

The Cowles Commission and the Emerging Chicago School:

Conflicting Economic Methodologies at the University of Chicago in the late 1940s and early 1950s

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Abstract: Under the directorship of Jacob Marschak (1943-48) and Tjalling Koopmans (1948-55), the Cowles Commission at the University of Chicago sponsored pioneering work on general equilibrium, social choice, activity analysis, and simultaneous-equations econometric models by Kenneth Arrow, Gerard Debreu, Trygve Haavelmo, Leonid Hurwicz, Lawrence Klein, Harry Markowitz, and Herbert Simon, all, like Koopmans, future Nobel laureates. The Cowles Commission’s methodology was contested by the emerging Chicago school of economics, led by future Nobel laureates Milton Friedman and Theodore Schultz (department chair 1946-61) who upheld partial equilibrium price theory, the quantity theory and rational choice against the Cowles emphasis on general equilibrium and Keynesian macroeconomics and against Simon’s bounded rationality, and who were suspicious of Cowles projects on activity analysis and macroeconomic models as creating tools for central planning and Keynesian demand management. We examine the interactions and methodological debates between the two groups, which led to the departure of the Cowles Commission to Yale University in 1955.

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Introduction: The Cowles Commission and the Emerging Chicago School

The Cowles Commission for Research in Economics, a non-profit research organization affiliated with the Econometric Society and sharing the Society's commitment to advancing economic theory in its connection to mathematics and statistics, moved from Colorado Springs to Chicago in 1939. The Commission then became also affiliated with the University of Chicago, with Theodore Yntema, a statistics professor in the Business School, becoming research director of the Commission and with an advisory committee named by the University of Chicago replacing the advisory council chosen by the Econometric Society. When Jacob Marschak came from the New School to succeed Yntema as research director in January 1943, he was also appointed a professor of economics. Tjalling Koopmans, recruited by Marschak as a research associate in 1944, became an associate professor in 1946, with promotion to full professor when he succeeded Marschak as research director in 1948. After World War II research associates of the Cowles Commission usually also held faculty appointments¹ at the University of Chicago (unlike research consultants, who were at other universities). Under the directorships of Marschak from 1943 to 1948 and Koopmans from 1948 to 1954, the Cowles Commission in its Chicago years sponsored pioneering work on economic and financial theory, mathematical economics and econometrics: Trygve Haavelmo (1944) on the probability approach to econometrics, Koopmans (ed., 1950) and Hood and Koopmans (ed., 1953) on statistical inference in simultaneous-equations time-series models (LIML and FIML, and the rank and order conditions for identification of linear structural models), Lawrence Klein's Keynesian econometric models of the US economy (Klein 1950), Koopmans (ed., 1951) on activity analysis and linear programming, Kenneth Arrow (1951) on social choice, Arrow

¹ The 1950-51 annual report of the Cowles Commission listed John Gurland as assistant professor in the Cowles Commission and assistant professor of statistics in the Committee of Statistics, Clifford Hildreth as associate professor in the Cowles Commission and research associate (associate professor) in the Department of Economics, and Leonid Hurwicz as research associate at Cowles and visiting professor in the Department of Economics (until he became research professor of economics and mathematical statistics at the University of Illinois in January 1951), but Gerard Debreu just as assistant professor in the Cowles Commission and Morton Slater as "a research associate of the Cowles Commission (with the rank of assistant professor)."

and Gérard Debreu (1954) and Lionel McKenzie (1954) on proving the existence of Walrasian general equilibrium, Harry Markowitz (1952) on portfolio choice. In addition to Arrow, Debreu, Haavelmo, Klein, Koopmans and Markowitz, other future Nobel laureates on the staff of the Cowles Commission in Chicago as research associates and research consultants were Leonid Hurwicz and Herbert Simon, contributors to the Cowles volumes edited by Koopmans, and Franco Modigliani, who had written his PhD dissertation with Marschak at the New School. Don Patinkin, another Marschak doctoral student, wrote his first four articles in monetary economics at Cowles. In addition to these economists, the mathematical statisticians T. W. Anderson and Abraham Wald published major in Cowles conference volumes in the Chicago years. Under Marschak and Koopmans, the Cowles Commission became the heir to a European tradition of econometrics and mathematical statistics, with Eugen Slutsky's and Emil Lederer's student Marschak, Jan Tinbergen's student Koopmans, Ragnar Frisch's student Haavelmo, and Abraham Wald and Karl Menger from Menger's Vienna mathematical colloquium. With Herbert Simon and with George Katona (author of a 1945 Cowles monograph), the research of the Cowles Commission also had a strand of what became behavioral economics.

One might expect that such a record of achievement would be welcome to any university department but the Cowles Commission and its distinctive, then-unusual approach to economics was anathema to another highly-productive group within the University of Chicago's economics department, a group that was also to have great influence on the development of economics but from a different perspective: the emerging "Chicago school" of economics led by Milton Friedman (hired as associate professor in 1946, see Mitch 2016) with the support² of Theodore Schultz (department chair from 1946 to 1961) and,

² Notably in dissuading James Tobin from accepting the directorship of Cowles in 1954, yet, according to Mitch (2016), Schultz joined Marschak, Koopmans and Lange of the Cowles Commission and Paul Douglas (of the Cobb-Douglas production function, soon to be a liberal Democrat in the US Senate) in 1946 in ranking J. R. Hicks first and Paul Samuelson second for the position that ultimately went to Friedman, while Frank Knight, Lloyd Mints and Henry Simons, all associated with "Chicago school" opposition to Keynesianism, mathematical economics, econometrics and general equilibrium, all ranked Samuelson fifth out of five on the short list. Mitch quotes a July 23, 1946 letter from Friedman to W. Allen Wallis attributing Marschak's support for Samuelson to Marschak's

outside the department, W. Allen Wallis (dean of the Business School), committed to free markets, Marshallian price theory, rational choice and the quantity theory of money in continuation of the economics of Frank Knight, Henry Simons, Lloyd Mints and Aaron Director (see, e.g., Miller 1961, Patinkin 1981, Reder 1982, Mirowski and Plehwe, eds., 2009, Emmett, ed., 2010, Rivot 2013, Cord and Hammond, eds., 2016, Colander and Freedman 2019)³. The two groups disagreed about the role of mathematics and general equilibrium analysis in economics⁴, about how to analyze data, and about the usefulness of econometric models as guides to economic policy and especially as guides for Keynesian demand management for macroeconomic stabilization, a function of modeling celebrated by Marschak (1953) and Klein (1947).

