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ECONOMICS' PAST AND PRESENT: HISTORICAL ANALYSIS AND CURRENT PRACTICE

BY

J. ALLAN HYNES

> The study of economic theory is not defensible on aesthetic grounds—it hardly rivals in elegance the mathematics or physics our sophomores learn. The theory is studied only as an aid in solving real problems, and is good only in the measure that it performs this function (George Stigler, 1949).

I. INTRODUCTION

The place for the teaching of and research dealing with the history of economics continues to draw attention, at least from within our speciality. Recent examples are the History of Political Economy mini-symposium (1992) that was anchored to Margaret Schabas's paper and Donald Walker's affirming essay in the *Journal of the History of Economic Thought* (1999). The former asked whether the history of economics, given the neglect shown by mainstream economics, should be incorporated into history of science programs. The latter argued that this neglect is unfortunate because there is a place for the history of economics in the training of economists. (The neglect is documented by Aslanbegui and Naples (1997).) An important feature of Walker's essay is that the argument was based on structural aspects of the discipline. His point of departure was Donald Gordon's (1965) claim that there is little in the history of economic analysis that facilitates the understanding of current economics and the research problems with which it is concerned. Certainly the education of natural scientists, as Gordon noted, is consistent with this claim. George Stigler (1969) made a similar claim, and both more or less took the conclusion to be self-evident. Walker's response was that, at least among a variety of important sub-disciplines, there is not professional consensus and, therefore, there may not be a sharp distinction between current and past theory. This suggests that some form of historical analysis may be useful. Walker's paper moved the discussion away from arguments that begin with the presumption that every educated economist should be versed in the

University of Toronto, Department of Economics, 150 St. George St., Toronto, Ontario, Canada, M5S 3G7. I want to thank Bob Coats and two anonymous referees for comments on a previous draft.

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origins of economics.[1] Instead, he focused on questions relating to history as a tool for understanding and for doing today's economics. In this respect, Walker returned to an issue addressed by Filippo Cesarano (1983)—they both focused on historical studies as a source of illumination for current economic analysis.

This paper follows Walker and Cesarano's leads. There is a short discussion of the implications of the shifts in historiographic perspectives that have occurred in the years after WWII. However, the primary focus is on the structure of research programs. As science develops, there is a desire to explain more complex evidence and in more exact ways. This leads to the introduction of mathematical and statistical models that intercede between high theory and facts; research problems become ones for which theory and analytical techniques are inextricably intertwined. (A large portion of the research in the history of economics studies the evolution of high theory.) As research programs become more narrowly focused, history becomes less helpful for unraveling current dilemmas. These points are illustrated with references to the natural sciences, and with a moderately detailed analysis of an exemplar for economics—the emergence of the life cycle and permanent income hypotheses. The conclusions are somewhat less supportive than those of Walker and Cesarano. There is a final section offering some speculative observations on a potential role for the historical literature of economics in enhancing economists' analytical and rhetorical skills.

Before moving on, it should be noted that there are problems of scope for discussions of this type. Smith and Ricardo may not be relevant for current research, but discoveries and solutions to problems do not arise *in vacuo*. Current research always has a history to which it is connected, and a scientist will recognize at least some of this history as part of her motivational statement. I am assuming that the horizon and breadth people have in mind when they address questions about the role of science history are longer and broader than these immediate histories. These distinctions are of course not that well defined.

II. FAMILIAR HISTORIOGRAPHIC DEVELOPMENTS

Since the 1960s, the historiography guiding work in the history of the natural sciences, and to some extent economics, has recognized that past theories have their own integrity and are not simply error-ridden shadows of current theories. The historian is advised to see science of the past on its own terms to the extent that this is possible for a person trained in the details of current science. Histories in this tradition have led to a reexamination of the view that science exhibits a continuous development of currently held ideas and a discarding of past faults. These histories have also been influential in changing perspectives of the philosophy of science, with probably the most important change being that science

[1] The purpose of this paper is not to denigrate this position but rather to think about the role of historical studies in contributing to the "tool bag" of the practicing scientist. This is an important (but narrow) question, and it is certainly not the only issue. There is evidence, for example, that students would like to see the economic analysis they learn placed in its historical context. While I am sympathetic, I am setting the questions related to this demand aside for the moment.

cannot be seen and interpreted as a narrow positivistic activity. This is what D. N. McCloskey labeled the decline of modernism; and these generalizations, while having well-known sources, fit comfortably in several historiographic frameworks.

