PART ONE

ECONOMICS BUILT FOR POLICY: THE LEGACY OF MILTON FRIEDMAN

ONE

Positive Economics for Democratic Policy

Milton Friedman, Institutionalism, and the Science of History

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As economics pushes on beyond "statics," it becomes less like science, and more like history.

– Sir John R. Hicks 1979, xi

Conventional wisdom would place early institutional economics and the postwar Chicago School on opposite ends of any methodological spectrum. Prominent faculty and graduates from postwar Chicago have derided institutional economics not merely as incorrect but as actually devoid of content. Thomas Sowell characterized it as "half economics, half sociology, and all mush" (Sowell 1993, 788); Ronald Coase claimed it "had nothing to pass on except a mass of descriptive material waiting for a theory, or a fire" (Coase 1984, 230); and George Stigler found it vacuous beyond "a stance of hostility to the standard theoretical tradition" (Kitch 1983, 170).

Nonetheless, recent scholarship has softened this superficially sharp contrast. In the first place, institutionalism encompassed a diverse range of approaches (Rutherford 2000), and postwar Chicago economists have been noticeably less critical of at least one prominent strand: the quantitative analysis associated with Wesley C. Mitchell, the National Bureau of Economic Research (NBER, cofounded by Mitchell), and some aspects of the Robert Brookings Graduate School (forerunner of the Brookings Institution). Stigler expressed grudging respect for Mitchell, and Milton Friedman likewise concluded that Mitchell was "not as empty of content as most of the [institutionalists]" (Kitch 1983, 170, 171). In fact, Friedman spoke quite positively about Mitchell in a lengthy posthumous assessment

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in 1950 in which he argued for viewing Mitchell as an "economic theorist" (Friedman 1950), a significant label because the most common objection to institutional economics from postwar Chicago was its alleged neglect of theory (e.g., Kitch 1983, 169–171).

This more benign view of Mitchell surely results in part from the personal connections between him and key members of the postwar Chicago School, especially Friedman. Friedman studied under both Mitchell and one of Mitchell's most prominent advisees, Arthur F. Burns; Chicago economists (including Mitchell and Stigler) worked closely with the NBER when it was directed by institutional economists (Mitchell and Burns); and interwar Chicago itself was home to a diverse range of methodological approaches, including those promoted by institutional economists (Hammond 2001; Rutherford 2010). Yet Friedman's assessment of Mitchell reflects more than respect for a former colleague and mentor: As Abraham Hirsch and Neil de Marchi have argued in detail, Friedman's own methodological outlook was quite similar to Mitchell's except for their disagreement over the empirical validity of neoclassical price theory (Hirsch and de Marchi 1990, 42–52; cf. Hammond 1996).

In this essay, I aim to reinforce the perceived ties between Friedman and the strand of institutional economics associated with Mitchell. On the most concrete level, I introduce a new and important locus for that connection: Friedman's 1935–1937 work for the U.S. National Resources Committee. More broadly, I highlight a hitherto overlooked dimension to their shared methodological perspective, namely the common goal of creating a new form of economics that could have an extensive role in democratic policy making. Briefly put, both Mitchell and Friedman believed that economics could be established as a predictive science with a broad scope capable of yielding objective claims (that is, claims that demanded universal assent from all rational observers). The source of this power would be an expanded empiricism: Theories and analytical tools would be developed from, and tested against, "real-world" data, especially statistics.

The predictive and empirical character of this scientific economics was the foundation for its political value. Because it was predictive – given appropriate variables and constraints, economists could predict outcomes for real-world policy choices – the development of economics was de facto the development of human capability to control economic behavior. Likewise, because (according to proponents) it was objective and politically neutral – resting strictly on the fit between models and facts – it could be readily assimilated into political life without subverting democratic discussion. Economists, in other words, could provide policy knowledge independent of political judgments. Crucially, both Mitchell and Friedman envisioned economists as heavily involved in policy making, where they would not serve as partisan advisers but as neutral scientists who clarified available means and predicted the outcomes of various actions. Thus the expansion of scientific economics was simultaneously the rationalization of politics.