In his memoirs (Friedman and Friedman 1998, p. 197) Milton Friedman proudly recalled that “I developed a reputation as something of a hair shirt since I was, and still am, a persistent critic of the approach to the analysis of economic data that became known as the Cowles approach.”⁵ Expressing a then widely-shared opinion, Friedman stated that “I believe that mathematicians, whether pure mathematicians or economists or statisticians, tend to be favorable to central planning ... When they enter a field like economics, they carry over the belief that all problems have clear-cut solutions and that

preference for “quasi-mathematical analysis of a Keynesian ... type” (item 12 in online appendix to Mitch 2016). Friedman wrote to Arthur Burns on March 3, 1946 that, according to Knight and Mints, Schultz was now “pretty well sold” on hiring Friedman, whom he had not met before (item 8 in online appendix to Mitch 2016).

³ Jacob Viner recalled that “It was not until after I left in Chicago in 1946 that I began to hear rumors about a ‘Chicago School’ which was engaged in organized battle for laissez faire and ‘quantity theory of money’ against ‘imperfect competition’ theorizing and ‘Keynesianism.’ I remained skeptical about this until I attended a conference sponsored by University of Chicago professors in 1951” (quoted by Patinkin 1969, p. 112, Colander and Freedman 2019, p. 69). Viner’s move to Princeton created the opening filled by Friedman.

⁴ Robert Lucas, in Cord and Hammond, eds. (2016, p. 11), remarks that when Friedman began teaching at Chicago in 1946, “Chicago Ph.D. students still had to petition to substitute a calculus course for one of the two foreign language requirements. By 1960, much of the complex diagrammatic exposition of *Price Theory: A Provisional Text* [Friedman 1962] was already outdated and the good students knew it. There were other books and teachers to turn to what we then called ‘mathematical economics’ and now call simply ‘economic theory’” and that Friedman (1962) emphasized “what he dislikes about Walras more than anything he has learned from Marshall.”

⁵ According to Epstein (1987, p. 107, citing minutes of Cowles staff meeting on 20 September 1946), “Friedman constantly intimated in the [Cowles] seminars that the estimation results discussed there merely reflected the prejudices of the investigator, prompting Koopmans to exclaim at one point, ‘But what if the investigator is honest?’”

they are competent to find them” (Friedman and Friedman 1998, p. 262). The two groups quarreled over how to approach economic theory (Marshallian partial equilibrium versus Walrasian general equilibrium), about empirical methodology, and over hiring, as Friedman and his associates fended off Cowles-inspired attempts to recruit Paul Samuelson⁶ and James Tobin, preferring to bring in George Stigler, close to Friedman ever since they were Chicago graduate students. Methodological disagreement overlapping with contrasting views on public policy and with personal disputes, as when Tjalling Koopmans’s “Measurement Without Theory” (1947) critiqued the work of two of Friedman’s former teachers⁷, Arthur Burns and Wesley Mitchell (1946), and Friedman (1950) responded with an elaborate argument that Mitchell’s work was economic theory, just not the kind of theory fashionable at the Cowles Commission. Despite this multi-faceted friction, Friedman ingenuously stated that the only reasons he could think of for the Cowles Commission to leave were “a combination of [Alfred] Cowles’s ties to Yale, of which he was a graduate, and financial incentives offered by Yale that Chicago was unable to match⁸” (Friedman and Friedman 1998, p. 198). On October 9, 1954, Friedman wrote to Arthur Burns that he was “glad to report that the rumors are entirely true” that the Cowles Commission was leaving Chicago for Yale: “Poor Yale” (item 18 of the online appendix to Mitch 2016).

⁶ Samuelson declined offers from the University of Chicago in 1947 and 1949. Stigler wrote to Friedman in January 1949 that “I’m inclined to write him [Samuelson] off as an economist. Two of his recent jobs ... were pure mathematical exposition, as is his current *Economica* item” (Hammond and Hammond 2006, p. 97; Colander and Freedman 2019, p. 185). Samuelson (2011, p. 158) recalled that “Mathematics, which I was beginning to get interested in [as a Chicago undergraduate before World War II], was laughed at by the Knight wing. Chicago was happy when the Cowles Commission left Chicago after the war, and they left because they felt that it was a hostile environment” (Colander and Freedman 2019, p. 185). Samuelson expressed concern that continual debate with Friedman would both push him leftward and divert his time from scientific research to polemics.

⁷ Friedman had studied statistics with Burns at Rutgers and economics and statistics with Burns and Mitchell at Columbia (where he wrote his dissertation with Simon Kuznets) as well as economics at Chicago.

⁸ Any financial incentives pledged by Yale were minor compared to Yale’s insistence that Alfred Cowles provide an endowment (hence the change from a commission, funded by annual donations, to an endowed foundation). Although Alfred Cowles 3rd had graduated in 1913 from Yale (where his father and uncle had graduated in the 1880s, before the 1892 founding of the University of Chicago), he had close, lifelong business ties to Chicago as grandson of one of the founders of the Chicago *Tribune*, and he remained in Chicago when the Cowles Commission left. In 1937 he had rejected an invitation by Yale economics professors James Harvey Rogers and Edgar S. Furniss (the latter then provost of the university) to bring the Cowles Commission from Colorado Springs to Yale.