Do the broad historiographic and implied methodological themes speak to issues related to the importance of historical studies for current analysis? The fact that theory selection is not based on narrow positivistic criteria raises questions about legitimacy, and historical analyses may highlight these. Theories are always works in progress, and it is, therefore, possible that past theories have been set aside too early in their development; if this is pervasive, revisiting past accomplishments may shed light on current problems. This issue was at the center of Cesarano's discussion. Imre Lakatos (1978) emphasized that past research programs sometimes get recycled, while Paul Feyerabend (1975) argued from his Millian perspective that it is a useful methodological strategy to continually confront accepted theories with alternatives because this competition facilitates refinement. Natural questions arise about the sources of these alternatives, but the historical record may be one source.[2] However, it is interesting that Thomas Kuhn, an innovator of the new historiography and a proponent of the view that scientific development exhibits important discontinuities, was pessimistic about history's usefulness for current research:

> Advocates of the history of science have occasionally described their field as a rich repository of forgotten ideas and methods, a few of which may dissolve contemporary scientific dilemmas. When a new concept is successfully deployed in a science, some previously ignored precedent is usually discovered in the earlier literature of the field. It is natural to wonder whether attention to history might not have accelerated the innovation. Almost certainly the answer is no. The quantity of material to be searched, the absence of appropriate indexing categories, and the subtle but usually vast differences between the anticipation and the effective innovation, all combine to suggest that re-invention rather than rediscovery will remain the most efficient source of scientific novelty (1977, pp. 120–21).

Excavating the past in search of solutions to current problems has many similarities with excavating complementary disciplines. An example from my own experience is suggestive. In the late 1960s and early 1970s, Donald Gordon and I (1970) were in a group, the members of which were rethinking the theoretical foundations of dynamic relationships like the Phillips' curve. An important issue was—and is—the speed at which people learn about their environments; a natural place to turn for advice was the literature dealing with Bayesian learning. In our division of tasks, the search was my assignment. The literature was a

[2] There was an interesting tension between Kuhn and Feyerabend. Kuhn argued that science is inherently conservative, and this is productive because theory development is a difficult task and requires commitment. Feyerabend supported the normative view that science should actively encourage the exploration of alternatives. In fact, Kuhn's assessment was less rigid than first appears; he claimed that alternatives will be accepted and paid attention to so long as they conform to certain broad principles Examples of this observation from recent history are given by the research on non-expected utility and behavioral hypotheses relating to various market phenomena. Contributions in both areas have found homes in the best journals.

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dead end because sequential Bayesian updating rules have features common to any adaptive learning model, and the latter were understood and were seen to be part of the modeling problem. In an important sense, the problem we confronted was simply different, and different in important ways, than the type of problem this literature addressed. (Robert Lucas (1972) dealt with this issue by setting aside learning and focusing on stochastic equilibria. By ignoring a formally insoluble problem, some important ground was won.) The siren song that beckons to the past or to other disciplines is often misleading because the new questions being asked are in subtle but nevertheless fundamental ways different than the ones previously examined, or examined in another literature.

III. THEORY AND MODELS: RECENT ISSUES IN HISTORY AND METHOD

While the changes in the interpretation of science's practices and the implied changes in the interpretation of scientific method that occurred in the 1960s and 1970s were dramatic, the assumed relationship between theory and phenomena was in many ways traditional. Theory was seen to set the structure from which implications for facts are directly—possibly with the help of correspondence rules—deduced. Even for Kuhn, paradigm articulation originally seemed to be an exercise in theory improvement and extension within this general frame of reference.[3] Beginning in the 1980s, the validity of this relationship between theory and evidence has been questioned. Probably the most influential initial attack was that of Nancy Cartwright (1983); and an excellent summary of the entire literature, complemented by a set of case studies from both the natural sciences and economics, is contained in Morgan and Morrison (1999).

