560].

teaching not only in microeconomics but in macroeconomics, money and banking, finance, and econometrics.

The phrase “economic correctness” corresponds to what Peter Howitt, in a recent paper [26], calls “The Neowalrasian Code”:

Adherence to an increasingly complex code of formal ideas has become the overriding criterion of success, rather than fruitful modelling of observed phenomena. The code of modern economics has become for the most part that of neowalrasian analysis, with its rules for modelling all behaviour as the outcome of rational choice. [. . .] But accounting for some phenomenon in a discipline dominated by an elaborate code consists not of telling stories designed to convince others that this is why the phenomenon exists, or why it appears the way it does, but of telling stories, no matter how ad hoc, that incorporate some aspect of the phenomenon, no matter how trivial, without violating the code. [. . .] Economists building “rational models” to account for things not found in conventional theory think of themselves as seeking explanations in the usual sense, whereas in fact they are addressing purely semantic questions that don’t even arise once one ventures out of the neowalrasian cloister. Only by the rarest fluke could someone working under such a delusion come up with a convincing scientific explanation of anything.

I have titled my paper “Economics as an Inductive Science” in recognition of the contrast between inductive (fact-oriented) science and the kind of microeconomic theory—really “verbal mathematics” [20, 278]—our contemporary textbooks contain, which Whewell [56, 14] aptly characterized as a philosophy (some might say *catechism*)

. . . constructed on notions obscure, vague, and unsubstantial, and held in spite of the want of correspondence between its doctrines and the actual train of physical events [. . .] . . . the object is not to interpret nature, but man’s mind.

Joan Robinson [42, 122] long ago described the central problem of economics as being to understand how the economic system works, or as Keynes once expressed the issue [28, 35], “Is the economic system self-adjusting?” More generally, as the astronomer Simon Newcomb [35, 9] observed:

There is nothing in the wonders of the heavens or the mysteries of chemical combination better fitted to kindle our curiosity, and to gratify our desire to understand what is going on around us, than the study of the social organism.

To date, however, we economists have failed to seriously address much less resolve these issues.

How are the myriad economic activities of the millions of independent transactors in private ownership economies coordinated? It is correct, of course, to assert that the coordination of economic activities is performed by an “invisible hand,” or “the price system,”² through variations in prices and quantities in response to changing “market conditions”: correct, to be sure, but just as surely inane, because such a response is no more informative than an appeal to Jupiter or Providence [1, 144]. An intellectually respectable answer should consist of something more than tired clichés; observable economic events derive ultimately not from unspecified coordinating mechanisms, whether invisible hands, price systems or neowalrasian “auctioneers” but, as James Tobin has indicated, from definable actions of real people [54, 796]. What we economists have yet to explain is the working of the *fingers* of the invisible hand [9].

A partial explanation for our failure heretofore to explain the modus operandi of “the market” may lie in our infatuation with technique. It is widely believed that a great achievement of

2. Cf. Coase [14, 387–9]; for a critique of Coase, see Clower [12].

postmarshallian (neowalrasian) economics was the “discovery” of exact conditions under which perfect coordination of individual economic activities will be achieved automatically [44, 469–70; 30, 41–53]. The truth is quite otherwise. The neowalrasian version of general equilibrium theory provides a mathematically rigorous statement of conditions under which a competitive equilibrium “exists,” but the statement is interpretable only for a hypothetical world where coordination uses no resources, where no agent ever imagines that a failure of coordination might prevent trading plans from being completed, and where institutions such as business firms and markets through which agents routinely interact in real-world economies are not just absent but otiose. In truth, neowalrasian theory makes no mention of any mechanism or agent that undertakes the task of coordination.³ The closest it comes is in the theory of “tâtonnement,” where it is postulated that prices adjust to eliminate discrepancies between demand and supply. But if we ask, Who changes prices?, Who pays whom, and with what?, Who matches buyers with sellers?, Who pays the costs of arranging and executing transactions?, Who goes long or short when demands don’t match supplies?, and What incentives motivate any agent to perform coordinating tasks?, then the theory is silent.

How might we resolve these and similar questions? Einstein wrote [18, 98]:

Science is the attempt to make the chaotic diversity of our sense-experience correspond to a logically uniform system of thought. [. . .] The sense experiences are the given subject matter, but the theory that shall interpret them is man-made. It is the result of an extremely laborious process of adaptation: hypothetical, never completely final, always subject to question and doubt.