The intensifying friction within the Department of Economics contributed to the departure of the Cowles Commission, which in 1955 became the Cowles Foundation for Research in Economics at Yale University. In 1953, with Koopmans scheduled to take the sabbatical during which he began *Three Essays on the State of Economic Science* (1957), Marschak, Koopmans and Alfred Cowles invited James Tobin of Yale (then as much an econometrician as a monetary economist, known for the Tobin estimator of limited dependent variables) to come to Chicago to succeed Koopmans as research director of the Cowles Commission. “Although the Cowles appointment carried with it a professorship in the University of Chicago economics department, when I asked the chairman [Theodore Schultz] if the department would have been interested in me without the Cowles connection, he said ‘No’” recalled Tobin (quoted in Dimand 2014, p. 14)⁹. When Tobin telephoned Koopmans to decline the Cowles offer, Koopmans inquired whether Yale would be interested in his spending his 1954-55 sabbatical at Yale. The transformation of the Cowles Commission at the University of Chicago into the Cowles Foundation at Yale was negotiated during Koopmans’s sabbatical.

The conflict between the two rival approaches within the Chicago department, and its role in the move of Cowles from Chicago to Yale, has received limited attention in the literature on Cowles. Clifford Hildreth (1986) dealt with the topic in one sentence: “The Commission’s general equilibrium-Keynesian orientations and methodological approaches sometimes produced sharp disagreements with other Chicago economists – see Reder (1982); Friedman (1946); Patinkin (1981), Chapters 7, 10, 11.” Carl Christ (1994, p. 35) gave not quite half a page to the subject, quoting two verses about Friedman from a song at a 1949 Cowles Commission party but also quoting, in a footnote, Melvin Reder (1982, p. 10) on

⁹ In contrast to Tobin’s recollection of why he declined, Clifford Hildreth (1986) did not mention conflict within the department as a reason for the departure of the Cowles Commission, stating only that “Offers [to succeed Koopmans as research director] were made to two outstanding prospects [Tobin and Kenneth Arrow], who declined, citing difficulties of raising families in Chicago as principal reasons” (1986, p. 12). Apart from his sentence stating that there were Chicago critics of Cowles approaches (pp. 62-63), Hildreth (1986, pp. 56, 102, 109) mentioned Friedman only in a list of discussants of Christ (1951a) and as a coauthor with L. J. Savage on the curvature of the utility of wealth function.

“a fairly intense struggle” at Chicago between the two groups of economists. Marcel Boumans (2016, p. 602) considers that “the history of the Cowles period at Chicago was almost forgotten.”

The eminence of the two rival groups is shown by a 1957 reputational survey among department chairs, which found that the three highest-ranking economics departments at US universities (that is, excluding specialized institutes such as MIT and California Tech) were Harvard, the University of Chicago and Yale University¹⁰. The eminence of the two groups of economists was underlined when Tjalling Koopmans and Milton Friedman won the Royal Bank of Sweden Prize in Economic Science in Memory of Alfred Nobel in consecutive years. Chicago school suspicions of activity analysis and Cowles research as tools of planning were echoed in Koopmans sharing the 1975 prize with a Soviet “planometrician,” Leonid Kantarovich, while the Chicago school would find it appropriate that Friedman was honored in the bicentenary of both the Declaration of Independence and *The Wealth of Nations*. The Chicago years of the Cowles Commission have faded out of the history of Chicago economics, except to inflate the count of “Chicago” Nobel laureates in articles celebrating the achievements and influence of the Chicago school, without mention that many of the laureates were distinctly outside the Chicago school (e.g. Liaquat Ahmad 2019, p. 27¹¹).

How Friedman and the Cowles Commission Viewed Each Other

Despite the strong differences, the leaders of the two groups able to appreciate each other’s strengths. In a February 28, 1946, memorandum to University of Chicago Chancellor Robert Maynard

¹⁰ The economics portion of Hayward Keniston’s 1957 survey “Standing of American Graduate Departments in the Arts and Sciences” is on “Economics in the Rear-View Mirror,” www.irwincollier.com and is taken from Hayward Keniston, *Graduate Research in the Arts and Sciences at the University of Pennsylvania* (January 1959), pp. 115-19, 129. James Tobin stated in 1999 that with the arrival of Cowles, “We soon became a major department in the country, one of the top four or five, whereas Yale in 1950 [when Tobin arrived] had not ranked at all among major departments of economics” (“Yale. Ruggles, Tobin, Parker, Peck, Levin, and Brainard Muse About Their Economics Department, 1999,” www.irwincollier.com).

¹¹ Ahmad (2019, p. 27) also dates the founding of the University of Chicago economics department to the 1930s, but that was only when the department, founded with the university in 1892, changed its name (but not the name of its journal) from political economy to economics.

Hutchins (item 9 in the online appendix to Mitch 2016), Cowles research director Jacob Marschak proposed “to invite both [Samuelson and Friedman], not as competitors, but as candidates for two positions” and held that “If we get both Friedman and Samuelson [underlining in original], we shall have at Chicago a most formidable team. Our University will then easily compete with Cambridge whose economics has grown because of another pair of opponents (Pigou vs. Keynes) ... economics in its really important problems will be advanced by responsible and clear thinking and vigorous testing, and not by glorified politics and ill-reasoned axe-grinding. Of the two men, Samuelson is a most powerful system-builder, Friedman a strong critic of detail [underlining in original]. Both are extremely intelligent... It so happens that Friedman is an obstinate partisan of the old tradition, so that the appointment of both men will just preserve the present dis-equilibrium. Since both are sincere thinkers and not shallow politicians, a hearty controversy between them will not do the harm it usually does between men who have more respect for faith than truth, and who refuse to face an argument if it threatens to lead to unpleasant conclusions.” Marschak wished to make job offers to both men (as was eventually done) despite the contrast that “Samuelson’s numerous publications show the mark of creative genius” while “Friedman has published little of note, presumably because of the deadening atmosphere of crass empiricism [at the NBER] in which he spent his last 10 years; and because he indulges in destruction more than in construction.” Marschak emphasized that Friedman’s “contributions to statistics proper are more interesting, and his role during the war in the development of mathematics of quality control quite remarkable.”¹²

¹² Friedman’s demonstrated proficiency in statistics and in the mathematics of quality control presumably made him more acceptable to the Cowles-affiliated members of the department than Friedrich Hayek, a scholar of similarly free-market views, who joined the Committee of Social Thought rather than the Economics Department, with even a motion for a courtesy appointment in economics not being approved by the department – see items 14 (Director to Hayek, July 1948), 15 (Hayek to Director, August 1948) and 16 (on the November 1948 motion) in the online appendix to Mitch (2016). Frazer and Boland (1983, p. 133 n. 8) quote a 1940 letter from Friedman, replying to Canadian statistician Nathan Keyfitz concerning Friedman’s review of Tinbergen (Friedman 1940), in which Friedman remarked to Keyfitz that “I have never been able to understand his [Hayek’s] dogmatic insistence on the proposition that statistical data could never verify economic hypotheses.”