This literature argues that there is a sharp distinction between high theory and the models that mediate between high theory and facts. High theory offers a set of general principles but has little if any empirical content; the latter is provided by a set of models. Moreover, models have a considerable degree of autonomy in that they are not delivered from theory, or facts, by the application of algorithms. Instead, models are pieced together using a variety of elements that are integrated to address specific empirical problems. The process is inherently both *eclectic* and *ad hoc* where these terms are used in the strict senses of their definitions: selecting the best from a variety of sources and concerned with a specific goal or end. These terms are often used in a pejorative sense, but they seem to describe features inherent in the process of model construction. Moreover, the specificity of models arising from this process contributes to the

[3] Kuhn (1977) acknowledged the problem of moving from mathematical theory to empirical implications. However, he thought correspondence rules were to a large extent in the minds of philosophers of science—he claimed that practicing scientists cannot articulate many formal rules of this kind. He did not discuss many of the specific problems of model construction, but he did emphasize the importance of exemplars in the analysis of new situations.

problems alluded to earlier relating to the separation of current research from broader historical contexts.

The relationship between models and high theory in physics is nicely illustrated by two examples discussed by Margaret Morrison (1999). (1) Newtonian mechanics provides a theory of the ideal pendulum. However, if a laboratory pendulum is used to measure gravitational acceleration, a number of modifications must be made to the ideal. Nevertheless, these modifications are obvious from the direct consideration of the relationship between the actual and the ideal constructs. This is a case where the model and high theory are closely aligned, a situation, Morrison argued, that is quite rare. (2) A more typical situation is provided by hydrodynamics at the beginning of the twentieth century. Experimental evidence, even for water—a fluid with low viscosity—was inconsistent with classical theory, which assumes no friction. A mathematical model that incorporated viscosity, the Navier-Stokes equations, was available, but it was too complex to provide a unified explanation of the evidence. In 1904 Ludwig Prandtl, based on an experiment involving water flowing through a trough, constructed a model in which behavior away from barriers was described by the classical equations, and behavior near barriers was described by a simplified approximation to the Navier-Stokes equations—the latter was made possible by the former. The model was autonomous because it was not derived from either of the two theories; instead it was pieced together, and the insight that this would work came directly from experimental observations.

Economists have often recognized the central place of models in economic analysis and have seen this as an embarrassment. It appears that physics is very much like what we do. In fact, the relationships between theory and models that are discussed in the Morgan and Morrison volume are illustrated by cases drawn from both economics and physics. One result is the discovery of a unity, but a unity that is quite different than often hoped for by economists. We are often accused of being too enamored with physics, and of trying too hard to emulate its pristine methods. The research on models suggests that physics and other natural sciences are really quite messy, just as our own efforts seem to be.

IV. A CASE HISTORY FROM ECONOMICS

The research leading up to, and the initial formulations of, the life cycle and permanent income hypotheses illustrate many of the features of a modern research problem in economics and provide an exemplar for understanding the problem-history-theory-model connections that are relevant for our discussion. To draw out the implications, more detail than might be usual is necessary; for present purposes, the devil is in the anatomic details of research problems. (An extensive discussion of this episode is contained in Hynes (1998, 1999).)

By the early 1950s evidence about consumption behavior from the long-established research on family budgets, and from the more recent aggregate time series studies, was recognized to contain important inconsistencies.[4] The budget studies and the short time-series studies were consistent with Keynes's hypothesis, but the evidence from both sets of data were inconsistent with Kuznets's (1942) long run results. In addition, the budget data contained unexplained instability between samples in the average and the marginal propensities to consume. These anomalies provided a self-contained research problem for budget research, but they were important for mainstream macroeconomics because of the place consumption has in Keynesian aggregate demand theory. It was the latter issue that drew the attention of Franco Modigliani and Richard Brumberg, and Milton Friedman. Modigliani and Friedman were both established macroeconomic theorists who had strong empirical interests—a rare combination in science.