Of course, Einstein’s reference is to Physics and Astronomy; but from a non-normative point of view, economics is just Social Astronomy. Its purported aim is to enhance understanding of the working of the economic universe. If we are ever to be taken seriously as scientists we would be well advised to proceed with this task as most practitioners of other inductive sciences have proceeded—by taking a hard look at the world around us in a serious effort to lend intellectual order to the “chaos” that strikes our eyes at first sight.

What conceptual framework is appropriate as empirical background for economic theory, supposing we wish to portray salient aspects of real-world economic behavior during, say, the past three centuries? More shortly, what are we to regard as relevant stylized facts? I propose the following (cf. Clower [8, 206–7]):

- 1) Trading occurs in decentralized, geographically disconnected, privately owned and operated retail, wholesale, and auction markets. Centralized direction or attempted improvement of coordination occurs only as an incidental aspect of law enforcement.
- 2) In all exchanges, sellers routinely insist on receiving cash or its equivalent for every sale. All advanced economies are “monetary.”
- 3) No transactor has direct knowledge about the state of the economy at any point in time, about the supposed laws that govern its behavior, or about the trading plans of any other transactor. What is known must first be learned.
- 4) All exchange economies (historically) have been self-organized: markets are created and operated as income-earning institutions by self-interested individuals who, in exchange for implicit or explicit fees, provide physical facilities (location, office equipment, trans-

3. For an extensive critical discussion of this and related issues, by a doctoral student working under the supervision of Kenneth Arrow in the late 1960s, see Starr [49].

port, telephone and other communication devices, etc.) to “give wing” to what Adam Smith called “the propensity” to truck and barter.

- 5) Prices “asked,” “bid,” and “realized” are “made” by agents, not by ineffable “market forces” [36, vol. 1, 76–83].
- 6) Transactors are able routinely to execute pairwise (commodity for “money”) trades at such times, and in such size lots, as they desire, generally without previous communication with any market maker. Because the probability is zero that sales of any commodity will equal purchases over any specified time interval, actual markets almost never “clear.”

How does this conception of economic “reality” compare with the conception logically implied by neowalrasian theory? To save time, let me state the main points baldly, leaving any needed elaboration for later. In neowalrasian theory:

- (i) Although there are demands and supplies, *there are no markets*.
- (ii) *There is no communication between prospective trading agents*; prospective trades are signalled only to a central “demon.”
- (iii) *Agents generate no observable data*; “trading plans” are stored, as it were, in the random access memory of the mediating “demon.”
- (v) *There are no endogenous institutions*: all behavioral logistics are imposed from outside the theory (contrived ad hoc by theorists).
- (vi) *No agent announces bid or asked prices*; rates of exchange are proposed and changed only by a demon mediator.
- (vii) *There is no competition among agents*, because agents never interact directly.
- (viii) *No agent voluntarily holds inventories or buffer stocks*.
- (ix) *There is no money or other medium of exchange* [14, 28].
- (x) *There is no trading*; the theory does not define, much less deal with, commodity transfers from one agent to another [49]. Appropriate “logistical” embellishments can and have been invented and added to the neowalrasian model [2; 10; 13; 15; 28; 37; 38], but always on an ad hoc basis.

Has a more counterfactual collection of ideas ever before been assembled? My answer is affirmative. Keynes argued that ideas rather than vested interests prevail in the long run. However that might be, we know that through much of history (specifically from 400 B.C. until 1500 A.D.) theories that were arguably useless, mindless or vacuous have never been discarded without a struggle. And, indeed, after Newton, at least one empirically vacuous “hard science” enjoyed not just popularity but positive acclaim for more than two centuries; so neowalrasian analysis, having existed little more than half a century, is not unique in its apparent disregard of common sense.