Given this emphasis on prediction and empiricism, it should be unsurprising that both Friedman and Mitchell saw strong parallels between the physical sciences and economics (Hirsch and de Marchi 1990, 47). Just as the physical sciences provided objective, predictive theories for the behavior of nonhuman objects, so too would economics provide objective, predictive theories in the social realm. One serious glitch in this analogy, of course, was economists' general inability to isolate a phenomenon for repeated experimentation. In practice, economists collected empirical, historical data (whether from last month or last century) and then developed, refined, and tested their theories with that data. "Prediction" thus frequently reduced to "fit with historical data." Not only did this practice raise epistemological puzzles (in what sense could tests against existing data count as prediction?), it also committed adherents to a particular vision of history. If the value of economics lay in its ability to make predictions about future outcomes, and if these predictions were (in practice) extrapolations from historical analysis, then belief in the scientific character of economics implied a similar belief in the potential for scientific history. In other words, to believe in economics as defined by Mitchell and Friedman was to believe that historical analysis could yield objective predictions about future developments. In fact, economics *qua* science was the embodiment of this very kind of historical study. Thus the prediction of social behavior, the establishment of a scientific form of historical analysis, and the rationalization of politics all formed a logically connected framework that united Mitchell's strand of institutional economics with Friedman's version of Chicago economics.

Recognizing these connections helps us distinguish Friedman from many of his mentors in the interwar economics department at Chicago, notably Frank Knight. Melvin Reder (a self-described "participant-observer" of Chicago economics) has suggested that the germ of the postwar school "sprang from the affinity group of Knight's students and protégés that formed in the middle 1930s," including "Milton and Rose Director Friedman, George Stigler, Allen Wallis, Aaron Director and Henry Simons" (Reder 1982, 1, 6–7). Not only was Knight a vocal critic of institutional economics (including Mitchell's version) and a committed proponent of neoclassical price theory, but his political leanings corresponded to those of key Chicago School figures: Thus in 1947 he would join Friedman, Director, and Stigler as founding members of the Mont Pelerin Society. Yet this superficial alignment can be highly misleading, because Knight's critique of institutional economics targeted many of the same features that it shared with the postwar Chicago School: the conviction that economics could make reliable predictions about real-world problems, the belief in the scientific character of historical analysis, and the effort to rationalize politics (which Knight found both futile and dangerous). In short, whereas Knight attacked institutional economics from a largely orthogonal direction – denying its key presumptions – Friedman's critique came from within the general framework that Mitchell had already established and institutionalized in several locations, including the NBER and (as we will see) economic planning agencies within the New Deal. Thus, surprising though it may seem at first, Friedman's outlook was fundamentally much closer to Mitchell's than to Knight's.

Having grasped this connection, we are better positioned to understand the ambiguous relationship between the Chicago School and institutional economics, to understand why key members could be so clearly indebted to Mitchell's methodological outlook and yet so dismissive of the movement with which he was associated. For behind this attitude lies a natural consequence of any effort to rationalize politics through objective scientific study: If policy analysis is a matter of objective science, then opponents who use different methods to reach diametrically opposed conclusions must not only be incorrect but actually unscientific, doomed by flawed methods. In the conclusion, I suggest that this dynamic explains much of the rhetoric surrounding the Chicago School (from both critics and supporters) and also illuminates contemporary discussions of economic policy. Just as the Chicago School was the heir to Mitchell's methodological vision on the political right, so too does the political left have its parallel version, and the clash between these conceptions can involve nothing but appeals to scientific purity and charges of ideological blindness.

1.1. Wesley C. Mitchell and the Reconstruction of American Economics

One of Mitchell's more famous methodological statements was his 1924 presidential address to the American Economics Association (Mitchell 1925) in which he dismissed neoclassical theory as an unrealistic creation of armchair philosophers and outlined a new vision for economics founded on extensive empirical study. However, another essay from the same year, "The Prospect of Economics" (Mitchell 1924), is equally revealing. Here, Mitchell more explicitly discussed the motivation underlying his proposed reforms: the desire to make economic theory useful for policy.

Mitchell divided his assessment of economics into two sections: the "Past" and the "Future." Tellingly, the "Past" started not with Adam Smith, but with David Ricardo, who had constructed a systematic analysis of political economy in response to the financial crises and social unrest arising in England in the wake of the Napoleonic Wars. Thus economics, in Mitchell's depiction, began as pragmatic policy science. Yet his tale of origins also included a sinful fall that left classical political economy alienated from the political concerns that had animated it.