In a four-page memorandum to J. H. Willits stamped as filed by the Rockefeller Foundation in May 26, 1948 (but dated by Boumans 2016, p. 597, as written in September 1947), Milton Friedman gave “In accordance with your request ... my general views about the possible value of the approach towards the study of economic fluctuations¹³ that is being followed by the Cowles Commission” (Rockefeller Foundation Records, Rockefeller Archive Center). The Rockefeller Foundation, which had rejected an earlier grant application from the Cowles Commission (see Alfred Cowles 3rd to Jacob Marschak, April 20, 1938, in Marschak Papers at UCLA), had long financially supported the statistical approach to business cycles by Wesley Mitchell and Arthur Burns at the NBER. Joseph Willits of the Rockefeller Foundation was among the speakers at the Columbia University memorial for Mitchell (Burns, ed., 1954, pp. 143-48) and wrote the memorial article on Mitchell in *Science*, the journal of the American Association for the Advancement of Science, of which Mitchell had been president.

Friedman wrote to Willits that “The major articles of faith underlying the Cowles Commission work are first, the belief that it is currently possible to construct a comprehensive quantitative model for the economy as a whole from which it will be possible to predict with a reasonable degree of accuracy the future course of economic activity; second, the belief that this quantitative model should take the form of a system of simultaneous equations of a special kind (namely, stochastic difference equations). ... I do not subscribe to the articles of faith listed above; at the same time, I do not believe that these articles of faith can be conclusively contradicted with present evidence ... I therefore believe that they should be given a fair trial. It is of the essence of scientific work that most experiments in largely uncharted fields are bound to be unsuccessful. The few successful ones pay for the many unsuccessful ones.” Friedman referred to what he considered to be “the resounding failure to date of all efforts to construct a general model as well as by the multiplicity of qualitative models that have been proposed. The failures to date

¹³ The Cowles Commission’s (successful) application for a Rockefeller Foundation grant was for the study of resource allocation (e.g. Koopmans, ed., 1951), rather than of economic fluctuations (e.g. Koopmans, ed., 1950, Klein 1950).

include the system constructed by J. Tinbergen in his League of Nations study, the systems constructed by various groups in Washington, and the most recent system constructed by Klein for the Cowles Commission.”

Friedman recognized the strength of the Cowles Commission staff as mathematical economists and statisticians (but did not mention Marschak): “Koopmans is intelligent, careful, and scientifically minded. Kenneth Arrow is ingenious and well trained as a mathematical economist and statistician. Don Patinkin is one of the best of the recent crop of students at Chicago and is well trained as an economist, though he has so little experience elsewhere as to be immature and hardly capable of a sound judgment about the value of the Cowles approach. Klein, a former member of the staff, is a highly original economic logician. Haavelmo, another former associate, is also highly original and some of his work is of considerable importance.”

“Nevertheless,” Friedman added, “there are certain special characteristics of the group of people listed that leads me to retain considerable confidence that their experiment will fail. Almost without exception, the people listed are primarily mathematicians or statisticians rather than economists and have had no occasion to do careful scientific quantitative work on a limited segment of the economy. Koopmans, who strikes me as perhaps the ablest and most promising of the Cowles staff ... has fundamentally a theoretical mind and inclination, and came to economics relatively late from mathematics and statistics ... I have no great confidence in his judgment about realistic economic problems or about techniques for attaining sound knowledge of economic processes.” Friedman concluded with two proposals “that might greatly increase the value of the Cowles Commission experiment. First, the Cowles Commission should be urged to provide translations of their basic work from their increasingly specialized jargon into a form in which it will be accessible to, and capable of

judgment by, the great bulk of economists¹⁴ ... Second, the Cowles Commission should be urged to specify in advance [underlining in original] the circumstances and conditions under which they would regard their experiment as a failure.”

Mathematics and Interventionist Policy

“Looking back,” remarks Robert Lucas (in Cord and Hammond, eds., 2016, p. 11), “I think that Friedman (in common with many socialists at that time) viewed mathematical models as a tool for central planners. Now, of course, some view mathematical models as tools of the right, of ‘market fundamentalists.’” One such socialist was Oskar Lange, who played a leading role in bringing the Cowles Commission to Chicago after the death of Henry Schultz (the department’s statistician) and later in recruiting Marschak from the New School in New York as research director, after Marschak and Lange (on leave from Chicago at Columbia University 1940-42) conducted a NBER-sponsored weekend econometrics seminar. In the “socialist calculation” debate of the 1930s, Lange argued, in articles reprinted as Lange (1938), that while Marxian political economy was the key to understanding the laws of motion of capitalist society, it had nothing to offer for the planning of a socialist economy – but that, paradoxically, neoclassical economic theory, and especially Walrasian general equilibrium analysis, was applicable to socialist planning. In 1945, Lange took leave from the University of Chicago to become Communist Poland’s first envoy to the United States and the United Nations, and later returned to Poland to chair a committee of economists advising the planning commission (with Michal Kalecki as vice-chair). Techniques of activity analysis (Koopmans, ed., 1951) originated in studies of the

¹⁴ Friedman cited John Maurice Clark’s “Mathematical Economists and Others: A Plea for Communicability,” which Clark presented to the American Economic Association in January 1947 and published in *Econometrica* that April. Koopmans (1957, p. vii) also invoked Clark’s 1947 plea when stating that “The first essay of this book is an attempt to communicate the logical content, and some of the underlying reasoning, of certain recent developments in mathematical economics ... the first essay attempts to respond to Professor Clark’s appeal.” Friedman did not welcome or take note of Koopmans’s attempt.