The “hard science” I have in mind is the mathematical theory of ideal fluids, known more familiarly as classical or “rational” hydrodynamics, which ruled the roost in the study of fluid motion from the middle of the Seventeenth until the beginning of the Twentieth Century [17, 303–394]. Early in its history, as a result of brilliant work with Newton’s pure theory of fluid motion in so-called “ideal” fluids (which are unknown in real life), the mathematicians Daniel Bernoulli, D’Alembert, Euler, and Lagrange developed hydrodynamics into an academic study so abstract as almost to count as pure mathematics. Indeed, one scholar described its status at the turn of the twentieth century as one in which “. . . fluid dynamicists were divided into hydraulic engineers who observed what could not be explained, and mathematicians who explained things that could not be observed” [6, 4]. For want of alternative teaching materials, students interested

in applied hydrodynamics were taught the classical theory of nonexistent fluids, augmented by a pseudoscience called “the science of coefficients” that bore a striking resemblance to contemporary econometrics. Then during the last half of the nineteenth century [40, 23], the inductively motivated work of Helmholtz, Lamb, Kelvin, Rayleigh, Lanchester and others transformed hydrodynamics into a discipline that could help working engineers design real airplanes [17, 337, ff]. The story is summarized in a paragraph of Prandtl and Tietjen’s classic memoir [40, 3]:

. . . the great growth in technical achievement which began in the nineteenth century left scientific knowledge far behind. The multitudinous problems of practice could not be answered by the hydrodynamics of Euler; they could not even be discussed. This was chiefly because, starting from Euler’s equations of motion, the science had become more and more a purely academic analysis of the hypothetical frictionless “ideal fluid.”

I won’t waste time drawing obvious parallels between “rational” hydrodynamics and most of what passes for “serious” theory in contemporary economics. Suffice it to say that, in my opinion, what we presently possess by way of so-called pure economic theory is objectively indistinguishable from what the physicist Richard Feynman, in an unflattering sketch of nonsense “science,” called “cargo cult science” [19, 308].

Unlike neowalrasian theory, rational hydrodynamics dealt with real-time rather than virtual processes; its “agents” [ideal fluids] no doubt were fantasies, but its “operations” [activities] had real world counterparts⁴ hence its theoretical implications could be compared with analogous factual findings about real fluids. These comparisons bred numerous scientific paradoxes—evident inconsistencies that could not be resolved without significant extensions of conventional theory [7, 3–4]. Neowalrasian analysis is an entirely different animal. It does not deal with calendar time or with real-time processes; it is concerned exclusively with hypothetical mental states (Walras called them “trading dispositions”) of “agents” whose “actions” are described in terms of concepts (e.g., production, consumption, choice) that strongly, but misleadingly suggest observability. Because “actions” in neowalrasian theory refer to “plans” [15, 37–8, 43] that are purely mental if not positively metaphysical, the implications of so-called actions could be confronted “empirically” only by a demonic being (e.g., the neowalrasian “auctioneer”) capable of collecting data by reading minds and performing other feats that would make an episode of TV’s *Star Trek* series seem like live news from CNN!

The scientific vacuousness of neowalrasian theory appears not to be recognized by some of its leading practitioners. How else can we account for Frank Hahn’s assertion, in connection with the postulated “existence” of a large number of “contingent futures markets,” that we have here “. . . an empirical confrontation since we know that these markets are in fact very scarce” [22, 15]. Arrow-Debreu theory deals with an indefinitely great number of “commodities” and excess demand functions, but it does not define much less deal with anything that remotely resembles a market in the ordinary sense of that word (but compare [15, 76, 80]); how, then, can fewness of actual spot or future markets be brought into empirical confrontation with neowalrasian theory? A more remarkable example of similar confusion is Koopman’s discussion [31, 62–3] of “Survival of Consumers in a Competitive Equilibrium”! Finally, in the same vein, a distinguished colleague, referring obliquely to the “stylized facts” 1–6, above, wrote in a recent letter:

4. To appreciate the critical importance, for purposes of empirical confrontation, of theories that refer in their logical foundations to real-time processes, see Schwinger’s account [46, 81–5] of the conjectured properties of the graviton, a theoretical entity that is as yet unknown to experimental science.

. . . you know as well as I do that all the Walrasians are perfectly aware of the basic facts about organized markets. [. . .] I presume that they must be taking it for granted that, with enough competition, the general-equilibrium model will give broad steady-state results that approximate [the basic facts]. They have not proved that, but neither have you disproved it.”

This passage reflects not only an apparent unawareness of the vacuousness of neowalrasian analysis, but an unawareness also of the impossibility of proof or disproof of any assertion about real-time processes [23, 270]. Inductive sciences deal with plausible inference, not with demonstrative reasoning [39, v].