By the turn of the twentieth century, Mitchell lamented, "economics had settled into an academic discipline, cultivated by professors and neglected by men of action, modest in its pretensions to practical usefulness, more conspicuous for consistency and erudition than for insight, hated by few and feared by none" (Mitchell 1924, 19). This irrelevance was a direct corollary of the isolation of economic theory from applied problems:

> For many years there has been a notable difference between the way in which economists handled economic theory on the one hand and the way in which they handled such problems as transportation, public finance, tariffs, money, banking, insurance, trusts, and labor on the other hand. The monographs made little use of the theoretical treatises, and the treatises drew upon the monographs for little beyond illustration. Text books often had a theoretical part and an applied part held together by nothing more intimate than the binding. (24)

Returning economics to political relevance would require bridging this division.

According to Mitchell, blame for the bifurcation fell on Ricardo's successors, who had treated economics as a set of "hypothetical" deductive analyses that began with "certain postulates" and yielded "tendencies" within political economy. These tendencies were not predictions (in the strict sense) because they took no account of the particularities in any given case. Rather, they formed general trends that might be realized to a greater or lesser degree in any specific situation, or perhaps not realized at all.

In Mitchell's view, this approach created a double mischief. First, it rendered economic theory practically immune to empirical correction because general tendencies could not be disconfirmed by any particular evidence (Mitchell 1924, 12). Second, it left economics unable to offer concrete policy guidance. As Mitchell explained, a science of tendencies had proven inadequate for economic planning during the First World War. When economists

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were asked to devise production or labor policies, "it seldom sufficed to say that a given action would have consequences of a certain kind," although that was the only guidance offered by "orthodox treatises." Instead, "the important thing was to find out at least in what order of magnitude these consequences should be reckoned." Managerial decisions required calculation, not loose generalization. Accordingly, Mitchell concluded, "in proportion as economists face real problems they will strive to cast even their general theory into the quantitative mold" (Mitchell 1924, 27–28).

To move from tendencies to quantitative predictions required "objective records of mass behavior" – that is, historical statistics – which in turn would permit economics to become a "science of behavior." Rather than deduce likely actions from an analysis of imputed motives (the procedure of classical political economists), future economists would gather statistical data in order to improve "the objective validity of the account [economics] gives of economic processes." According to Mitchell, this confrontation with the empirical record would transform the very nature of economics. As economists grasped the role of institutions and "social habits" in shaping behavior, they would recognize the necessarily dynamic character of economic theory, which would make "the cumulative change of institutions" a primary component of its work (Mitchell 1924, 22–28).

Economics, therefore, was fundamentally a historical science for Mitchell, albeit a soft Hegelian – or better, a Marxist – form of history in which one studied the dynamics of historical transformation. Significantly, Mitchell believed that Marx had grasped how "the central problem of economics" lay in analyzing the "cumulative change in economic institutions," and despite the flaws in Marx's analysis, he had nonetheless revealed the "scientific possibilities" within such studies (Mitchell 1924, 18). The task for future economists was to blend Marx's attention to historical evolution with detailed empirical research, all while recognizing (contra Marx) that the possibility of altering social and institutional structures through political action implied the potential to direct that evolution down alternate paths. Ultimately, Mitchell declared, "In economics as in other sciences we desire knowledge mainly as an instrument of control," which raised "the alluring possibility of shaping the evolution of economic life to fit the developing purposes of our race" (Mitchell 1924, 25). Statistics and historical analysis would thereby make possible what Mitchell elsewhere called "a method by which we may make cumulative progress in social organization" (Mitchell 1919, 232). Economics would once again become an applied science, now built on a firmer foundation.

