An Assessment of the Impact of the Computer*

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I. Introduction

Goodwin [14] pleads for an extension of the analysis of the process shaping economic theory, and Ruggles [27] stresses the need to recognize new influences on economic research. Traditionally emphasized influences include (1) the economic problems calling for attention in a period and (2) the genius, intuition, perceptions, or logical powers of economists themselves. The first source is certainly dramatized by Keynes' development of the concept of less-than-full-employment equilibrium, partly in response to the persistent unemployment of the early 1930s. The second ascribed cause of change in economics must be borrowed in large part from mathematics with its time honored tradition of naming theorems after their authors, e.g., Euler's theorem, Bayes' theorem, Bernoulli's solution, and so on.

Ruggles extends the list of influences on economic research to include the technology of information processing. In its early history, there were adding machines, desk calculators, the punch card, and the early, modern computers of the '50s. In line with Goodwin's plea and Ruggles' suggestion, this paper is about the initial impact of the modern computer on economic theory. The monetary area of theory receives special attention, but the impact in this area is quite broad, extending to the theory of relative prices and that of an exchange economy [5; and 8, Appendix to ch. 7]. The period of initial, noticeable impact is mainly in the '60s, as review elsewhere [8].

In broad outline, the enlarged flow of empirical results facilitated by the computer was more than the numerous, relatively static constructs of much of received economics could cope with. In essence, these were constructs that had expectations of economic events impounded in the ceteris paribus assumption, and excluded the treatment of uncertainty by the introduction of the assumptions of perfect foresight and information. These were problems some economists had treated before [e.g., 16, ch. 1]. The difference was the overwhelming impact of the computer.

The events of the '60s played a role, especially the persistence of inflation in the mid-1960s and early '70s, even in the presence of unemployment. They gave

* This paper is based partly on a recent book [8].
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monetarists an issue\(^1\), but the overwhelming results from analyses of data were more crucial. These, one may suggests [e.g., 8, and 33], brought about a crisis in Keynesian economics in particular\(^2\). The special attributes of Keynesian economics affected were [8, sect.1.3] the liquidity trap, the inelasticity of investment demand, and a unique emphasis of fiscal policy and the investment multiplier. In addition, there was the tendency to separate the analysis of prices from that of real output [8, sects. 5.4 and 8.2], the focus on the rate of interest as the control variable in monetary policy matters [8, ch. 4 and sect. 15.2], and the tendency to focus on home construction to illustrate the workings of monetary policy [8, sect. 3.4 and Appendix to ch. 15]. Another relevant tendency was that of using distinct and unrelated languages in the discussion of “monetary theory”, on the one hand, and “monetary policy”, on the other (as also indicated in part by the tendency to consider formal models and to manipulate them with little or no attention to empirical content and range of relevant applicability).

No detailed review of these matters is in order, but some comments may add perspective. In the one instance, the existence of a liquidity trap in the 1930s – i.e., the condition of a hoarding of an accelerated growth of money balances in anticipation of a rise in interest rates (and a decline in bond values) – was questioned on empirical grounds [10, ch. 4]. There was instead the prospect [8, sect. 5.4] that the normal or safe rate (as subjectively viewed by the behavioral unit) toward which low rates would gravitate might itself shift, especially under dire circumstances. In another instance, the reintroduction of and the empirical support for the expected rate of change in prices as a component of market rates of interest [8, sect. 1.4, and Appendix to ch. 1] helped weaken the attachment to the rate of interest as a directly, readily controlled variable\(^3\). Such results, as well, helped question the assumed independence of the rate of return on additional capital outlays and the rate of interest, as in the Keynesian investment demand block. The demise of the rate of interest as a causal factor – especially as an explanation of the post-World War II rise in velocity – had in fact been evidenced even earlier in the ’60s [8, sect. 4.2; and 9, pp. 118–121]. Instead, among others, a greater certainty thesis was suggested as an explanation [8, ch. 6]. The Phillips curve was offered by Keynesians as an appendage for treating prices, but the

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\(^1\) “Monetarist” is a label attached to those who gave almost singular importance in the 1960s to the money stock as a causal force. There are variations on the label with slightly different meanings, e.g., “weak monetarist”, Frédmanians, and so on.

\(^2\) Leijonhufvud [22] was an early author drawing a distinction between Keynes' and Keynesian economics – one being what Keynes actually said and the other being a parochial American version found in economics textbooks of the ’50s and ’60s. Since the publication of Leijonhufvud’s book, the distinguishing characteristic of Keynesian economics have received additional attention in a variety of papers, as reviewed elsewhere[8].