It will be obvious from what has gone before that I see no way to make progress in economic science except by first discarding neowalrasian analysis. As indicated by the cases just discussed, the neowalrasian code exerts an insidious influence even on those who, like myself, have long harbored doubts about conventional formalist economics. For reasons that even in retrospect are inexplicable to me, my every attempt to break out of the neowalrasian mold seemed to end in a toy model that has a fundamentally neowalrasian cast;⁵ in effect, the neowalrasian code acts like a black hole, consuming everything it touches and cloning even residual orts into an Arrow-Debreu monster. I speak with some passion on this point because I, like many of my friends and colleagues, have felt “the power of the Neowalrasian Code”⁶ and can only now recognize (in the clear light of hindsight) how the code corrupts and weakens the scientific force of one’s theoretical work.

In calling for radical reconstruction of economic theory, I am sounding a tocsin that James Tobin rang more than twenty years ago, echoing earlier warnings by Oskar Morganstern [36; 54, 293–7] and Milton Friedman [20, 291–300]. And Herbert Simon’s entire professional career has been dominated by the same concern to inject *process* into formal models so that economic theory can be confronted with empirical evidence [47, vol. 1, xix–xx]. I am well aware of the difficulty of reconstructing doctrine in which “. . . there is no agreed procedure for knocking out error” [41, 75]. In that connection, let me emphasize that my discontents with neowalrasian analysis concern not its lack of “realism” but its scientific vacuousness; it deserves to be discarded not for lack of realism, whatever that term might mean, but for the reasons that Galileo and Newton discarded Aristotelian physics—because it inhibits coherent intellectual analysis, that is to say *serious theorizing*, about observable events.⁷

In an unguarded address to The Econometric Society in 1968, Frank Hahn remarked [21, 2]:

. . . there is something scandalous in the spectacle of so many people refining the analysis of economic states which they have no reason to suppose will ever . . . come about. . . . It is an unsatisfactory and slightly dishonest state of affairs.”

Hahn’s phrase “slightly dishonest” understates the case. In the first chapter of *Value and Capital* [24, 7], Hicks asserts “This is a work on Theoretical Economics, considered as the logical

5. Classic examples of what I mean by “toy model” are the search-theoretic “Rube Goldberg” monetary machines constructed by Kiotaki and Wright [29] and by Aiyagari and Wallace [2]. For details, see Clower [12]. For further examples, see Diamond [16] and Howitt [25, 176 ff.]. To my chagrin, I find that I have unintentionally constructed some toy models of my own; but to spare myself further embarrassment, I leave the identification of particular instances “as an exercise for the interested reader.”

6. The quoted phrase is my recollection of a sentiment voiced by Peter Howitt in the course of oral discussion at the Montevideo conference (above, reference [26]).

7. The difficulty, as described by Born in his explanation of Einstein’s theory of relativity, is that when one becomes habituated to conventional habits of thought (e.g., to the idea that the earth is the center of the physical universe, to the idea that absolute space and time are inherent features of “reality”), supposedly “true” theoretical results become problematic “. . . due to a confusion of habits of thought with logical consistency, a tendency we all recognize to be an obstacle to progress.” [7, 226]

analysis of an economic system of private enterprise, without any inclusion of reference to institutional controls.” Similarly Debreu, in the preface to his book [15, x] says: “. . . the theory . . . is logically entirely disconnected from its interpretations.” In the event, however, neither Hicks in his purportedly “pure logical analysis” [24, 7] nor Debreu in his more conspicuously formal *Theory of Value* [15, viii] hesitate to write freely about “markets” when in strict logic they must be referring instead to demand and supply functions. Thus their supposed reliance on formalism is a sham. What they seem actually to discuss is the real world as they conceive it intuitively—or more probably the world as they imagine it was conceived by Hicks as reflected in the text of *Value and Capital* (the influence of Hicks’s work on all later neowalrasian writings is vastly more powerful than is commonly recognized). The actual subject matter of so called formal theory these days is *sui generis*; in no way is it the abstract world of sets, elements, axioms, and mathematical operations that most theorists pretend to take as their formal, technical universe of discourse.

All things considered, therefore, Hahn’s phrase “slightly dishonest” should probably be replaced by the more concise term “fraudulent.” The apochryphal Judge Howlson of “Truth in Teaching” fame [50] would have a field day in court if one of our leading graduate schools in economics were ever charged with false advertising; for perhaps Howlson’s most trenchant remark in the “truth” case was: “It seems paradoxical beyond endurance to rule that a manufacturer of shampoos may not endanger a student’s scalp but a premier education institution is free to stuff his skull with nonsense” [50, 191].