For Mitchell, that future looked much closer after the First World War. Not only had war mobilization revealed the weakness of orthodox theory, it had also convinced a growing number of businessmen, social scientists, government officials, and philanthropists that the nation suffered from a dearth of empirical knowledge about the economy. By the mid-1920s, Mitchell could point to a range of new organizations dedicated to "economic research," including his own postwar cocreation, the NBER (Mitchell 1924, 28–29). The NBER made quantitative research the hallmark of its work and was soon collaborating with philanthropic organizations (such as the Carnegie Corporation) and U.S. Secretary of Commerce Herbert Hoover on a variety of research projects, notably estimates of national income and research on business cycles (Hawley 1974; Alchon 1985; Barber 1985). As these projects suggest, Mitchell did not envision economics as a source for radical, abrupt social transformation. Instead, empirical investigation would allow social scientists to describe and analyze the economy as an interlocking system; to recognize tensions, pressures, imbalances, and lags; and to highlight adjustments that might ease potential economic disruption or social unrest. It was a program, in the assessment of the historian Guy Alchon, dedicated to developing a "technique of balance" (Alchon 1985) predicated on recognizing historical patterns and understanding their relevance to the present (cf. Mitchell 1927, 56–57).

Mitchell's desire to make economics more relevant to policy discussions (exemplified in practice by the NBER's choice of research topics during the 1920s and its intermittent collaboration with Hoover) necessarily raised a thorny question: What role should economists play in the political life of a democratic nation? Mitchell took this issue very seriously, and his response rested on two principles.[1] First, he drew a sharp distinction between ends and means, with social science providing only the latter. As he put it in a 1936 lecture, "It is not the business of the social sciences to say what is good and what bad; all they can do is to trace functional relationships among social processes, and so elucidate the most effective means of obtaining whatever ends we set ourselves" (Mitchell 1936, 461). Mitchell embedded that distinction in institutional structures whenever possible, most notably in his efforts to keep the NBER at arm's length from policy decisions even when consulting for government agencies. Thus when the NBER was asked to prepare a report for Herbert Hoover's Committee on Unemployment and Business Cycles in 1921, Mitchell emphasized that the bureau would not offer "recommendations concerning what ought to be done, but facts which ought to be considered by those who have the responsibility of formulating policies." The resulting volume separated the NBER's contribution from the

1 Biddle 1998 provides the most detailed and nuanced evaluation of Mitchell's perspective on this question.

analysis written by Hoover's committee, and likewise contained a disclaimer absolving the NBER of any responsibility for the committee's policy proposals (Committee of the President's Conference on Unemployment 1923, 2, xxxiii). A similar arrangement governed the NBER's extensive work with the committee later that decade (President's Conference on Unemployment. Committee on Recent Economic Changes 1929).

To this distinction between means and ends, Mitchell wedded a strong belief in the pragmatic possibility of producing objective (universally accepted) social-scientific research, and once again, he structured the NBER to reflect this commitment. Officially, the NBER was led by a board composed of twenty-one directors associated with various academic, commercial, and labor organizations. Mitchell's description of the board emphasized its diversity, with members having "widely divergent training, experience and opinions" and collectively "represent[ing] all the important angles from which economic problems are viewed" (Committee of the President's Conference on Unemployment 1923, 1). These directors had to approve all NBER publications, and any director who disagreed with the majority vote had the right to publish a dissent in the resulting report. If the board eschewed radicals and kept itself largely to the political mainstream, its diversity was nonetheless sufficient to lend credence to the bureau's hope that its work would "carry conviction to Liberal and Conservative alike" (President's Conference on Unemployment. Committee on Recent Economic Changes 1929, xxxv).

This last statement reminds us that the pursuit of objectivity in the interwar social sciences was ultimately a political strategy: an effort to supersede the conflicts of a pluralistic society by crafting a domain of consensus that could form the basis for political action. Mitchell's assessment of the NBER's first year (1921) made the point clearly. To gather "men who represented so many and such divergent views on public policy" in hopes that they might approve extensive reports on "controverted social facts" had been "a very bold plan." Yet it had succeeded and thereby had justified Mitchell's vision: "We believe that social programs of whatever sort should rest whenever possible on objective knowledge of facts.... By putting this faith into practice we are making a contribution to the working methods of intelligent democracy" (Mitchell, quoted in Mitchell 1953, 355–356).

The phrase "intelligent democracy" hints at Mitchell's skeptical view of prior efforts at democratic social action, which, despite good intentions, had been ad hoc and often unsuccessful (cf. Mitchell 1919, 229). In this respect, his concerns matched those of many moderate and left-wing progressives, for the United States in the early twentieth century presented a difficult