It is commonly believed that the success of these contributions came from the introduction of high theory in the form of intertemporal utility theory. However, the puzzles were clearly understood by the end of the 1940s and the beginning of the 1950s, and important steps had been taken to resolve the inconsistencies. This progress was based on the common sense distinction between long run and current income, combined with the working hypothesis that aggregate consumption choices are governed by the former. Two important advances in understanding the budget studies followed. William Vickery (1947) observed that idiosyncratic shocks to current income relative to long run income implied marginal propensities to consume estimated from budget studies are biased downward relative to the true (long run) values.[5] This result was then used by Margaret Reid (1952a) as the basis for an explanation of cross section differences in the marginal propensity to consume. Marginal propensities to consume for farm families were smaller than for non-farm families, and her hypothesis was that measured farm incomes had larger measurement errors and transitory components than non-farm incomes, which given Vickery's result would explain the observed pattern. Reid had samples for all quarters of 1941 and the first quarter of 1942; she argued that average

[4] The construction of budget data had a long history, but the collection and analysis of this evidence became an organized professional activity as part of the emphasis on the collection of social statistics in the nineteenth century. The interest in aggregate time series statistics was a continuation of this general interest. However, the analyses of the two types of data were separate research programs. The original interest in budgets arose from an interest in the welfare of families. This continued well into this century to be the central concern for those who analyzed this data. The construction and analysis of aggregate time series was also the result of an interest in economic performance, but the availability of this data coincided with the emergence of the interest in econometrics. And the results from both programs attracted mainstream attention because of Keynesian theory. The creation of specific data sets and the problems and methods associated with their analysis result in research programs that are in important ways separated from "long" historical perspectives. There has probably not been sufficient attention paid to this issue. An interesting study of data collection driving model construction is given by Bogaard's discussion in Morgan and Morrison (1999) of the relationship between Jan Tinbergen's macroeconomic model construction and the construction of the Dutch national accounts in the 1930s.

[5] Vickery's paper was an examination of the welfare implications of budget data. His argument was that incomes above the sample mean would contain a disproportionate fraction experiencing positive shocks while those below the mean would contain a disproportionate fraction experiencing negative shocks. This was an informal argument, but the logic was that of the formal problem of regression toward the mean. There is evidence that this issue was understood for some time. Unfortunately, citation policies in this early literature are very casual, and it is difficult to trace certain ideas.

transitory components should have been larger in the 1942 sample. When conditioned by year and income type, each income type had a smaller marginal propensity to consume for 1942; and for each year, the marginal propensity to consume for the farm sample was smaller than for the non-farm sample. This was the first test of a permanent income hypothesis.

The conflicting evidence from time series studies was also explained by the difference between long and short run income. Kuznets's finding of a constant long run consumption-income ratio was adopted as an empirical law, and the non-homogeneous Keynesian consumption function yielded by short time series was interpreted as reflecting the fact that, for short time series, the cyclical component of current income does not average out. This would flatten the short run relative to the long run consumption-income relationship. While this explanation was conjectured by several economists, the econometric resolution was independently produced by Franco Modigliani (1949) and James Duesenberry (1948). Modigliani's argument is particularly useful for present purposes. He assumed Kuznets's finding was a long run law, and he separated the long and short run effects by constructing what he termed a cyclical index—the difference between the highest previous income and current income, relative to current income. A regression of the current consumption-income ratio on this index delivered estimates of long and short run marginal propensities to consume that were close to both Kuznets's result and the estimates from short time series (.89 and .7 respectively).

