Identifying a ‘Chicago School’ of Economics: 
On the Origins, Diffusion, and Evolving Meanings of a Famous Brand Name

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To economists the world over, “Chicago” designates not a city, not even a University, but a “school.” The term is sometimes used as an epithet, sometimes as an accolade, but always with a fairly definite—though by no means single-valued—meaning. In discussions of economic policy, “Chicago” stands for belief in the free market as a means of organizing resources, for skepticism about government intervention into economic affairs, and for emphasis on the quantity of money as a key factor in producing inflation. In discussions of economic science, “Chicago” stands for an approach that takes seriously the use of economic theory as a tool for analyzing a startlingly wide range of concrete problems, rather than as an abstract mathematical structure of great beauty but little power; for an approach that insists on the empirical testing of theoretical generalizations and that rejects alike facts without theory and theory without facts. (Friedman 1974, p. 3)

1 Introduction

The existence of a ‘Chicago school of economics’ has for many years been all but taken for granted in the profession, recognized in scholarly articles and books, in history of economics texts, and even in the popular media. Its defining characteristics and membership, though, have been the subject of controversy, and some have even questioned whether it is proper to speak in terms of a ‘school’ of economists associated with Chicago. The ‘school’ label at once connotes the perception of a certain commonality of purpose among the school’s members and a distinctiveness within the field. The emergence of a school of thought within the professional consciousness, then, is the result of scholars in the field identifying a particular sub-group as having these elements of within-group commonality and professional distinctiveness. Though the Chicago school has been the subject of no small amount of research over the past several decades, that scholarship has focused largely on persons, ideas, and influence—in short, on the school itself. No attention has been paid to how it was that economists came to apply the ‘Chicago school’ label to a particular set of ideas and the individuals propounding them—that is, to the origins and emergence of the


2 See, especially, Stigler (1962) and Bronfenbrenner (1962).
professional perception of a Chicago school of economics. This paper attempts to address that lacuna.

The typical delineation of a Chicago school traces its roots to the 1920s and 30s and to the persons of Frank Knight, Jacob Viner, and Henry Simons. The second generation of the school is generally associated with Milton Friedman and George Stigler—perhaps the two names with which the Chicago school has been most closely identified—and the third with a group of scholars that includes, among others, Gary Becker and Robert Lucas. Each of these individuals spent a considerable amount of time on the Chicago faculty. Some were trained at Chicago, others elsewhere. Their intellectual differences are in many ways striking, yet they are all key figures in the perceived Chicago school tradition. And, of course, a host of other individuals have been classified under the ‘Chicago school’ banner along the way.

The first expansive attempt to delineate a Chicago school of economics came in 1962, when UCLA’s H. Laurence Miller published a paper entitled, “On the Chicago School of Economics” in the Journal of Political Economy (JPE). Miller took the existence of a Chicago school—in the professional consciousness, at least—as given and attempted to identify “the set of common attitudes and interests which distinguishes [Chicago school economists] from the rest of the profession” (1962, p. 64). In doing so, he confined his analysis to the ideas of those whom he associated with Chicago; no effort was made to elaborate how the label, ‘Chicago school’ emerged and became, as he put it, “a recognizable and meaningful distinction” (p. 64). The present paper takes the publication of Miller’s article as its point of departure and looks backward in time, showing how the idea of a ‘Chicago school’ crept into the economics literature and what it was that economists believed made this ‘school’ sufficiently distinctive to merit the label.

The discussion will proceed as follows. Section II explores the origins of the identification of a ‘Chicago school’ of economics, including the initial references to it in the published literature. Section III presents some data on references to the school up to the time when Miller’s article was published in 1962 to demonstrate its diffusion in the literature and the extent to which various

3 Miller had earned his Ph.D. in economics from Harvard in 1956.
individuals were said to be associated with this school. An analysis of the ideas said to distinguish the ‘Chicago school’ is taken up in section IV. The final section offers some concluding thoughts on how our inquiry into this literature informs our understanding of the early history of the Chicago school.

II. Origins

A. Bookending the Analysis

Precisely when and where the perception of a Chicago school of economics originated is something of a mystery. The University of Chicago, as Friedman (1972, p. 27) once noted, has been “exceptionally fertile” when it comes to producing schools, giving us ‘schools’ in fields including philosophy, sociology, political science, literary criticism, and theology in the first half of the twentieth century alone. This propensity to associate the ‘school’ label with distinctive Chicago approaches, then, was not unique to economics and, in fact, likely played some role in the genesis of its application on the economics front. Of course, this alone does not tell us when and why economists began to perceive that there was a distinctive brand of economics being practiced at Chicago that would warrant identifying such a school.