\(^3\) The rate of interest may still be viewed as a constraint on expenditures. But where planning on the part of decision makers to avoid destabilizing effects of tight credit is introduced – as in Galbraith’s “The New Industrial State” (1967) – the notions are quite distinct from the Keynesian ones.
prospect of its simply reflecting covariation, in lieu of causation, in the rate of change in prices and the percentage of unemployment was soon illuminated [8, sect. 1.3].

One might say that the foregoing were overly simplified views of the Keynesians. But the key elements of the views expressed were present even in more sophisticated structural equations models, as noted further below and as Teigen concedes in some instances [32].

In the questioning of orthodoxy in the ’60s, the outpouring of results from analyses of data was especially important, and their abundance was uniquely dependent on numerous, readily available modern computers. With them more data could be analyzed in more combinations than was dreamt possible, in the old archaic days before the computer in 1960. One of the first controversies bearing on the above issues centered about an early 1960s paper by Friedman and Meiselman [13]. The question raised was whether the investment or money multiplier was most relevant—i.e., whether “autonomous” expenditures or money was the better explanatory variable for consumer expenditures [8, ch. 5]. The participants in the controversy grew in numbers and between them they generated more regression results than would have possibly been undertaken with hand calculators. The participants themselves appeared to grow in methodological sophistication, even in the process of using the methods. The issue of results from simple and reduced form equations versus complicated structural equations models was planted in this early encounter. The role here of Ando and Modigliani was tied with their subsequent role in the Federal Reserve-MIT model [8, ch. 14].

The modern computer concerned the numerous adjustments to series (e.g., to obtain rates of change), the transformation of data, the enormous labor involved in experimenting with the Almon lag technique (e.g., while varying the lag time and the degree of the constraining polynomial), the construction of large scale models, simulations with large and simple models, the development of methods of solution for large simultaneous equations models, and the opportunity to split sample periods, re-estimate coefficients, and so on, with an abundance heretofore unimagined.

The point need not be labored. But the instability in the coefficients of the estimated equations and the apparent instability of the parameters of assumed probability distributions—as revealed by the re-estimations and so on—all this evidenced the apparent, ever-present nature of forces outside of economics. The early promise held for the large models was not realized. More realistic ambitions were substituted, and instead of emphasis on causal linkages, there was a shift to the emphasis on forecasting primarily. By the early 1970s, Carl Christ could write [6, p. 447]:

4 Bibliographies on this issue are available [e.g., 23]. Extensive ones on this issue and others of the ’60s are found elsewhere [8].
“Economic theory is not very useful for financial econometric models, and ... once we get a sufficiently large number of economic variables, it is not difficult (though it may be expensive and time consuming) to put together a system of economic equations that will tract their behavior reasonably well.”

Something of a positive nature did come forward from the computer facilitated outpouring of empirical results, notably: the crisis in theory, and the suggested need for devoting more attention to probabilistic aspects of behavior (i.e., to the treatment of uncertainty, probabilistic slanting, learning, and related psychological aspects of economic behavior). Even Brunner – who with Meltzer is viewed as attempting to avoid and suppress treatment of these phenomena [8, Appendix to ch. 7; and 4] – could conclude as follows, after surveying aspects of the monetarist revolution [1, p. 26]:

“It seems probable that the information assessment process governing the anticipated inflation velocity [i.e., the rate of change in prices] is not confined to actual price movements. This adaptive behavior absorbs much information bearing on the government sector’s financial behavior and the political constraints imposed on that behavior. Experiences beyond the past evolution of prices are quite important for any useful analysis of the anticipated inflation velocity. Unfortunately, we know very little about the detail of these important informational processes.”

The study of money (variously defined) came forward in the ’60s. This was facilitated in large measure by the computer, and in part by the perceptiveness of the empirically oriented theorists. It was precisely because the study of money is related to the sorts of forces orthodoxy could not cope with. Money, one may recall, is the link between the past and the future, the real goods and financial sectors of the economy. In other words, money is a way of dealing with uncertainty, foreseen and unforeseen developments, and a means of purchasing information about only probable occurrences of various states of nature and future conditions. The tendency for the orthodox of the ’50s and ’60s to overlook these links made their economics vulnerable to empirical research of the magnitude and character of that of the ’60s. To be sure, the computer changed the cost of empirical research to favor greater attention to it, with the results just described.

The above issues aside, the thesis of this paper is that those already proned toward the testing of economic theory may be expected to have come forward in the discussions of issues of theory and empirical economics in the ’60s. The method of the present paper is simple. The more strongly empirically oriented groups of the ’50s, with interest in economic theory may be identified in Section II of this paper. One may then ask whether in fact these groups do come forward to play a major role in the discussion, formulation, and communication of ideas under the initial impact of the modern computer. To reveal the plot, the answer is that they do, as evidenced by a review of the literature [8]. Those who fared best in the outcome,
however, are those who were prone to favour the unorthodox views receiving the greatest support from the computer.