Despite this progress, Modigliani and Brumberg (1954) were critical of the state of research, claiming there was considerable disarray; Friedman (1957), while less critical, claimed too little attention had been devoted to the connection between measured magnitudes and their theoretical counterparts. In addition, while there had been important gains, the evidence had not been organized within a coherent theoretical framework. To eliminate this gap, both hypotheses used utility theory as an organizing framework; both also began their discussions by emphasizing that current income is only one element in the determination of opportunities, and, therefore, the Keynesian consumption function is mis-specified. Friedman formulated the theoretical consumption function in terms of wealth, or permanent income.[6] Because of their strong life cycle assumptions Modigliani and Brumberg chose a more contrived stationary income variable. Unfortunately, the data did not contain measurements of wealth or any long run income variable, and utility theory, from which the theoretical consumption function was derived, did not contain an operational solution for this problem.

While both hypotheses used essentially the same strategy to explain the budget evidence, the permanent income hypothesis did a more elegant job, and it is a more useful expository instrument. Friedman adapted the model of income that he and Kuznets had used to analyze professional incomes (1945). Measured income was assumed to have a permanent and a transitory component: $y = y_p + y_t$, and from the homogeneity assumption, the consumption function was $c_p = ky_p$. Given a set of independence assumptions, least squares algebra gives an interpreta-

[6] In addition, both contributions assumed that the intertemporal utility function was homogeneous. These functions are consistent with constant consumption-wealth or consumption-permanent income ratios. This assumption was almost certainly influenced by Kuznets's empirical finding.

tion of the coefficients in the current income function, $c = a + by$, in terms of the structural parameters:

$$b = kP_y, \text{ and } a = k(1 - P_y)\bar{y}_p, \qquad (1)$$

where $P_y = var(y_p)/var(y)$, and $\bar{y}_p$ is the mean value of permanent income. The permanent income hypothesis expressed one set of parameters, those of the Keynesian consumption function, in terms of a set of deeper parameters. This gave the permanent income hypothesis the ability to explain most of the cross section variation in the naive parameters a and b. (Although, the explanatory success required the introduction of outside information about the relationship between P_y and other properties of the samples, which Reid (1952b) partially confirmed.) An additional feature of the permanent income hypothesis was that it provided an encompassing framework within which both the cross section and the time series evidence could be interpreted.

The relationships in (1) illustrate several of the previous comments about the relationship between models and theory. High theory—in this case utility theory—provided a framework within which a consumption function could be derived: it provided an interpretation of k, but it was the imported model of income structure that supplied the explanatory power. Little would have been lost had the function $c = ky_p$ been introduced as a hypothesis based on Kuznets's results, as had been done in the earlier literature. This comment is equally relevant for the cross section and time series interpretations. But for the time series analysis, Friedman needed an estimate of the unobservable permanent income. Again, high theory did not have a suggestion, and he adapted the error learning model that had proved useful in examining hyper-inflations.[7]

While utility theory did not provide explanatory empirical power, it did provide cachet because it was a unifying framework within which the consumption function had a coherent interpretation. Even though the explanations of the empirical anomalies were to an important extent independent of this particular interpretation, it is understandable that Friedman and Modigliani would value this role for utility theory. They were theorists and were addressing the consumption function as an issue at the center of mainstream macroeconomic theory.[8] Also, capital theory was in the air. The controversy surrounding the Pigou effect had culminated in the publication of Don Patinkin's classic work in monetary theory, and there was optimism about the ability of capital theory to provide a foundation for macroeconomics.

[7] Modigliani and Brumberg employed essentially the same line of argument in their analysis of the budget data. However, they did not provide the neat parameterization that was a signature feature of the permanent income hypothesis. But their analysis of the time series evidence was based on the income dynamics of the life cycle model, not on an error-learning model for long run income.

[8] The discussion has focused on a classic episode where the connections between theory and specific facts were at issue. But the relationships between models and high theory are present in almost any current program in what is usually labeled middle-brow theory (where the actual mathematical sophistication is fairly modest). The research on contract theory under various assumptions of symmetric and asymmetric information is a good example as is the work by Randall Wright and a series of co-authors on the foundations of monetary economies. (See Kiyotaki and Wright (1989) for example.)

V. DISCUSSION

There are obvious difficulties with drawing general conclusions from specific cases, but questions relating to potential roles for the history of economics need empirical contexts, and the early history of the modern theory of aggregate consumption behavior contains features that are present in a wide variety of research programs. Moreover, specific histories make it possible to see at least some of the issues in sharp relief.