One possible answer lies in a Chicago-spearheaded proposal for monetary reform developed in the early 1930s. This proposal, which most famously included a 100% reserve requirement for banks, was drafted (apparently) by Chicago’s Henry Simons and signed by nearly all members of the Chicago economics faculty. Despite the wide range of economists of varying theoretical/policy perspectives and university affiliations who actively supported many elements of this reform proposal—e.g., Irving Fisher, Lauchlin Currie, Gardiner Means, and James W. Angell—it became known as the “Chicago Plan,” a label seemingly placed on it by Chicago’s

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3 This memorandum is reprinted in Phillips (1995, pp. 191-98). The signatories included Garfield V. Cox, Aaron Director, Paul Douglas, Albert Hart (then a graduate student), Frank Knight, Lloyd Mints, Henry Schultz, and Henry Simons. Notably absent from this list is Jacob Viner. Knight noted in his covering letter to Henry Wallace, U.S. Secretary of Agriculture, “I think Viner really agrees but doesn’t believe it good politics” (Phillips 1995, p. 192). See also Laidler (1999).
Albert Hart (1935) in the *Review of Economic Studies.* Hart’s terminology stuck, and dozens of references to the “Chicago Plan” appeared in the literature between 1935 and 1962. It is plausible, and perhaps even likely, that this so-called ‘Chicago Plan,’ combined with the propensity to associated academic ‘schools’ with Chicago, fed into the identification of a ‘Chicago school’ of economics.

The earliest data point that we have for the identification of a ‘Chicago school’ of economics traces to the early 1940s and to the U.S. government’s Office of Price Administration. On January 11, 1988, Herbert Stein penned a letter to George Stigler, who had solicited Stein’s comments on a draft chapter on “The Chicago School” for his memoirs (Stigler 1988). Stein, like Stigler, had been a graduate student at Chicago in the 1930s, but he had little to offer Stigler in the way of comments, apart from one anecdote:

> I do remember this incident about the Chicago School. During the War (WW II), I believe in 1941 although it may have been early in 1942, I met Jacob Viner in Bassin’s Delicatessen, Pennsylvania Avenue, NW near 14th Street, in Washington. He asked me what I was doing. I replied, callowly:
> 
> “I’m working at the OPA. They don’t have much use for the Chicago School there.”
>
> To which he responded:
> 
> “Chicago School, Chicago School! What’s that? I’m not a member of it.”
>
> That struck me at the time as evidence of his desire to distance himself from Knight, Simons and some others. Did I then invent the term “Chicago School?” If I did, wouldn’t he have said; “I never heard of it.” [sic] rather than “I’m not a member of it?”

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5 Indeed, as David Laidler (1993, 1999) has convincingly demonstrated, this Chicago monetary viewpoint did not arise in a vacuum, for many of the ideas seen to be emblematic of the Chicago monetary approach during the 1930s can be found in the work of contemporaries and near contemporaries such as Ralph Hawtrey, Allyn Young, and Currie.

6 Indirect references to the plan are far more extensive; see Hart (1935, p. 105n.1).

7 Herbert Stein to George Stigler, January 11, 1998, George J. Stigler Papers, Addenda, Box 20, Special Collections Research Center, University of Chicago Library. Stein (1995, p. 233) repeats this story and defends his recollection of it.
If Stein’s memory was accurate, this story suggests that by the early 1940s there was an entity known as the ‘Chicago school’ and that it had a reputation for opposing price controls—the implementation of which was the bread-and-butter of the OPA.

Some twenty years after Stein’s alleged encounter with Viner, Miller opened his *JPE* article with the assertion that, “To a great many economists, the phrase ‘the Chicago School of Economics’ is a recognizable and meaningful designation.” His claim was that this school had “crystallized” in the days of Frank Knight, Jacob Viner, and Henry Simons (that is, the 1920s through mid-1940s) and, at the time of his writing, was exemplified in the persons and work of Milton Friedman and George Stigler. Miller recognized, though, that he was treading on potentially controversial territory here, observing that “a number of people who would obviously be included in the school argue that this designation has no real content—that the Chicagoans do not constitute a school in any relevant sense and that there is no difference between a good Chicago economist and any other good economist” (1962, p. 64).