Section III re-examines some of the folklore about the antecedents of Milton Friedman's approach, since in a review of the '60s, much is said about his ties with the National Bureau of Economic Research and about attributes of his work [8, sects. 1.1 and 5.1].

II. *Enclaves of Empiricism*

Enclaves of empirical thinking existed in economics in the pre-computer, hand-calcular days (i.e., mainly before the 1960s). These can be identified in the Federal Reserve System, at the University of California (Los Angeles), at the National Bureau for Economic Research, at the University of Chicago, among Keynesians, and at the Survey Research Center of the University of Michigan. Independent researchers with empirical interest abound, came to the forefront also in the '60s, but no passing comments will be directed presently toward them.

The Federal Reserve of course comprised no homogeneous group, and the Board in Washington and the St. Louis and New York Federal Reserve Banks may be singled out. Indeed, questions concerning informational detail and policy have interested officials in parts of the Federal Reserve System from its inception, and the issue of theory vs. policy comes to reflect the research and interest of Karl Brunner and his former student Allan Meltzer, initially in the UCLA setting. The National Bureau's thrust and orientation too has been tied in with Milton Friedman's group at the University of Chicago (8, chs. 3 and 5, and Appendix to ch.8]. The tie between empiricism and Keynesians is a more subtle one, and actually concerns only a small proportion of Keynesians, or at least the mass of economists converted to the rather parochial Keynesian approach of the '50s as delineated earlier.

Too many economists protected the sanctity of economic theory and, at the same time, appeased the policymakers by making a distinction between theory, on the one hand, and policy, on the other. The settlement was a tenuous one for two reasons: first, because economics from the days of Adam Smith has been thought of as justifying itself by serving as a guide to economic policy; and, second, because theory so removed from the empirical realities confronting policymakers could not stand for long as good theory, once large-scale, systematic efforts could be made in evaluating it. The Federal Reserve has continuously had an interest in information, institutional detail, and the publication and discrimination of facts, as indicated by the various issues and statistical segments of the *Federal Reserve Bulletin*, as published over the years. They enjoyed a flexibility of purpose and orientation that has doubtlessly contributed to the political survival and promi-
nence of the institution. But the distinction between theory and policy was, to repeat, tenuous.

Dissatisfaction over the theory and policy distinction [e.g., 2, pp.3-4, 112] reflects a part of Karl Brunner’s early interests in scientific method. The particular science orientation comes out strongest in his individual writing [1, 2, 3], and carries over to his coauthored work with his associate and former pupil, Allan Meltzer. Its particular attribute is not the effort found in some quarters to impound troublesome aspects of expectations in ceteris paribus, as if economics was a laboratory science. Rather it seems to center about a conscious use of logic, a gymnastic as referred to elsewhere [8, Appendix to ch. 7], to exclude appeal to “analytically extraneous sociological or psychological convolutions” [1, p. 4], even in the presence of psychological phenomena. The effort is reminiscent of the use of the term “scientism” [e.g., 31, pp. 17-18], to refer to an uncritical use of or copying of the methods of the physical sciences, for example, by economists. (Whatever the case, their particular approach gained the strong support of the National Science Foundation.) The Brunner-Meltzer objection to a “gap” between economic theory and monetary policy [8, ch. 10; 2, n. 1, pp. 3-4, 112; 4, p. 240], on the other hand, appears to be a highly practical kind of scientific interest in economics, that is, simply that of the empirical questioning of the relevance of a theory that cannot serve as a guide to (or explanation of) monetary policy.

Darryl Francis, as President of the Federal Reserve Bank of St. Louis, also has focused on the theory vs. policy issue in reviewing the research orientation of the Federal Reserve Bank of St. Louis [e.g., 7, pp. 3-4]:

“Those responsible for carrying out stabilization policies require considerable knowledge of the probable results of any particular course of action... Develop-

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5 These have been reported with special reference to Rudolf Carnap, a philosopher and logical positivist [8, Appendix to ch. 8], who conducted a philosophy seminar in Vienna (1922-31) and who came to UCLA during the early part of Brunner’s tenure there.

Logical positivism, as formulated in Vienna in the 1920s, has been viewed as a critique of language, and “its result is to show the unity of science— that all genuine knowledge about nature can be expressed in a single language common to all the sciences”. There is a role for empiricism, and it comes out in Brunner’s work and his more philosophical writing [e.g., 3]. In discussing rules for the market place for ideas, Brunner emphasizes the empirical content of theory [3, pp. 177-179]. He also draws on Carnap [3, n. 1], and semantic rules for relating “the (linguistic) entities of the formal structure [of a theory] with extra-linguistic entities of our observable world”.