“transportation problem” for planning purposes in World War II, by Koopmans for shipping and by George Dantzig and Marshall Wood for the US Air Force. According to the Cowles Commission’s 1950-51 annual report, even Gerard Debreu, a thoroughly-abstract theorist, addressed the University of Chicago’s Political Economy Club on “Socialist Economics.” Apart from possible tools of planning, Klein (1947) and Marschak (1953) saw simultaneous-equation macroeconometric models as guides for Keynesian stabilization policy through demand-management.

Friedman (1946) criticized the text of Oskar Lange’s Cowles monograph *Price Flexibility and Employment* (1944) as “verbal mathematics,” mathematical reasoning disguised by translation into prose, and as a purely theoretical presentation of Keynesian macroeconomics without substantial empirical content. He did not mention that, following the example of Alfred Marshall and J. R. Hicks, Lange’s book had a mathematical appendix, nor did he show any more enthusiasm when Lawrence Klein’s Cowles monograph (Klein 1950) provided an empirical counterpart to theoretical accounts of Keynesian economics.

But as Lucas indicated, the socialist inclinations of some econometricians and mathematical economists, such as Lange, did not justify Friedman’s belief of a leftward, statist bias in the use of mathematics in economics. Lucas and other New Classical heirs to Friedman’s arguments for the ineffectiveness of Keynesian monetary and fiscal policy embraced mathematics and Dynamic Stochastic General Equilibrium (DSGE) modeling, although every part of the phrase Dynamic Stochastic General Equilibrium would have been abhorrent to Friedman and congenial to Koopmans.

Theory and Measurement

In the wake of the “Measurement Without Theory” debate between the Cowles Commission and NBER approaches to studying macroeconomic fluctuations, initiated by Arthur Burns’s “Economic Research and the Keynesian Thinking of Our Time” in the 1946 NBER annual report (reprinted in Burns

1954) and by the scathing critique of Burns and Mitchell (1946) by Koopmans (1947) and including Koopmans's 1949 exchange with Rutledge Vining (see reprints and commentary in Hendry and Morgan, eds., 1995, and Jacob Marschak's critique in NBER 1951 of Mitchell 1951), the Cowles Commission changed its motto in 1952 from "Science as Measurement" to "Theory and Measurement," with implicit emphasis on the word *and*. This was hardly a conciliatory gesture to Friedman, who argued (1950, pp. 465-66) that "Mitchell's work is itself a contribution to economic theory – and a contribution of the first magnitude ... It is always analytical, never aridly descriptive. His theoretical work is throughout interwoven with his empirical work and made a part of an 'analytical description' of the phenomena under study" – "analytical description" being a term that Mitchell had used in 1913, in his first book on business cycles, to describe his approach. Friedman (1950, p. 467) held that Mitchell "was himself almost exclusively concerned with a part of economic theory that was largely outside the main stream of economic thought when he began his scientific work and that even today is least satisfactory – the dynamic adjustment of the economic system as a whole. Because we know so little about this part of economic theory, we tend to neglect it in thinking about economic theory, to use the term to cover what we have, rather than what we ought to have." Friedman (1950) staked a claim for Mitchell and for himself to theorizing about "the dynamic adjustment of the economic system as a whole", challenging claims expressed in the titles of Cowles monographs published that year on *Statistical Inference in Dynamic Economic Models* (Koopmans, ed., 1950) and *Economic Fluctuations in the United States, 1921-1941* (Klein 1950). The most that Friedman (1950, p. 489) would concede was that Mitchell's "subsequent work would have been more fruitful if he had devoted more of his energies to testing and improving the theory in his 1913 volume and less to purely factual analysis."

The emphasis on explicit rational-choice, individual-optimization microeconomic foundations in a Dynamic Stochastic General Equilibrium framework by New Classical economists endorses the Cowles Commission methodology rather than the NBER approach defended by Friedman, notwithstanding New

Classical affinity with Friedman on policy ineffectiveness, endogenous expectations and the natural rate hypothesis. On the hand, the vector autoregression (VAR) approach of Christopher Sims (1980) revives, with more advanced statistical techniques, the NBER Burns-Mitchell statistical approach to analyzing economic time series without much *a priori* macroeconomic theory.

Cowles, Friedman and Prediction

In “The Methodology of Positive Economics” (the lead essay of Friedman 1953), Friedman advanced the methodological position that economic models should be judged by their success at out-of-sample prediction, rather than by the realism of their assumptions. He wrote his methodological manifesto soon after arguing, in Friedman (1951), that the Cowles Commission’s project of Keynesian macroeconomic modelling was a predictive failure, with Klein’s model no better at forecasting than a naïve model. Marcel Boumans (2016, pp. 586, 602) has written that “The interaction between Friedman and the Cowles Commission represented a confrontation of two research programs, each claiming to offer the best scientific approach for the conduct of empirical economics. Although they never reached a consensus, this interaction was to result in the so-called ‘naïve model test’ for econometric models, designed by Friedman and subsequently developed by Carl Christ, a member of the staff at Cowles ... when the history of the Cowles period at Chicago was almost forgotten, the name of the originator of the naïve model test was also almost forgotten.” Unfortunately, this gets the history of the “naïve model test” for the predictive power of econometric models backwards. Rather than Friedman (1951) being the originator and designer of a method that was then “subsequently developed” at the Cowles Commission, the Cowles Commission (and *Econometrica*) existed because Alfred Cowles (1933, 1944), in the wake of the 1929 Wall Street crash, wanted a forum for his use of the naïve model test to show that stock market forecasting services had been no better than chance at predicting stock price movements, a concern with evaluating predictive performance that was continued at the Cowles Commission by

Lawrence Klein (1946), Harry Markowitz (“The Accuracy of Naïve Models,” 1949), Andrew Marshall (1949a) and Carl Christ (1949b, 1951a).