For Miller, however, the differences were real and clear. “The Chicago School of economics,” he said, “is noteworthy, first and foremost, for its advocacy of a private-enterprise economy and limited government.” Miller acknowledged that many other economists held to this view; what distinguished the Chicago school economists from the others, he argued, were “a number of closely related attributes”:

- the polar position that he occupies among economists as an advocate of an individualistic market economy; the emphasis that he puts on the usefulness and relevance of neo-neoclassical economic theory; the way in which he equates the actual and the ideal market; the way in which he sees and applies economics in every nook and cranny of life; and the emphasis that he puts on hypothesis testing as a neglected element in the development of positive economics. (p. 65).

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8 The year 1946 saw Viner decamping for Princeton and the passing of Henry Simons, and Knight’s influence on the department was beginning to wane.

9 The only other names mentioned by Miller in association with (then) contemporary Chicago-school ideas were Ronald Coase (who at this point had no connection to Chicago), Reuben Kessel, and Simon Rottenberg. See Miller (1962, pp. 66n.10, 68n.13).
Miller allowed that there were elements of both continuity and difference between the earlier
generation of Knight, Viner, and Simons and the generation of Friedman and Stigler. But the extent
of the continuity was sufficiently great, for Miller, that all of these individuals were properly
grouped under the Chicago school banner.10

Miller’s claim that some scholars associated with the ‘Chicago school’ considered the term
devoid of content certainly applied to Stigler and (perhaps to a slightly lesser extent) Martin
Bronfenbrenner, both of whom penned rather harsh responses to his article. Stigler (1962), who had
been trained at Chicago (Ph.D. 1938) but had only returned as a faculty member in 1958,
effectively denied the existence of a Chicago school in anything but the professional (outside of
Chicago) mindset, emphasizing both the heterogeneity within the Chicago orbit and the
commonality between essential aspects of Chicago thinking and that of the larger profession.
Bronfenbrenner, who also had received his graduate education at Chicago (Ph.D. 1939) and was
then at Wisconsin, took a rather different tack, suggesting that, if one wished to speak in such
terms, there were actually two Chicago schools, with “the departure of Jacob Viner and the passing
of Henry Simons [being] the watershed between them” (1962, pp. 72-73).11 But Bronfenbrenner
was not convinced that even this typology was appropriate. Like Stigler, he emphasized the
heterogeneity at Chicago past and present, as against the monolith it was portrayed to be, as well as
the important points of both commonality and distinction between Chicago and the profession as a
whole. What drew his true ire, though, was what he perceived to be the “automatic rejection of
people and propositions because of past or present association with the (or a) Chicago School”—an
activity that he considered a “prevailing error” in the profession and akin to “witch-hunting, book-
burning, madness pure and simple” (1962, p. 75).

10 Miller also allowed that not all members of the Chicago faculty or those trained at Chicago should be
considered members of the “school” and that there were “a substantial number” of members who had never
been at Chicago in either capacity. “But,” he argued, “the Chicago view is dominant at the university and the
faculty there has been the energizing, central influence in the life of the school to date.” Because of this,
Miller concluded that “the Chicago name is not a misnomer” (p. 64).
11 Miller, in contrast, had emphasized a single school of two generations and an essential continuity.
For all of their objections to Miller’s characterization of the Chicago school, neither Bronfenbrenner nor Stigler disputed its existence in the larger professional mind. And indeed they could not, for the term had been tossed about quite liberally in the literature for more than a decade. Given the strenuous nature of their objections to the label, however, its genesis in the literature becomes just that much more interesting.

**B. The Identification of a ‘Chicago School’ in the Economics Literature**

If the origins of the perception of a ‘Chicago school’ are ambiguous, the sources of its first appearances in the literature are not. Though their 1962 commentaries suggested hostility to the idea of a Chicago school, it was Stigler and Bronfenbrenner who were themselves responsible for introducing the label into the literature.

Stigler’s reference was the first to appear in print—in April 1949—and it is found in his *JPE* review essay on *A Survey of Contemporary Economics* (1948), a volume overseen by a committee of the American Economic Association and edited by Howard Ellis of UC-Berkeley. The *Survey* consisted of a set of original essays by leading specialists that, in Ellis’s words, attempted “to provide to the economist outside a particular field an intelligible and reliable account of its main ideas … which have evolved during the last ten or fifteen years” (1948, p. v). Stigler, who was then at Columbia, found the volume less than satisfying, in no small part because the economists contributing to it were, from his perspective, “a much more homogeneous group, with respect to age, academic background, policy predilections, and theoretical affinities, than are American economists in general” (1949, p. 100). This, he argued, had resulted in a number of omissions, not least among which was that “Not all schools of thought are represented, or even heard.” He assembled from the name index a list of the thirty economists whose work was most frequently cited in the *Survey*, noting that, in this list,

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12 Of course, the fact that the *JPE*, then edited by the fabulously eclectic Harry Johnson, elected to publish Miller’s article speaks to the label’s currency at that time. Johnson’s publication decision may be attributable to his distaste for some of the ideas that he associated with certain of his colleagues, especially Friedman. See Johnson (1960) and Moggridge (2010).