6 Brunner seems especially fearful of the possible use of expectations theories of the short-run volatility of capital investment. He says [1, p. 6]: “They justify claims of innocence and achievement.” In the former case, they justify a disclaimer when a tidal wave has engulfed the economy and made it intractable to monetary impulses. In the latter, they can claim that their intelligent adjustments moderated the impact of volatile investment on the pace of economic activity. Continuing, Brunner says, “This theory obviously maximizes the political survival possibilities of a Central Bank’s bureaucracy”. And, indeed, any theory with such vagueness would. Even so, both the possible relevance of such a theory and its current vagueness, suggest the need for more precise study and formulation.

7 Brunner’s and Meltzer’s papers throughout the 1960s and early ’70s acknowledge support from the National Science Foundation.
ment of this knowledge requires empirical substantiation of various economic theories.”

Karl Brunner (often in work with Allan Meltzer), the Federal Reserve Bank of St. Louis, and Milton Friedman (often in work with Anna Jacobson Schwartz, and especially in publication under a National Bureau imprint), came to emphasize “first approximation”, special empirical relationships, and a search for regularity in the data, as distinct from a mass of detail in the form of a structure. In fact, Friedman’s apparent Machian orientation – as described elsewhere and as related to comment by Samuelson [8, Appendix to ch.8, n.4] – and his particular National Bureau orientation concern the relatively ad hoc character and broad, business conditions, leads-and-lags approach found in his work, as reviewed elsewhere [e.g., 8, ch.5].

The foregoing groups have emphasized rates of changes in stock and flow quantities that gave a rather dynamic quality to their work. The particular approach led Friedman and the St. Louis bank especially to emphasize the demise of the rate of interest as a determinant of the income velocity of money, all in contrast to the Keynesian approach. The St. Louis bank’s position is summarized thus [7, p.11]:

“One study in the mid-1960s found that interest rates have generally been high and rising during periods of rapid economic expansion and have been low and declining during periods of economic contraction. Although this behavior of interest rates appears to contribute to economic stabilization, the effect may not be great since the state of the economy itself appears to be the major influence on rates.”

The St. Louis Bank’s research efforts began to move toward national policy matters and extend beyond the traditional regional, public relations, data collecting interests of Federal Reserve Banks about 1960 [7]. Their effort was to be a challenge to the Board. This coincided with the initial thrust given to empirical research by the computer. Homer Jones, research vice president at the bank in the ’60s, provided direction. His empirical interests were evidenced early in his career. Others to join the St. Louis Bank’s staff, and give early direction to its research had exposure to Karl Brunner’s and Chicago influences.

8 Samuelson also comments on the Machian view in a passage to his lecture in Stockholm, Sweden, on the occasion of his receiving the Nobel Prize in 1970. That passage follows [28, pp.250–251]: “Seventy years ago, when the Nobel Foundation was first established, the methodological views of Ernst Mach enjoyed a popularity they no longer possess. Mach, you will remember, said that what the scientist seeks is an ‘economical’ description of nature. By this, he did not mean that the navigation needs of traders decreed that Newton’s system of the world had to be born. He meant rather that a good explanation is a simple one that is easy to remember and one which fits a great variety of the observable facts ... Mach is not saying that Mother Nature is an economist; what he is saying is that the scientist who formulates laws of observable empirical phenomena is essentially an economist or economizer.”

9 Katona [16] mentions his early interest in the consumer survey work supported by the Board of Governors.
The Board’s own view, at least since the ’30s, can be described quite broadly as a “banking view” (as defined elsewhere [8, chs.3 and 4], i.e., one with Keynesian attributes, with special interest in money market and institutional detail. This provided a natural tie with Keynesian interest and with the structural equations methodology – at least in the mid-1960s when that method was viewed as being most promising [8, ch.14] for identifying structural detail, causal chains, and linkages between the interest rate (as a major control variable) and national economic goals, all without much interference from forces operating exclusively of those allowed for in the model. The Board’s view in most of the ’50s and much of the ’60s was not different from that predominating at the Federal Reserve Bank of New York. The New York bank has always served as a foreign arm of the System, and as the center for open market operations. Its money market interests have been pre-eminent, and the banking view suited them quite well. In the econometric sphere, papers in the late ’60s and early ’70s by Hamburger have characterized their research stance [8, sects.10.4,13.2,13.3,15.1].