According to Boumans (2016, p. 600), “Christ [1949b] noted that the name ‘naïve model’ had been suggested by [Andrew] Marshall [1949], but that the procedure itself was attributable to Friedman (ibid., p. 24). In actual fact, two naïve models were used by Christ.” But Christ (1949b, p. 24) wrote something quite different. He did not attribute the *procedure* to Friedman but only, after indicating which two naïve models were used by Marshall (1949), added in a footnote, “*These* have been suggested by Milton Friedman *also*” (emphasis added) – the two specific naïve models used, not the procedure of using naïve models as a benchmark for evaluating predictive performance. Boumans (2016, p. 598) states that “Friedman proposed such a test – the naïve model test. It came be his only, albeit lasting, contribution to the Cowles econometrics program, namely the testing of econometric models by comparing their predictive performance with that of a naïve model, the ‘Friedman criticism’ (Koopmans 1951: 4)” – although the “Friedman criticism” as stated by Koopmans (1951, p. 4) made no mention of comparing predictive performance with that of a naïve model¹⁵. Boumans then quotes Koopmans (1957, p. 203) in support of this attribution, but Koopmans (1957, p. 203) only said that the two naïve models used by Christ (1949, 1951) were “suggested by Friedman as standards of comparison.” That is, Friedman suggested the two specific naïve models, not the idea of using naïve models as a benchmark, which had been used by Alfred Cowles (1933) in the inaugural volume of *Econometrica*.

¹⁵ Koopmans’s point 9 reads in full: “Can we meet the Friedman criticism: That Christ’s experiments have shown that the information contained in the data so far processed have been insufficient for good forecasting?” (Koopmans 1951, p. 4). Roy Epstein (1987, p. 107) reports Friedman as repeatedly asking in Cowles seminars from 1946 to 1948 “How does one choose a model, given that numerous possible models exist for the same period? Marschak once replied to him [in October 1947] simply that more data would eventually reveal the true hypothesis” —but Friedman did not mention out-of-sample prediction as the answer to his question.

In the words of Carl Christ (1994, p. 30 n2), “Cowles and his associates generated random stock market advice, and random portfolio choices, and compared them with actual market newsletters and actual fire insurance company portfolios. They found that on average the actual advice and portfolios were slightly inferior to the random ones. The best actual advice and portfolios were about as good as the best random ones, but the worst actual ones were worse than the worst random ones (Cowles 1933).”¹⁶

Boumans (2016, p. 599) also places weight on Christ (1949, p. 1) thanking Friedman for “helpful criticism.” Friedman was one of Christ’s discussants at the November 1949 NBER conference on business cycles (published as NBER 1951, in which Christ 1949b was revised as Christ 1951a). The thanks by Christ (1949b, p. 1) to Friedman and six others for unspecified “helpful criticism” is the acknowledgement due all discussants. Friedman (1951), commenting on the results of the naïve model tests in Christ (1951a), made no claim, or even hint, that Christ owed the procedure to Friedman. It is noteworthy that Christ (1951b) ignored Friedman (1951), instead writing an elaborate reply to the discussant’s comment by Lawrence Klein (1951). Christ (1994) also ignored Friedman (1951), mentioning instead a 1957-58 exchange that Friedman and Gary Becker had with Klein about whether the apparent goodness of fit of the Keynesian consumption was just a statistical illusion due to regressing consumption on a variable, income, of which consumption formed a large part (Christ 1994, p. 35 n7).

While Friedman did not originate the naïve model test (and never claimed that he had), his commentary on Carl Christ’s results is noteworthy. Friedman (1951, p. 107) was moved to “congratulate Carl Christ and the Cowles Commission for undertaking to test the predictive value of Klein’s econometric model and for the thoroughly objective and scientific manner in which they have

¹⁶ When Dickson Leavens, a statistician on the staff of the Cowles Commission and managing editor of *Econometrica*, computed returns on twenty random portfolios for Cowles (1944), he noticed that returns on more diversified portfolios had smaller standard deviations (Leavens 1945). Markowitz (1952) used the activity analysis techniques of Koopmans (ed., 1951) to optimally diversify portfolios, apparently without knowledge of Leavens (1945).

performed this task.” He welcomed Christ’s findings: “Christ’s revision of Klein’s model does no better than the naïve model for the one year for which Christ could make the test, 1948. The econometric model makes larger errors than the naïve model for approximately half the variables predicted, and its average error is, if anything, larger than the average error of the naïve model ... it is an act of pure faith to assert that the econometric model can predict the effect of policy changes, and there is no reason for anyone else to share this faith until some evidence for it is presented” (1951, p. 110-111). Thus, Friedman (1953) adopted out-of-sample predictive performance as the sole criterion for judging economic models, to the exclusion of testing a model’s assumptions, at a time when he believed that criterion showed the failure of the Cowles Commission project of Keynesian simultaneous-equation macroeconomic model-building to evaluate and guide interventionist policy: “the results suggest that Klein’s experiment was unsuccessful” (1951, p. 107)¹⁷, just as Friedman had predicted to Willits.

Ironically, as Neil Ericsson, David Hendry and Stedman Hood point out (in Cord and Hammond, eds., pp. 96, 135), “Despite Friedman’s (1953) critique of testing a model’s assumptions, Friedman and Schwartz (1982) do explicitly test assumptions about their empirical models ... In fact, Friedman and Schwartz’s final money-demand models perform worse than simple random-walk models for velocity.” Carl Christ (1949b, 1951a) found that the first generation of Keynesian macroeconometric models predicted poorly, but this did not guarantee that the criterion of predictive performance would always produce results that Friedman would welcome.

Conclusion

¹⁷ Robert Solow (in Arrow et al. 1991, pp. 92-93) summarized the discussion of Christ (1951a): “Friedman concluded that the whole econometric model-building enterprise had been shown to be worthless and congratulated the Cowles Commission on its self-immolation. Klein demonstrated the truth of the French doggerel: *Cet animal est très méchant. Quand on l’attaque, il se défend.* Carl Christ behaved like a perfect gentleman.”