13 This claim is based upon an exhaustive literature search using JSTOR, Google, Google Scholar, and various print resources.

14 Earl Hamilton, who was never identified with the ‘Chicago school,’ was editor of the *JPE* when Stigler’s article was published.
One misses the institutionalists—Veblen, Commons, Hamilton, etc. One misses all the luminaries of the Chicago school—Knight, Simons, and Viner. One misses all the leaders, save Kuznets, of the school emphasizing empirical work—Mitchell, A. F. Burns, Colin Clark, etc. (1949b, p. 100)

Stigler’s point here was not that the contributions of particular individuals went unmentioned in the Survey, but that certain distinct approaches to and perspectives on economic analysis—among them, that which he associated with Chicago—were absent from its pages. The explanation for these omissions, according to Stigler, was that these contributions were not among those favored by the individuals who “shared, and helped to mold, the view of economics that was dominant during this period” (p. 100). Unfortunately, Stigler did not take the further step of identifying what distinctive “Chicago school” ideas and methods were ignored by the volume’s contributors.

Bronfenbrenner was responsible for the next two references to a “Chicago school.” The first appeared in his review of Alvin Hansen’s Monetary Theory and Fiscal Policy, published in the Annals of the American Academy of Political and Social Science in July 1949. The occasion for Bronfenbrenner’s invocation of a “Chicago School” was his discussion of the many influences on Hansen’s way of thinking—including Keynes, of course, but also numerous figures who came before and after him. The influences since Keynes, Bronfenbrenner said, demonstrate “an admirable eclecticism,” with only “Functional Finance (Lerner) and the ‘Chicago School’ (Simons, Mints, Friedman) having nothing but targets to contribute to Hansen’s thought” (1949, p. 174)—the Chicago “targets” reference reflecting the Keynesian Hansen’s lack of affinity for Chicago’s emphasis monetary rules and its dim view of countercyclical fiscal policy measures. The Chicago school here, then, was a monetary phenomenon.

A year later, however, Bronfenbrenner offered a much more expansive take on the Chicago school in his survey of “Contemporary American Economic Thought” for the American Journal of

While Knight, Simons, and Viner are missing from this list, Stigler himself was among the top 30. Friedman, however, was not—though he was cited a handful of times (Ellis 1948, p. 474).

Lloyd Metzler, a Keynesian, was the only Chicago economist represented among the 13 contributors, nearly all of whom (including Metzler) were either on the Harvard faculty or had received their graduate training there.
Economics and Sociology. He made reference to a Chicago school no fewer than four times in this essay, and in four different contexts, giving us a sense—or at least one data point—for what was considered the Chicago school view circa 1950.

The first of these references was to “The so-called Chicago School of economic policy.” Knight, he said, was the school’s “intellectual parent” and Simons its “best-known publicist” (Bronfenbrenner 1950, p. 487, emphasis added).\(^{17}\) What distinguished this school, according to Bronfenbrenner, was that its members based “their [policy] prescriptions upon the ‘optimum conditions’ … which are supposedly satisfied by the ideal competitive price system,” believing that “these optimum conditions would, in fact, be realized quickly and painlessly in a free economy,” so long as certain conditions were satisfied on the policy front (p. 487).\(^{18}\)

Bronfenbrenner contrasted this approach with that of “Most American planners,” who “throw ‘economic rationality’ out the window, and prefer to operate on the quasi-military basis of ‘Father Knows Best’ or ‘Me (and My Gang) for Dictator,’ unhampered by any rules whatever”—work that he saw exemplified in the uses to which linear programming was being put during this time—as well as with that of socialists such as Abba Lerner and Oscar Lange, who believed that planning should be founded on rationality principles but that the competitive market system was unlikely to generate optimal outcomes (p. 488).

Bronfenbrenner’s three other references do even more to bring out the idea that the Chicago school held to views distinct from those of significant segments of the profession. On the economic growth front, he said, members of “the Chicago group already mentioned … think of investment outlets as nearly unlimited at going rates of interest (i.e., they see the demand for new capital as almost infinitely elastic) except for the influence of monopoly, particularly of labor monopoly as exercised through the trade unions,” a position that he contrasted with the Keynesian

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\(^{17}\) Bronfenbrenner cited Simons’ Economic Policy for a Free Society (1948) as evidence for this claim.