I have much to say about the direction reconstruction of economic theory should take, but that is another article, or more accurately a book called *Monetary Economies* on which Peter Howitt and I are presently working. One broad comment is in order here, however: an *inductive* science of economics must start from explicit recognition that every observable action of real-life transactors entails finite set-up costs—real or subjective costs that are largely independent of the level of activity to which the observable action is related. In short, economies of scale are ubiquitous, and must be accommodated in any real-time description of economic processes. Neowalrasian analysis is limited strictly to convex economies [15, x; 30, 35–7; 11, 449–50]; so any reconstructed theory must deal with systems that involve nonconvexities in essential ways. This means, among other things, that the whole of modern welfare economics must be consigned to metaphysical oblivion. And that is just a minor casualty, because only slightly less draconian changes are needed to lend honesty to other constrained-optimization branches of economic theory such as the theory of “demand,” the theory of “cost and production,” and on and on. But instead of indicating what must go, let me sketch briefly some of the things that Howitt and I expect to restate or create and keep.

Specifically, we propose to contribute through our book to the development of a discipline that deals in an intellectually coherent manner with:

- (i) Self-organizing firms, markets, and related institutions, including such things as Merchant Courts (see [5]), that make and enforce laws respecting property rights and contracts;
- (ii) Business operations, costs, revenues, and survival strategies, replacing Vinerian and related fables of production and cost theory with ideas that are consistent with fact-based research;
- (iii) Household behavior: choosing income and determining budgeted expenditure, choosing the timing, frequency (lot size) and composition of actual purchases;
- (iv) Determining (time-averaged) holdings of money and other trade inventories;
- (iv) Competition as a struggle for economic viability;

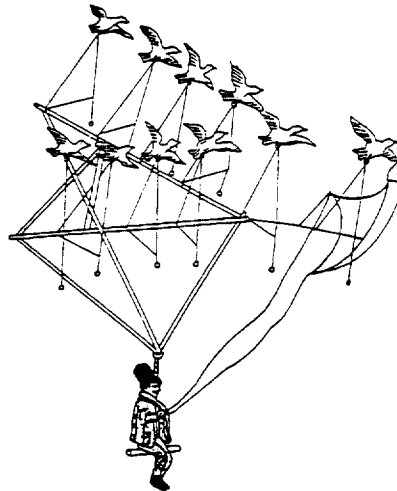
- (v) The *modus operandi* of the invisible hand (an aspect of business behavior and the operations of market-making firms);
- (v) The reasons why, in economic affairs as in freeway traffic flows and field theories of light and gravity, all action and reaction involve only “neighborhood” effects, never “action at a distance” or by metaphysical entities such as “the invisible hand.”

In reconstructing our discipline, and throughout the inductive science of economics, I would urge that our motto be: *If it isn't common sense, it's probably wrong.*

Let me end by drawing attention to the 1983 introduction to the enlarged edition of *The Foundations of Economic Analysis* [45] where Paul Samuelson refers nostalgically to the joy of having been born an economist in 1932—because then there was so much still to be “discovered” that economics seemed like a well-stocked but seldom-fished pond: one could hook something juicy with every cast of the line. It occurs to me to wonder what has changed since 1932 in the way of improved understanding of how actual economic systems work? Isn't 1993 just as good a time to be “born” an economist? We may or may not make significant progress during the next century towards reconstructing economics as an inductive science—progress in converting the present subject from a quasi-religious academic catechism into a respectable and respected intellectual pursuit. Whether, if and when such a time comes, we economists will deserve to be thought of as “humble, competent people, on a level with dentists” [26, 373], is an open question; but if such a time should ever come, we'll surely have no reason to feel humble.

Postscript

At a luncheon address to the Canadian Economic Association in Ottawa on June 5, 1993, I displayed a picture of the 1638 goose-powered space vehicle of *The Man in the Moone* [53, 16] as a mechanical analog of economic theory circa 1995. With the thought that it may furnish intellectual fun as well as profit, I reproduce the figure here.



ANALOG MODEL
OF
ECONOMIC THEORY
{CIRCA. 1995}

Reproduced from: Taylor, J.W.R. *The Lore of Flight*. Göthenberg: Tre Tryckare Cagner & Co., 1970.

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