The departure of the Cowles Commission from the University of Chicago in 1955, after a series of Cowles monographs and papers that would later be honored with Nobel Memorial Prizes, followed an intense clash of theoretical and empirical methodologies, of political and public leanings, and of personalities between the Cowles Commission and the emerging Chicago school of economics, two groups that each exert great influence on the development of economics. By 1957, two years after the split, these groups formed two of the three highest-rated economics departments in the United States. The two groups and their approaches evolved subsequently: Chicago economics gradually ceased to spurn mathematics, econometrics and general equilibrium as tools for central planners and Keynesian interventionists, the Cowles Foundation ceased to be as distinctive within the discipline of economics as, in the words of Robert Lucas, what had been “mathematical economics” became simply “economic theory.” The frictions and debates within the economics department at the University of Chicago in the late 1940s and early 1950s both shed light on the approaches of the two groups of economists and reveal key developments in the institutional and intellectual development of modern economics.

References

- Ahmad, Liaquat. 2019. “Widening Gyre: The rise and fall of economic inequality,” *New Yorker*, September 2, 26-29.
- Arrow, Kenneth J. 1951. *Social Choice and Individual Values*, Cowles Commission Monograph No. 12. New York: John Wiley & Sons, 2nd ed. 1963.
- Arrow, Kenneth J., and Gerard Debreu. 1954. “Existence of an Equilibrium for a Competitive Economy,” *Econometrica* 22: 265-90; Cowles Commission Paper, New Series, No. 87.
- Arrow, Kenneth J., Gerard Debreu, Edmond Malinvaud and Robert M. Solow. 1991. *Cowles Fiftieth Anniversary: Four Essays and an Index of Publications*. New Haven, CT: Cowles Foundation for Research in Economics.
- Boumans, Marcel. 2016. “Friedman and the Cowles Commission,” in Cord and Hammond, eds. (2016), pp. 585-604.

- Burns, Arthur F., and Wesley C. Mitchell. 1946. *Measuring Business Cycles*. New York: National Bureau of Economic Research.
- Burns, Arthur F., ed. 1952. *Wesley Clair Mitchell: The Economic Scientist*. New York: National Bureau of Economic Research.
- Burns, Arthur F. 1954. *The Frontiers of Economic Knowledge: Essays by Arthur F. Burns*. Princeton, NJ: Princeton University Press for the National Bureau of Economic Research.
- Christ, Carl. 1949a. "Further Comments on Lawrence Klein's *Economic Fluctuations in the United States, 1921-1941* (second draft)," Cowles Commission Discussion Paper No. 241.
- Christ, Carl. 1949b. "A Revised Klein Econometric Model for the United States, 1921-1947," Cowles Commission Discussion Paper No. 269.
- Christ, Carl. 1951a. "A Test of an Econometric Model for the United States, 1921-1947" in NBER (1951), pp. 35-107; Cowles Commission Paper No. 49.
- Christ, Carl. 1951b. "Reply by Mr. Christ," in NBER (1951), pp. 123-126.
- Christ, Carl 1994. "The Cowles Commission's Contributions to Econometrics at Chicago, 1939-1955," *Journal of Economic Literature* 32(1): 30-59.
- Colander, David, and Craig Freedman. 2019. *Where Economics Went Wrong: Chicago's Abandonment of Classical Liberalism*. Princeton, NJ: Princeton University Press.
- Cord, Robert, and J. Daniel Hammond, eds. 2016. *Milton Friedman: Contributions to Economics and Public Policy*. Oxford: Oxford University Press.
- Cowles, Alfred 3rd. 1933. "Can Stock Market Forecasters Forecast?" *Econometrica* 1: 309-24; reprinted in Cowles Commission Paper, New Series, No. 6 (1944).
- Cowles, Alfred 3rd. 1944. "Stock Market Forecasting," *Econometrica* 12:206-214; in Cowles Commission Paper, New Series, No. 6.
- Dimand, Robert W. 2014. *James Tobin*. Basingstoke, UK: Palgrave Macmillan, Great Thinkers in Economics.
- Emmett, Ross B., ed. 2010. *The Elgar Companion to the Chicago School of Economics*. Cheltenham, UK: Edward Elgar.
- Epstein, Roy J. 1987. *A History of Econometrics*. Amsterdam: North-Holland.
- Frazer, William J., Jr. and Lawrence A. Boland. 1983. "An Essay on the Foundations of Friedman's Methodology," *American Economic Review* 73(1): 129-144.
- Friedman, Milton. 1940. Review of *Business Cycles in the United States of America, 1919-1932* by Jan Tinbergen, *American Economic Review* 30(3): 657-60.

- Friedman, Milton. 1946. "Lange on Price Flexibility and Employment: A Methodological Critique," *American Economic Review* 36(4): 613-31; reprinted in Friedman (1953).
- Friedman, Milton. 1948. "Memorandum to J. H. Willits from Milton Friedman re: Cowles Commission Research," stamped May 26, Rockefeller Foundation Records, Rockefeller Archive Center, RG 1.2 Projects, series 216s: Box 4, Folder 26:1.
- Friedman, Milton. 1950. "Wesley C. Mitchell as an Economic Theorist," *Journal of Political Economy*, 58: 463-95; reprinted in Arthur F. Burns, ed., *Wesley Clair Mitchell: The Economic Scientist*, New York: National Bureau of Economic Research, 1954, pp. 237-82.
- Friedman, Milton. 1951. "Comment" (on Carl Christ 1951a) in NBER (1951), pp. 107-114.
- Friedman, Milton. 1953. *Essays in Positive Economics*. Chicago: University of Chicago Press,
- Friedman, Milton. 1962. *Price Theory: A Provisional Text*. Chicago: Aldine.
- Friedman, Milton, and Rose D. Friedman. 1998. *Two Lucky People: Memoirs*. Chicago: University of Chicago Press.
- Friedman, Milton, and Anna J. Schwartz. 1982. *Monetary Trends in the United States and United Kingdom: Their Relation to Income, Prices, and Interest Rates, 1867-1975*. Chicago: University of Chicago Press for the National Bureau of Economic Research.
- Haavelmo, Trygve. 1944. "The Probability Approach to Econometrics," *Econometrica* 12 (Supplement): 1-118; Cowles Commission Paper, New Series, No. 4.
- Hammond, J. Daniel, and Claire H. Hammond. 2006. *Making Chicago Price Theory: Friedman-Stigler Correspondence 1945-1957*. London and New York: Routledge.
- Hendry, David, and Mary S. Morgan, eds. 1995. *Foundations of Econometric Analysis*. Cambridge, UK: Cambridge University Press.
- Hildreth, Clifford. 1986. *The Cowles Commission in Chicago, 1939-1955*. Berlin and Heidelberg: Springer-Verlag.
- Hood, William C., and Tjalling C. Koopmans, eds. 1953. *Studies in Econometric Method*, Cowles Commission Monograph No. 14. New York: John Wiley & Sons.
- Klein, Lawrence R. 1946. "A Post-Mortem on Transition Predictions of National Product," *Journal of Political Economy* 54: 289-308; Cowles Commission Paper, New Series, No. 18.
- Klein, Lawrence R. 1947. "The Use of Econometric Models as a Guide to Economic Policy," *Econometrica* 15: 111-51; Cowles Commission Paper, New Series, No. 23.
- Klein, Lawrence R. 1950. *Economic Fluctuations in the United States, 1921-1941*, Cowles Commission Monograph No. 11. New York: John Wiley & Sons.
- Klein, Lawrence R. 1951. "Comment" (on Carl Christ 1951a) in NBER (1951), pp. 114-126.