The Keynesian interest in structural equations and the special research approach to which the interest and method led in the ’60s is tied to the role of Lawrence Klein, as a Keynesian and an econometrician. Klein had written his dissertation on the Keynesian revolution [20] under Paul Samuelson in 1943–1944. In a chapter added in the revised dissertation, the essential nature of the Keynesian system as one of a simultaneous system of equations is stressed [20, ch. IX]. Klein, further, drew on the pioneering work of Tinbergen in the 1930s [e.g., 17, pp. 47–48; and 21] in his own work on multirelation models.10 He became identified as a pioneer in the application of structural equations methodology [e.g., 21]. His Wharton model [8, sect. 14.4] has been described by him [17, p. 42], as “a product of the Keynesian tradition based on the theory of income and employment determination”. The model places a primary emphasis on the real economy, fiscal changes, the interest rate as the control-transmission variable, and the short run. Brunner says [1, p. 14] Klein’s view of economic fluctuations as being conditioned by various events in the many parts of the system actually motivates the construction of “ever larger models with more and more equations”.

Probably because of Klein’s particular interest and orientation, he offered little new in the realm of economic as distinct from methodological ideas in the ’60s. The

10 Jan Tinbergen was co-recipient with Ragnar Frisch of the first Nobel Prize in Economics in 1969. In reviewing Tinbergen’s and Frisch’s contributions, Klein [18, pp.715–717] especially takes note of Tinbergen’s construction of a large-scale, mathematical statistical model that “did inspire and lay the groundwork for today’s models”. “These models,” Klein says [18, p. 717], “are in the tradition of Tinbergen’s pioneering models of the U.S. economy.” Continuing “They all pay substantial attention to money market phenomena, and this is one of Tinbergen’s special contributions; he had tried to capture the influence of the security markets in the U.S. expansion and decline of the 1920s and 1930s in his League of Nations model.”

Tinbergen’s introduction of the ideas of “targets” and “instruments” in policy models, as mentioned elsewhere [8, sect. 2.1], is also cited by Klein.
computer impact in his case was more strictly and simply that of forecasting with a structural equations model.

In the early '60s, partly in response to Friedman's and Meiselman's use of simple models \[8, sect. 5.2\], Ando and Modigliani became involved in rudiments of structural equations methodology. Both Ando and Modigliani were at MIT at the time, though Ando later joined Klein's department at the University of Pennsylvania (as reviewed elsewhere \[8, ch. 14\]).

During this same period, Frank de Leeuw took leave from the Board of Governors to return to Harvard (Ph.D. 1965). De Leeuw became involved in the Brookings Model, as introduced by Klein. He then became the main "in house" member at the Board, in the joint Federal Reserve-MIT model, with Ando and Modigliani serving as principal academic members. Thus the tie and the Board's initial venture into econometric model building.

There were numerous independent and most often young intellectuals involved in the exciting prospects for both old and new methods of research in the 1960s, as noted from time to time elsewhere \[8\]. But one not-so-young stood out in retrospect, as noted by Brunner [e.g., 2, p.3]. He has said [2, p.3]: "Clark Warburton deserves substantial credit... Throughout the 1940's, he insisted on important questions and relevant issues." And so Warburton had written [e.g., 35], but the present thesis helps explain his lack of impact on professional thinking, namely: his research efforts paralleled the older pre-computer, hand-calculator days. As Samuelson said of his own initial rise as a Keynesian [30, p.145]: "To have been born an economist before 1936 was a boon – yes. But not to have been born too long before."

Still another enclave of empiricism one might mention would encompass the Economic Behaviour Program of the Survey Research Center, University of Michigan, under the direction of George Katona, since its establishment in 1946. Katona's work and that of the center had been supported by foundations and business firms, and particularly the Board of Governors of the Federal Reserve financed its survey of consumer finances from 1946 through the '50s. Katona's and his group's special contribution was to be an empirical study involving the survey research method, and the introduction of an up-dated psychology into economics\[11\].

Katona's work too lacked any real impact on economics, broadly viewed, in part because it was offered without the hindsight of the experience with empirical research of the '60s (i.e., without the results from the initial impact of the computer), and in part because it offered no specific tie with the structure of economic analysis\[12\]. Katona [16, p.7] viewed economic psychology as non-

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11 The survey work was transferred to the Bureau of the Census in 1959, and changed from an annual to a quarterly survey, but only on an experimental basis.

12 Katona [16, p.7] actually hedged against such charges and recognized the overwhelming nature of the interdisciplinary work he undertook.
axiomatic, almost as a "controlled observation," but he [16, p. 9] also recognized it could be developed by studying economic behavior, and this broadened the field a bit. His, one might say, was exactly the laboratory concept implicit in classical regression and structural equations models. A paradox in fact centers about his special interest in learning, for it is precisely a possible tendency for economic units to learn and respond to shocks that get revealed as a source of inadequacy in the regression and structural equations approaches of the '60s. One might say that notions of learning from the past and the non-repetitiveness of some events alone suggest the prospect of continuously sampling from a different universe. Hence, the instability in the parameters of the characteristic economic models of the '60s [8, ch. 14].

