

Beyond Numbers: Keynes, Friedman, and the Mathematization of Economics

HISTORY 269

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Few individuals have had as significant an impact as Milton Friedman and John Maynard Keynes in twentieth-century economic thought. Beneath the surface of their well-documented divergences, often occupying opposite ends of the spectrum regarding policy recommendations, lies a less explored yet significant common ground: their shared critique of the growing reliance on mathematical modeling in economics. While both Friedman and Keynes were not entirely opposed to using mathematics in the field, they contended that the singular focus on abstract modeling neglected the limitations of statistics in some situations, undermined the nature of economics as a discipline where creativity, psychology, and logic play as big of a role as numbers, and, to Friedman, also posed a threat to his notions of economic freedom.

The origin story of the quantification of economics lies in the econometric movement of the early 1930s. Pioneers such as Jan Tinbergen and Alfred Cowles began applying statistical methods to economic data to try and understand the inter-war business cycles. Tinbergen's major 1939 work, *Business Cycles in the United States, 1919-32*, was the target of criticism from both Keynes (1939) and Friedman (1940). Cowles funded a commission of mathematical economists, which during the 1940s was headquartered in the University of Chicago and housed several economists with whom Friedman quarreled. During the Cowles period in Chicago, Friedman wrote his *Methodology of Positive Economics* (1953), which sums up much of his philosophical thinking on the nature of economics as a science.

Because so much of Keynes' and Friedman's views on mathematics are spread across their years of writing, this paper focuses on bringing their points to life by comparing their

specific contributions in reviewing Tinbergen's work and, later, after Keynes' death, how Friedman fought against the quantitative Cowles movement in Chicago. Other major works from both economists and secondary sources are incorporated into the analysis to understand how they reached their stances on the mathematization of economics.

Keynes' critique of the movement towards modeling is a technical analysis of the application of statistical methods and a broader assessment that economics as a practice cannot be reduced to arithmetic. To understand the essence of his points, it is essential to go back a decade to the 1920s, where his early writings underpin these two sides of his critique. On the technical front, it is worth noting that Keynes graduated with a bachelor's degree in mathematics, and one of his first significant works was *A Treatise on Probability* (1921). The book is neither a purely mathematical nor economic treatise; instead, Keynes focused on the logic and philosophy behind probability theory. He went against classical frequentist probability and proposed that probability is a measure of rational belief based on partial knowledge. Key to his future understanding of Tinbergen's flawed econometric analysis is how he defined the credibility of an inductive argument (i.e., an argument that derives general principles from specific observations). To Keynes, inductive reasoning was valid only because the "repetition and uniformity" of experiences justify our belief that any inductive assumptions laid out at first remain valid. This, in turn, allows us to extend the validity of the inductive hypothesis itself ad infinitum (Keynes 1921, 260). In the *Treatise*, Keynes also doubted correlations derived from insufficient or poorly discussed logical foundations (Keynes 1921, 424).

To Keynes, theory and logic precede algebra. It is only after deriving a provisional conclusion that it is appropriate to use statistics. In his magnum opus, the *General Theory of Employment, Interest and Money* (1936), Keynes pointed out that the "nature of economic thinking" was not to try to build a "machine (...) which will furnish an infallible answer" but to provide an "organized and orderly method of thinking" (Keynes 1936, 297). He acknowledged the importance of models, but only if they were used after the economist had a working provisional

conclusion. In other words, math and statistics were ways to validate a theory, not a way to come up with one. The danger in following the latter route was “blindly” writing algebra, disregarding possible interdependent variables without knowing what these variables represented in the real world (Keynes 1936, 298). Without this regard, the statistical method became self-defeating since strict independence between the variables was assumed to be a prerequisite for the model’s validity.

Keynes was also not satisfied with how economics was being mathematized because it contradicted his vision of economics as a science that went beyond mere descriptive theory. He believed that economics should also have a practical perspective on the future. Keynes portrayed economics as spanning theory and practice, scrutinizing the past and present to inform the future. He described the ideal economist as a “mathematician, historian, statesman [and] philosopher,” navigating both the “abstract and concrete.” This was clear from his 1924 obituary to his professor, Alfred Marshall (Keynes 1924, 322).

Keynes further demonstrated, implicitly in his work, the importance of thinking creatively and borrowing concepts from other fields, such as psychology, into economic theories. *The General Theory* is a product of this kind of creative vision. He looked at the familiar material of economic theory and the reality of the Great Depression and recognized that in the real world, factors like confidence, fear, and “animal spirits” could cause demand not to respond to supply in the ways traditional models predicted. To Keynes, human enterprise exists because of such animal spirits and not because of some “quantitative probability” or “mathematical expectation” (Keynes 1936, 161-2). Economics, to Keynes, was a study of humans in the real world, and the openness to understand more than just numbers is behind his breakthroughs.