- Koopmans, Tjalling C. 1947. "Measurement Without Theory," *Review of Economics and Statistics* 29: 161-72; also, in Koopmans and Marschak, "Two Book Reviews," Cowles Commission Paper, New Series, No. 25.
- Koopmans, Tjalling C., ed., 1950. *Statistical Inference in Dynamic Economic Models*, Cowles Commission Monograph No. 10. New York: John Wiley & Sons.
- Koopmans, Tjalling, C., ed. 1951. *Activity Analysis of Production and Allocation*, Cowles Commission Monograph No. 13. New York: John Wiley & Sons.
- Koopmans, Tjalling C. 1951. "Comments on Macro-Econometric Model Construction," in Lawrence R. Klein and Tjalling C. Koopmans, Cowles Commission Discussion Paper No. 2008, pp. 2-4.
- Koopmans, Tjalling C. 1956. "The Klein-Goldberger Forecasts for 1951, 1952 and 1954, Compared with Naïve-Model Forecasts," Cowles Foundation Discussion Paper No. 12.
- Koopmans, Tjalling C. 1957. *Three Essays on the State of Economic Science*. New York: McGraw-Hill.
- Krepienevich, Andrew F., and Barry D. Watts. 2015. *The Last Warrior: Andrew Marshall and the Shaping of American Defense Policy*. New York: Basic Books.
- Lange, Oskar. 1938. "On the Economic Theory of Socialism" in Oskar Lange and F. M. Taylor, *The Economic Theory of Socialism*, ed. Benjamin Lippincott, Minneapolis, MN: University of Minnesota Press, pp. 57-143.
- Lange, Oskar. 1944. *Price Flexibility and Employment*, Cowles Commission Monograph No. 8. Bloomington, IN: Principia Press; mathematical appendix, "The Stability of Economic Equilibrium," issued as Cowles Commission Paper, New Series, No. 8.
- Leavens, Dickson H. 1945. "Diversification of Investment," *Trusts and Estates* 80.
- Markowitz, Harry. 1949. "The Accuracy of Naïve Models," Cowles Commission Discussion Paper No. 339.
- Markowitz, Harry. 1952. "Portfolio Selection," *Journal of Finance* 7: 77-91; Cowles Commission Paper, New Series, No. 60.
- Marschak, Jacob. 1953. "Economic Measurements for Policy and Prediction," in Hood and Koopmans, eds. (1953), pp. 1-26.
- Marshall, Andrew W. 1949a. "A Test of Klein's Model III for Changes of Structure," unpublished master's thesis, University of Chicago; abstract (1950) in *Econometrica* 18(3): 291.
- Marshall, Andrew W. 1949b. "A Note on the Use of Tolerance Intervals as Test Regions," Cowles Commission Discussion Paper No. 335.
- McKenzie, Lionel. 1954. "On Equilibrium in Graham's Model of World Trade and Other Competitive Systems," *Econometrica* 22: 147-61.

Miller, H. Laurence, Jr. 1961. "On the 'Chicago School of Economics,'" *Journal of Political Economy* 70(1): 64-69.

Mirowski, Philip, and Dieter Plehwe, eds. 2009. *The Road from Mount Pelerin: The Making of the Neoliberal Thought Collective*. Cambridge, MA: Harvard University Press.

Mitch, David. 2016. "A Year of Transition: Faculty Recruiting at Chicago in 1946," *Journal of Political Economy* 124(6): 1714-1734.

Mitchell, Wesley C. 1951. *What Happens During Business Cycles: A Progress Report*. New York: National Bureau of Economic Research.

National Bureau of Economic Research (NBER). 1951. *Conference on Business Cycles*. Princeton, NJ: Princeton University Press for the National Bureau of Economic Research (Universities-National Bureau Committee for Economic Research, Special Conference Series).

Patinkin, Don. 1969. "The Chicago Tradition, the Quantity Theory, and Friedman," *Journal of Money, Credit and Banking* 1, reprinted in Patinkin (1981); as reprinted in Robert Leeson, ed., *Keynes, Chicago and Friedman*, London: Pickering and Chatto, 2003, pp. 87-120.

Patinkin, Don. 1981. *Essays on and in the Chicago Tradition*. Durham, NC: Duke University Press.

Reder, Melvin. 1982. "Chicago Economics: Permanence and Change," *Journal of Economic Literature* 20(1): 1-38.

Rivot, Sylvie. 2013. *Keynes and Friedman on Laissez-Faire and Planning*. London and New York: Routledge.

Samuelson, Paul. 2011. *Collected Scientific Papers, Vols. 6 and 7*, ed. Janice Murray. Cambridge, MA: MIT Press.

Sims, Christopher. 1980. "Macroeconomics and Reality," *Econometrica* 48(1): 1-48.