The two hypotheses had a well-defined history—they were not the product of flashes of insight. And understanding this history is necessary to appreciate the dynamics of the developments. Nevertheless, the history was circumscribed and focused on a narrow range of problems. Many of the important precursors were contained in research reports and were addressed to the relatively small group of specialists in the field; a large fraction of these contributions were presented at annual National Bureau of Economic Research (NBER) conferences and published in the NBER series, *Studies in Income and Wealth*. There was little if any connection to a larger, more general historical literature; there was certainly no connection to the type of literature that would be conventional fare in a history of economics program.

A common claim is that the history of economics provides theoretical alternatives that can be useful in removing current dilemmas. The example considered above speaks to this claim. The life cycle and permanent income hypotheses were not the only hypotheses put forward to deal with the evidence. A competitor was the relative income hypothesis that came from two independent sources. The initial suggestion came from Dorothy Brady and Rose Friedman (1947) who wanted a specification that would be more stable in cross sections than the current income consumption function. Their hypothesis (their model) was that the ratio of consumption to current income should be a stable function of the position in the income distribution, where this is measured either by the ratio of current income to the mean income of the sample, or the percentile location of current income in the sample. Brady and Friedman did not offer a sociological or historical motivation. Rather, theirs was a purely empirical conjecture which was suggested by the data (Hynes 1998).

James Duesenberry (1949) outlined the socio-economic connections in his version of the relative income hypothesis, and he motivated his work with historical references to Thorstein Veblen and with interdisciplinary references to the social psychology and sociology literature. Nevertheless, these explanations did not alter the implied empirical model. For the budget studies, Duesenberry tested one of the empirical models that Brady and Friedman had introduced. He regressed the consumption-income ratio on the percentile location of current income, and he obtained results that were broadly consistent with those of Brady and Friedman.

The idea that a person's consumption may depend on her place in the income distribution had been around for some time. It was part of the general criticism of utility theory that held preferences are to an important extent socially determined and, therefore, may not be stable either through time or in cross sections. Furthermore, this was not a view held only at the heterodox fringe.

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Frank Knight was a well-known proponent of this idea. And Paul Samuelson (1937), in a paper that applied intertemporal utility theory to a problem in the measurement of marginal utility, also claimed that this was not a realistic foundation for the study of actual consumption-saving behavior. George Stigler (1939) questioned the realism of holding tastes constant given large changes in income. Finally, Allen Wallis and Milton Friedman (1942) questioned the potential productivity of utility theory in general as a source of important empirical hypotheses.

Those who made this argument thought its credibility was self-evident: conventional utility theory is simply too unrealistic. The important feature of the contributions of both Brady and Friedman, and Duesenberry, was that they confronted evidence—the instability of the current income consumption function—in need of an explanation. Therefore, they required an operational characterization of this idea; they had to write down an implementable empirical model. Given the commitment to a specific model of distributional effects, it was possible to judge the importance of these effects in comparison to alternative interpretations of the same data. An examination of (1) shows that the permanent income hypothesis also implies a specification where the consumption-income ratio is a function of the ratio of average sample income to current income. (Given normal distributions, there is also a specification in terms of the percentile location.) However, unlike the relative income hypothesis, (1) has interpretations of the residual instability present in the data. The attraction of (1) was that it could characterize one set of parameters as functions of a deeper set of structural parameters and thereby offer an explanation for instability of the former. This illustrates a common source of progress in the construction of models that mediate between theory and facts (see the discussion in Hanson 1958).

A feature of the permanent income and life cycle hypotheses that Friedman, and Modigliani and Brumberg all thought important was that they explained the evidence in a manner consistent with standard utility theory, and it was not *necessary* to introduce complexities in the characterization of tastes. Modigliani and Brumberg (1954) were explicit about this point. And while Friedman recognized that the permanent and relative income hypotheses were not inconsistent with one another, he preferred the former because it unified the explanations of the cross section and time series evidence, and it seemed to have a richer set of implications.