Keynes’ technical and philosophical opinions on using statistics in economics from the 1920s to 1936 culminated in his 1939 negative review of Tinbergen’s study of business cycles. His main concern is how Tinbergen determined the relative importance of factors causing business cycles. Keynes posited that a model could only discover the true relative causal importance of

factors if it incorporated all possible factors into its analysis. Tinbergen jumped straight to modeling without adequately discussing his choice of variables. Even if Tinbergen had considered all possible variables, some real-world variables, such as psychological factors (in an allusion to his animal spirits), are not measurable. So, no model could be valid (Keynes 1939, 560-1). Moreover, Keynes labeled Tinbergen's assumption of linearity between variables as improbable and even "ridiculous." Variables in the real world do not move proportionally to one another. Linearity was introduced simply for the models' tractability, even though these models did not accurately predict the real world (Keynes 1939, 564).

Keynes questioned the validity of Tinbergen's inductive claims, concluding that his work was a mere "statistical description" (Keynes 1939, 566). In the *Treatise on Probability* (1921), Keynes explained that uniformity was a logical prerequisite to validate an inductive argument. Keynes suggested that Tinbergen disregarded the historical conditions of his data and would have done a better analysis had he broken down his business cycle data into historical periods that could be logically grouped to ensure the samples had homogeneity (Keynes 1939, 567). Because the sample data was not uniform, Keynes could not see how induction could be used to generalize Tinbergen's findings. Tinbergen's analysis was, therefore, unsuccessful since Keynes believed economic theory should be inducted towards understanding the past and the future and not stand as a static description of the present.

In later years, Paul Samuelson, a neo-Keynesian and well-known mathematical economist, attempted to discredit Keynes' Tinbergen review. He claimed that Keynes lacked the technical skills to comprehend what he was criticizing (Leeson 2000, 7). Regardless of whether this assertion was true, Samuelson's contemporary, Milton Friedman, certainly had the necessary technical expertise to understand what he was criticizing. During the Second World War, Friedman worked at Columbia's Statistical Research Group (SRG), where he developed new statistical techniques, such as sequential analysis, which became widely adopted by statisticians after the war (Burns 2023, 126).

Friedman rejected the mathematization of the field not out of ignorance but after having produced knowledge in statistics and choosing to reject it in economics. Early in his career, while still at the SRG, Friedman also wrote a review of Tinbergen's work (Burns 2023, 136). The review argued that Tinbergen's correlation coefficients were merely "tautological reformulations of selected data" (Friedman 1940, 659). In other words, the model was designed to fit the data so precisely that it failed to offer any new knowledge or predictive ability regarding the underlying nature of business cycles, echoing Keynes' point on the lack of valid inductive conclusions from Tinbergen's study.

Despite Tinbergen's critics, the field of econometrics continued to expand. During Friedman's tenure at the University of Chicago and after Keynes passed away in 1946, the Cowles Commission at Chicago housed some of the best econometricians and post-Keynesians of its time – the extent to which Keynes would have approved remains a matter of speculation. During this time of quarreling with his neighbors at the Cowles Commission, Friedman wrote *A Methodology for Positive Economics* (1953), laying out his vision of conducting better economics. Although Friedman never mentioned the Commission, the paper makes implicit counterpoints to Cowles (Burns 2023, 157).

While the main point of Friedman's *Methodology* – that economic theories should serve as tools for predicting and understanding economic phenomena, irrespective of whether the assumptions perfectly mirror reality – puts Friedman at odds with Keynes' *General Theory* approach of overemphasizing the realism of assumptions (Keynes 1936, 276), a closer reading reveals a striking similarity in how both economists thought about the nature of their discipline and cautioned against hasty application of statistics to economic data. Both Friedman and Keynes agreed that economics could only progress as a science through "improvement in the choice of models," as Keynes would put it in a letter to fellow economist Roy Harrods (Keynes 1938, 297). In his *Methodology*, Friedman provided guidelines for discovering meaningful economic hypotheses, indicating that economists must determine which entities of a model

corresponded to those of the real world and which were the most influential to the model's predictive power. Ultimately, economists should have ways of judging a model's suitability to its context and choose appropriately (Friedman 1953, 178-9).

Central to Friedman's *Methodology* was also the distinction between "tautologies," mere truthful descriptions such as the fact that prices go up when demand exceeds supply, and "substantive hypotheses," mechanisms that helped understand why and by how much the price would change (Friedman 1953, 158-9). The difficulty and slowness of measuring substantive hypotheses in the real world can quickly turn economics into a "structure of tautologies," but economists must resist that. Friedman believed that by focusing on substantive hypotheses, economists could predict and not merely describe the consequences of an action, echoing Keynes' earlier criticism that Tinbergen was merely describing his data but not inducting valuable predictions about the business cycle.