To be sure, however pioneering Katona's work, it was superficial as far as the structure of economics was concerned. Views on learning, expectations, and uncertainty lacked the formal structure provided by Bayes' theorem and related topics surfacing in modern economics [8, ch.8]. There was early emphasis on expectations and inflation, in a statement of some principles [e.g., 25]: "small price advances, initial perceptions of price advances, or large price advances stimulate advance buying; and when price advances persist, as during so-called "creeping inflation", consumers may react by reducing the flow of their discretionary purchases. Well, these are precisely the sorts of notions one encounters in the '60s when giving recognition to the role of an increase in the rate of change in prices as the source of stimulation for expenditures, rather than to a constancy of the rate of change [8, ch. 1]. Even so, a problem from the point of view of the acceptability of such notions, however, was that they were offered without structure, e.g., [8, ch. 1] as provided by a general equilibrium framework, the Phillips curve (exclusive of course, from the debate about whether the curve is simply a reflection of short-run covariation in the percent of unemployment and the rate of change in prices or a valid causal relationship), Fisher's equation equating the nominal rate of interest to the real rate plus the expected rate of change in prices, and the adaptive expectations model relating an expectative magnitude to the weighted average of magnitudes for recent and past changes (all with the prospect of changing weights)\textsuperscript{13}.

\textit{III. Friedman: An Enigma}

Friedman has been viewed as an empirical scientist, as one who rose to prominence in a highly organized professional setting [8, sect. 3.2], and as a leader with a devoted and loyal following, especially among his students. A good bit of his special rise to prominence in the '60s has been attributed to a combination of the

\textsuperscript{13} Mincer [24] shows the adaptive expectations model to be a general form of several other closely related models.
presence of the modern computer, and a pre-computer, pre-disposition toward empirical research, as represented by Arthur F. Burns and the National Bureau. Other sources of influences too are involved, especially in the area of monetary study, but controversy arises about preconceptions with respect to them [e.g., 11; 26].

In the light of controversy over intellectual antecedents, in the light of strong emphasis Friedman himself places on the differences in methodological points of view separating himself and his protagonists [e.g., 11, pp. 906-908, 921, 925, 933], and in the light of a history of controversy concerning empiricism in economics [8, Appendix to ch. 8], it is surprising that no attention has been given to the source(s) of Friedman’s strong empirical interest by Friedman watchers. In this respect, Friedman himself [11, p. 908, 920] mentions a Marshallian as distinct from a Walrasian approach in the use of economic theory and identifies himself and Keynesmore with the former and some of his protagonists with the latter. “Keynes,” he says [11, p. 908], “was a Marshallian, an empirical scientist seeking a simple, fruitful hypothesis”. He then views, for example [11, p. 908, 933], Patinkin and Tobin as Walrasian, as seeking “a general and abstract system of all-embracing simultaneous equations”.

These labels – “Walrasian” and “Marshallian” – were first used by Friedman in a 1949 paper (“The Marshallian Demand Curve”, Journal of Political Economy, reprinted in Essays in Positive Economics [12]). The main points, then, as in 1972, were methodological. For one thing, he said the distinction between Marshall’s analysis as “partial equilibrium” and Walras’ as “general equilibrium” was spurious. Marshall had his system of equations and unknowns. The distinction, then, lies in their purpose. For Marshall, Friedman said, theory was to lead to a body of substantive hypotheses. Keynes was placed in this tradition too. For Walras and the later Keynesians, however, the emphasis was on structural detail. And one may add, in a relatively static framework. The computer revolution has compounded this latter distinction, brought Friedman to the forefront, and precipitated a crisis in theory. Indeed, a crisis extending to the methodology of economics. The more dynamic elements of analysis come to the forefront, but still with some emphasis on detail.
one centering about the quantity theory of money and the teachings of Henry Simons, Lloyd Mintz, Frank Knight and Jacob Viner. One of Chicago’s products of the pre-Friedman period, however, says that Friedman has been responsible for unjustifiably perpetuating this notion of an “oral” Chicago tradition that preceded himself [26, p. 48]. In terms of an examination of the tradition at Chicago before Friedman by Patinkin [26], and in terms of an examination of the main characteristics of Friedman’s approach and an examination of its antecedents, the conventional and prevailing notion about Friedman’s tie with the tradition simply does not hold up in some ways. Friedman’s Chicago was not in some major respects the “other” Chicago (that of Simons, Mintz, Knight and Viner), as summarized by Patinkin [26, pp. 50-51]. Of these characteristics, only selected elements are parts of Friedman’s characteristics, namely, the money stock is the control variable, and money is important. This contrast with the Keynesian emphasis on the interest rate as a crucial link in the transmission mechanism, and on interest rate elasticities (notably the trap, and inelasticity of investment demand), and again Friedman readily recognized such [11, pp. 934-935, 944-945].