George Stigler, in the closing remarks to his review of the history of utility theory (1950), observed that for over a quarter of a century some of the best minds in the profession contributed to the development of utility theory, but in the end the principle finding was that if people do not purchase less of a commodity when their incomes increase, the law of demand holds. He argued that the theory would have been richer had it been developed to confront a larger set of empirical questions. The evolution of the permanent income hypothesis is an instance where an important extension, the understanding of income and wealth effects, came from an effort to make sense of empirical puzzles. The episode also underlines the difficulty of assessing the importance of methodological critiques independent of their comparative successes in dealing with specific empirical questions. This point is often overlooked in heterodox criticisms that come from the past or from current sources.

This latter point raises additional problems about studies in science history and their relevance for current economics. Consider an example. Philip Mirowski's interesting and provocative reexamination of the development of utility theory (1989) has been a magnet for controversy, a large part of which has centered on the relationship of the marginal revolution to features of nineteenth century physics. This is an important topic and one that is a natural subject for a history of science project. However, associated with the historical analysis was the argument that casting utility theory in a form that could be characterized by the mathematics of constrained maximization foreclosed potentially important investigations relating to the causal underpinnings of preferences. This conclusion is to a certain extent true, although it was not a new criticism. There has always been uneasiness, even at the center of the profession, that the social nature of tastes may raise problems relating to stability and reversibility. But like the development of utility theory itself, these criticisms in general, and Mirowski's restatement of them in particular, have to a large extent been independent of concerns for specific empirical problems. The relative income hypothesis was important because it addressed anomalies by introducing an operational model that reflected these concerns. This made possible a comparison with an explanation that fully accepted the conventional theory, and the results were ambiguous at best. These comments also have obvious implications for the often-made claim that additional realism serves to improve explanatory power.

Experimental evidence will often (always?) have explanations within several theoretical frameworks. Models that mediate between theory and facts will (must?) share some important characteristics, although the interpretation of these characteristics will differ. This makes it difficult to distinguish between theories, and it is one of the reasons that critical experiments are difficult to construct. The research on the theory-model-facts relationships seems to imply that the problems of testing and distinguishing between alternatives are more complex than is commonly recognized.

The ascendancy of neoclassical economics is seen by many to be the result of an unhealthy desire to be like physics. However, the same issues related to models and theory arise in research programs based on heterodox foundations. An interesting example from Marx's analysis of reproduction schema is discussed by Gert Reuten (Morgan and Morrison 1999). Reuten's analysis highlights the point that to make any theory congruent with even broad stylized facts, the mediation of models is required and must face the kinds of choices discussed above with the same broad implications. A second case is provided by Michal Kalecki's analysis of the business cycle (1935; see Baumans 1999). The latter was also explicitly motivated by a Marxian perspective in that it put investment at the center of macro dynamics in capitalist societies.

Kalecki's contribution illustrates a point made above. His objective was to construct an aggregate model, the solution to which would mimic the business cycle, including existing measures of amplitude and period. His investment process was given dynamic richness by the introduction of a time to build lag. But, he needed an output constraint and a specification of consumption behavior. (A simple linear current income consumption function served the purpose.) When the parts were integrated, the resulting structure was a fixed price multiplier accelerator model with a reduced form that was a mixed differential-difference equation. The solution technique was adapted from an independent contribution by Tinbergen. In addition to supplying another example of the role of models in interceding between theory and facts, Kalecki's contribution highlights the point that because *models* connect different high theories to the same set of facts—and it is inevitable that they will have important similarities—it is difficult to distinguish or choose between the theories without considering ancillary implications.