Friedman and Keynes did not dismiss the use of mathematics in economics but believed it should not take center stage. Math and logic as tautologies were essential, but only in checking the correctness of reasoning. In another correspondence with Roy Harrods, Keynes made the same point a decade before Friedman's paper, claiming that statistics does not help to generalize predictions but only "test the relevance and validity of the model" (Keynes 1938, 296). To Friedman, using models was acceptable, contingent on having the awareness that "abstractness and completeness" are characteristics of the "conceptual world," hence valuable models were those that worked in practice, predicting the "concrete, incomplete" real world (Friedman 1953, 167), again a point confirmed by Keynes' description of the master economist bridging the gap between the concrete and the abstract (Keynes 1924, 322).

In addition to a practical stance, both economists further agreed on an interdisciplinary approach to economics. Both write that economic thinking should not start with algebra but with a preliminary theory. Friedman argued that if economics consisted only of tautologies, it was nothing more than "disguised mathematics" (Friedman 1953, 159). He alerted against Cowles'

“abstractness, generality, and mathematical elegance for its own sake” (Friedman 1949, 490); a point comparable to Keynes’ claim is that Tinbergen seemed to take more pleasure in his “mazes of arithmetic” rather than from engaging in the necessary “mazes of logic” (Keynes 1938, 559). Ultimately, it was not statistical analysis but creativity and intuition, observing human psychology, and studying biographies and not treatises that Friedman envisioned knowledge being created in economics (Friedman 1953, 179).

While Friedman's and Keynes’ critiques were both driven technically by arguments on the applicability of statistics to economic data and philosophically by their understanding of economics as a practical and interdisciplinary science, Friedman’s critique was also politically driven, a third factor he did not have in common with Keynes. Part of this comes from the fact that the Cowles economists, whom he wanted to discredit, were known to be more politically liberal (Burns 2023, 289). Friedman expressed in his *Methodology* the belief that an observational science such as economics could not be entirely objective or neutral. Economists are part of the subject matter being investigated “in a more intimate sense than in the physical sciences” (Friedman 1953, 154). Friedman did not see a problem with this, so long as economists used the methodologies proposed to achieve better positive conclusions that could inform normative debates.

The problem was that Friedman did not believe the Cowles economists followed his *Methodology of Positive Economics*, preferring to stay in the realm of abstract models. Consider, for instance, the New Deal. Friedman, an advocate of practical economics like Keynes, approved certain parts of the New Deal, agreeing that in times of crisis, the government needed to stimulate demand (Burns 2023, 292). Keynes justified such intervention using psychology, claiming that supply does not create its demand when uncertainty or animal spirits drive spending, investing, and saving behavior, stopping the economy from reaching its equilibrium state. Keynes’ mathematical followers, however, simplified his notions of savings and investment into sets of linear equations and IS-LM models, linking various economic factors, and the new

norm became to prescribe stimulus even through ordinary times (Burns 2023, 187, 292-3). In mathematizing Keynes, his followers forgot that central to Keynes was the assumption that sometimes, human psychological factors can impede the economy from reaching the equilibrium solutions to their equations, a concrete example of mathematics overlooking several essential steps in mapping an economic model to the real world.

By the logic set out in Friedman's paper, not adhering to the *Methodology* meant an economist would fail to arrive at accurate positive conclusions, thereby misleading normative policy debates (Friedman 1953, 155). Friedman viewed this potential for misinformation as particularly dangerous in the political realm. He was concerned about the assumptions underlying specific economic models. In her account of the Cowles models, Burns highlights that they were built with the assumption of government management of the economy (Friedman 1953, 288-9), a concept that Friedman found morally problematic (Friedman 2020, 29-31). Friedman further believed that justifying government intervention to achieve outcomes such as "full employment" or "economic growth" was "misleading" (Friedman 2020, 47). From that, it is not hard to extrapolate a world where central economic planning was based on models that prioritized the factors that Friedman found misleading, which went against his political and moral ideals of freedom.

Friedman, however, would likely not admit that his critique of the mathematical economists was driven by politics, justifying instead that his skepticism came from one of the technical or philosophical considerations laid out in his *Methodology*. A clear example is in *Capitalism and Freedom*, Friedman's book written for a general audience, where he lays out his political and moral principles and policy proposals. Still, he refrains from adding political judgment when rejecting the "widely held belief" that increasing government expenditures is always expansionary; he instead claims that this was never shown to be logically or empirically true, demonstrating how the model from which the conclusion follows fails to account for monetary variables – so it was incomplete. To Friedman, this was not good positive economics

because it was a conclusion derived from a model-first and not logic-first approach. When logically reasoning through the monetary effects of this policy, Friedman posits that the policy outcomes become more dubious (Friedman 2020, 95-6).

Keynes and Friedman, often perceived as intellectual opposites, shared a critical stance on the rising trend of mathematical economics during their times. Both viewed economics as a discipline grounded in practical applications, demanding a balance between models and a broader interdisciplinary approach. Despite their contrasting policy recommendations, rooted in differing theoretical conclusions and political ideals, their involvement in the public debate is a testament to their shared vision of economics as a discipline committed to practice, addressing real-world problems beyond just the numbers.

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