As brought out from time to time, Friedman’s approach is primarily characterized by the following [8, chs. 1, 3, 4, 5, 10, 16, and Appendices to ch. 7]: a portfolio type of approach (the ideas of wealth and asset adjustments working through a general equilibrium framework, and associated changes in the velocity of money relating in turn to imbalances between desired and actual money balances), a demand for money concept with the public’s adjusting its desired holdings of money in relation to income by varying the rate of expenditures and possibly the price level, a liquidity approach to the demand for money with stress on the conditions of demand (in part as distinct from a medium of exchange or transactions demand approach with stress on conditions of supply), emphasis on underlying trends or permanent income as represented by an exponentially declining weighted average of current and past income measures, the Fisherian equation with the money rate of interest as the sum of the real rate and the expected rate of price inflation or deflation, and a strong (even if at times disguised) interest in expectations as in the permanent income notion and in emphasis on uncertainty. The historically perverse influences of monetary policy, alleged unpredictable influences of fiscal policy, and the prospect of greater certainty resulting from greater stability in the rate of growth of the money stock, all are additional aspects of Friedman’s approach.

In defending some of his early statements about his tie with a pre-Keynesian tradition at Chicago, Friedman does several things. Among these he makes a distinction between an Austrian view of the quantity theory of money as discussed

15 Friedman joined the faculty at Chicago in 1946, the year he received a Ph.D. degree from Columbia University. He was at that time thirty-four years of age. He received an A.B. (Rutgers U.) and a M.A. (U. of Chicago) degrees in 1932 and 1933, respectively.
at the University of London, and a view as discussed at Chicago and coincidently by Keynes too. The former apprently led to a relatively austere policy [11, p. 936], e.g., "the only sound policy was to let the depression [i.e., of the 1930s] run its course, bring down money costs, and eliminate weak and unsound firms". In contrast, the old Chicago group moved toward policy actions [11, p. 937-939]. In addition, "Keynes’ discussion of the demand curve for money in the ‘General Theory’", Friedman says [11, p. 941], "is for the most part a continuation of early quantity theory approaches, improved and refined, but not basically modified."

By such a diverse route, but not going so far as to embrace the liquidity trap, Friedman is tied with Keynes’ economics, if not the Keynesian economics of the early 1950s, toward which he was somewhat “hostile” [11, p. 936]. Thus, to some extent, as Patinkin has pointed out as best his evidence will permit (mainly written work, his lecture notes as a student at Chicago, 1943-45, and an examination of doctoral dissertations at Chicago in the monetary area from 1930 to 1950), some of the foregoing characteristics of Friedman’s work have more direct antecedents in Keynes’ work\(^\text{16}\) than in a pre-Friedmanian, “oral” or written tradition at Chicago. The antecedents he emphasizes are the portfolio approach and “Keynesian” theory of liquidity preference [26, pp. 47, 54, 57].

Why, then, would Friedman claim in his earlier work (mainly, “The Quantity Theory of Money – A Restatement”, 1956) to a tradition at Chicago? For one thing, Patinkin has suggested [26, p. 6] that Friedman simply did not initially recognize his intellectual indebtedness to aspects of Keynes’ work. In fact, Friedman’s reactions to his critics [11] does suggest that he has not had the time to sort out, from a history-of-doctrine point of view, the antecedents to his thinking. Harry Johnson has further suggested the need to meet his second criterion for the success of revolutionary intellectual change (notably [15, pp. 4, 8-9], that a theory should appear to be new, but also appear to have roots in a valid orthodoxy). Friedman, of course, does not like this inference about his conscious or subconscious motives. He says [11, p. 941], the “Chicago tradition” is clearly not something he invented “for some noble or nefarious purpose”.

**IV. Summary**

The modern computer brought about an outpouring of empirical results in the initial period of its impact. These were ominous for the Keynesian orthodoxy of the ’50s. This was largely because it had subordinated a consideration of monetary

\(^{16}\) Patinkin [26] does not distinguish between Keynes’ economics and Keynesian economics as done elsewhere [e.g., 8, Ch. 7]. At times he stresses Keynes’ “General Theory” and “Treatise”, but at times he clearly fails to distinguish between Keynesian economics. He brings in the Keynesian proposition whereby the interest rate exerts special causal influence on the velocity of money and switching between money and bonds [26, pp. 60–61], without referring to Friedman’s emphasis on the demise of the rate of interest as a causal factor.
phenomena in the interest of fiscal policy, had impounded expectations in ceteris paribus, and had excluded the treatment of learning and related probabilistic aspects of economic phenomena, often by invoking assumptions of perfect information and foresight. This view of economics led some to an increasing focus on structural detail and to reliance on structural equations methods of the 1960s vintage. The emphasis on detail and structure also had special appeal to groups inside the Federal Reserve.