VI. HISTORY OF ECONOMIC THOUGHT AS LITERATURE, AND OTHER ISSUES: CURSORY COMMENTS

If the history of economics is questionable as a source of solutions for current research problems, can it be relevant for current scientific activity in less immediate ways? George Stigler (1969) argued that economists' abilities to read and understand the literature of their discipline is lacking, and the study of our historical literature is a valid part of a program that would enhance these skills. Stigler emphasized the difficult task of isolating a scientist's essential contributions, and he understood the latter to be a problem of inference that requires an examination of the internal consistency of a scientist's body of work. From the perspective of post-modern critical theory, Stigler's point may seem old fashion. However, his perspective does not close the door to alternative interpretations and the need to reconcile these alternatives through persuasion. After all, inferences are always open ended with the resolutions depending on interpretative judgments. Therefore, his general point would seem to be consistent with the argument in critical theory (as set out for example by Stanley Fish (1980)) emphasizing the roles of alternative interpretive communities.[9]

The problems related to reading with understanding are, of course, as real for current texts as they are for the literature of the past. However, Stigler thought, because of the distance, a greater degree of neutrality is possible when reading the history of economics; current controversies tend to be more prejudicially contested. Although the examples given by contributions to Ricardian scholarship in the post-war years produce a certain amount of skepticism about this point. At the same time, these controversies are interesting cases that could provide general insights into problems of textual interpretations and their resolution—here is an example of an interesting role for the study of the history of economic analysis.

[9] Derridean deconstructive theory does not place any interpretive restrictions on the text—any interpretation would seem to be allowable. Fish accepts the position that texts do not have "innate" meanings. However, he argues that a text will have a determinate meaning given the rules and norms of a particular interpretive group or community. For each group the text will have a well-defined context; the evidence in support of an interpretation will be defined by the interpretive framework. Different interpretations can be resolved, but only by persuasion. This is a conventional view of textual understanding, and it has interesting connections to familiar conventional interpretations of scientific understanding.

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While interpretive readings of past and present contributions share many problems, there is at least one important difference. Depending on where the line is drawn, the history of economic analysis contains a larger literature in which the analysis is stated in a literary manner; theorems and proofs are set out in a narrative form. Recent literature contains a high portion of contributions in which the analysis and presentation is mathematical in form. Each presents its own set of difficulties, but mastery of both enlarges an economist's set of skills.

Stigler focused on the problems of reading, but there are the equally important problems of writing and presentation in general. There is also a specific issue. As analytical techniques become more mathematical, there is a tendency for expositions to let the mathematics do the talking. This is not only literally true, but it is also true for the language, which takes on a pseudo- mathematical form. The result is unfortunate for two reasons: it reduces the size of the potential audience, and it also has an unsatisfactory analytical consequence. Understanding the implications of mathematical analysis is sharpened when the results are stated in clear jargon-free prose—when the narratives implied by the mathematics are explicitly stated. Even the verification of theorems is aided by this strategy. D. N. McCloskey (1985, 1990) has convincingly made the case for a kinder, gentler rhetorical style in economics as a way to improve communication. It would at the same time deliver some very real analytical benefits. While these comments do not provide sufficient conditions for a role for history, again, depending on where the line is drawn, the history of economics supplies a rich source of interesting examples of narrative used in the service of sophisticated analysis.

Finally, the analysis does not seem to imply a definitive answer to a question posed at the beginning relating to where the history of economics should seek a home. I am not sure this is a meaningful question. Individuals will continue, depending on their idiosyncratic situations, to find shelter in both places. And a collective change in the profession's attitude seems to me to be a low-probability event. Nevertheless, there is an interesting issue. A friend, when she heard the punch line of the paper, commented that if we don't study and interpret our own history, others will. She was suggesting a custodial function. Controversy has focused almost exclusively on the role of the history of economics in the training of economists. The flip side has received almost no systematic attention. What is the role for training in economics in the education of those who wish to do research in science history? Even if Kuhn's comments and the foregoing analysis are not decisive, the ability of the historian of economics to provide help is difficult. Not only must the history be in hand, but a mastery of current research programs sufficient to enable participation would seem to be required. Put in its strongest form, the question becomes: is it necessary to have been a practicing scientist to gain a sufficient level of understanding to address the questions about structure that are central to science history. (Actually, Donald Walker (1999) thought this is a reasonable proposal. Nevertheless, it should be kept in mind that resource constraints are binding.) I will conclude with this question, leaving the answers and their implications for another time.

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