In the '60s, the initial claims for structural equations models – especially with their Keynesian underpinnings – did not fair well. Numerous, empirically oriented theorists received attention. Even so, those who stood out were most favored by the failures of the orthodoxy. Expectational, probabilistic phenomena came to the forefront – particularly as related to the study of money, and as calling for the further attention of economists.

The groups that were predisposed toward the study of business conditions, monetary phenomena with empirical orientations, gained attention. There were different methodological notions about in the '50s, but the '60s favored those who questioned the separate considerate of theory, on the one hand, and policy, on the other, because in an interpretation of reality the two are entwined. The computer forced the consideration of reality, if not in detail, certainly in the abstract. Notable, in the wake of its enslaught in the '60s, have been Milton Friedman and through him the National Bureau of Economic Research. In enumerating groups, one should also include Brunner’s and Meltzer’s, and the Federal Reserve Bank of St. Louis.

There were other empirically oriented groups of prominence in the '60s, but because of their special orientation, their contribution was more obviously in the area of experimentation with method, even if that most congenial with their preconceptions. Here one notes the prominence of Lawrence Klein’s Warton and Brookings model groups. There is too the Federal Reserve-MIT project with Ando and Modigliani from the academic community and de Leeuw at the Board of Governors in the formative stages of the project.

Others had been oriented at earlier times toward the treatment of topics that were favored by developments in the '60s. The difference in the '60s was the outpouring of empirical results from the modern computer. In addition, in the period, even those opposing the orthodoxy sought to tie their work with some structure, as so often favored by economists.
Selected References


Summary

*An Assessment of the Impact of the Computer*

Several economists have stressed the need for an extension of the analysis of forces shaping economic theory. The modern computer is one such force. There is the idea that empirically oriented economists with interest in economic theory might be expected to come forward in the controversies surrounding economic theory under the initial impact of the modern computer. To evaluate the idea enclaves of empiricism in the more immediate pre-computer period are identified. These concern Milton Friedman, among others. The plot is that they do in fact gain special professional attention. The monetary area is stressed because the impact was expected to occur there first, due in part to the availability of masses of data. The initial impact, however, is viewed as potentially extending to the theories of relative prices and an exchange economy, and hence economics broadly viewed. This impact is viewed elsewhere (*Crisis in Economic Theory*, University of Florida Press, 1973) as a force contributing to the crisis of the 1960's in economic theory. *Schweiz. Z. Volkswirtsch. u. Statist.*, Dec. 1973, 4 (109), (English). University of Florida, Gainesville, Florida.

Zusammenfassung

*Die Wirkungen des Computers auf die ökonomische Theorie*

Verschiedene Ökonomen haben betont, dass es eine Notwendigkeit ist, zu untersuchen, welchen Einfluss die modernen technischen Hilfsmittel bei der Entwicklung auf die ökonomische Theorie haben.


Die Wirkungen des Computers erstrecken sich jedoch auf weitere Gebiete der Ökonomie; diese werden in einer Arbeit über die Krise der ökonomischen Theorie in den sechziger Jahren behandelt.

Résumé

*Une évaluation des effets de l'ordinateur moderne*

Plusieurs économistes ont mis l'accent sur le besoin d'une analyse des forces qui façonnent la théorie économique. L'ordinateur moderne fait partie de ces forces. Il existe l'idée que les économistes d'orientation empirique qui s'intéressent aux controverses en théorie économique pourraient prendre une position sur les effets initiaux de l'ordinateur moderne. Pour évaluer cette idée, nous identifions le groupe d'économistes empiriques dans la période qui précède à peine l'âge de l'ordinateur. Milton Friedman en fait partie, parmi d'autres. Nous trouvons que ce groupe, en fait, reçoit une attention particulière dans la profession. La spécialité monétaire est l'objet d'intérêt particulier, car l'on s'attendait à y voir les effets initiaux en grande partie à cause de la disponibilité d'une quantité énorme de données statistiques.

Nous prenons le point de vue que l'effet initial s'étend potentiellement aux théories des prix relatifs et de l'économie d'échange, et par conséquent, à l'ensemble de la discipline économique. Ce point de vue est exprimé plus longuement dans *Crisis in Economic Theory* (University of Florida Press, 1973). En somme, nous tirons la conclusion que l'ordinateur moderne est une force qui a contribué à la crise en théorie économique des années soixante.