\(^{18}\) These conditions, Bronfenbrenner noted, were “(1) Elimination of all seriously monopolistic restrictions on the supply of goods and labor. (2) Monetary policy devoted to maintaining a stable price level, or, alternatively, a stable level of government expenditures, without any guarantee of full employment for products and resources which priced themselves out of their markets. (3) Mitigation of inequality by taxation (particularly the progressive personal income tax) rather than by interference with the pricing of goods and services” (1950, p. 487).
focus on the need for public investment and consumption stimulus (pp. 489-90). Bronfenbrenner also believed that “The Chicago School should be mentioned particularly” among those willing to abandon full employment as a policy criterion, though he allowed that its members were “not in complete agreement among themselves” on this score (p. 491).19 Lastly, Bronfenbrenner suggested that “the Chicago School strongly favors regulation of trade unions under the monopoly statutes,” as against “the great bulk of American labor economists” who either did not see unions as monopolies or rejected their regulation on practical grounds (p. 492).

For both Stigler and Bronfenbrenner circa 1949-50, then, there was a “Chicago school” that clearly occupied a distinctive space on the spectrum of economic ideas. It is also significant that each of them used the “Chicago school” label without feeling compelled to explain it—as if they expected that their readers would be well aware of the school’s identity. This, and Brofenbrenner’s reference to the “so-called Chicago School,” suggests that this characterization was not unknown within the profession at this time and perhaps that it had been applied by or at least used more frequently by outsiders. Still, it was two insiders who first utilized the term in print—though, as we shall see, the non-Chicagoans were not far behind.

C. A Still Earlier Reference?

Though Stigler and Bronfenbrenner made the first explicit references to a “Chicago school” in the literature, these were not the first published references to a “school” of economists at Chicago. In 1948, the University of Chicago Press published Economics for a Free Society, a set of essays by Henry Simons. The introduction to this volume was written by Aaron Director, who had been both a student and colleague of Simons at Chicago and had returned to Chicago to succeed him as the economist on the Law School faculty following Simons’ death. Director’s introduction, written on March 1, 1947, informed the reader that

Professor Simons occupied a unique position in American economics. Through his writings and more especially through his teaching at the University of Chicago, he was slowly

19 Simons and Mints preferred that monetary and fiscal policy be utilized to achieve price stability, regardless of employment effects, whereas Friedman favored a relatively constant level of real government expenditure and tax system, regardless of variations in prices and employment levels (p. 491).
establishing himself as the head of a “school.” Just as Lord Keynes provided a respectable foundation for the adherents of collectivism, so Simons was providing a respectable foundation for the older faith of freedom and equality.

Though Director did not refer explicitly to a ‘Chicago school,’ he appears to be suggesting that a subset of Chicago (and Chicago-trained) economists heavily influenced by Simons had coalesced into a group with a set of identifiable views, and that the profession was very much aware of this. Indeed, Simons had clearly worked to build such connections during his last years at Chicago, attempting to reinforce the emphasis on “free markets” and “political decentralization” that he considered a “distinctive feature of ‘Chicago economics’.”

D. Popularizing the ‘Chicago School’

Though the idea of a Chicago school of economics seems to have been confined to academic and perhaps policy-making circles in the 1940s, the year 1950 brought it to the general public when John McDonald published an article entitled, “The Economists” in *Fortune* magazine. McDonald’s mission was to introduce his readers to contemporary “economic theorists” and “show the drift of their thought” (1950, p. 109). The article’s focal point was Knight, who at that time was the president of the AEA and, as described by McDonald, the “elder statesman of the orthodox classical school of economics in the U.S.” (p. 109). But he was also, in McDonald’s view, something of a relic of a previous generation of economic thinking.

In tracing the evolution of economics ideas over the previous two decades and the major figures in that transition, McDonald identified the “Chicago school” as a “fortress of orthodoxy” defending the classical approach against the theory of monopolistic competition and the tide of Keynesianism that had “swept the profession.” Knight and Viner, he said, bred “such eminent younger classicists” as Friedman, Stigler, and Simons. The flavor of the school was said to be “Down with big government, big unions, big business, and all domestic and international forms of

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20 Some support for this can be found in W.A. Mackintosh’s (1948) review of the Simons volume.
21 Henry Simons, “Memorandum I on a Proposed Institute of Political Economy” (date unknown, circa 1944-45), Henry C. Simons Papers, Box 8, Folder 9, Special Collections Research Center, University of Chicago Library.
protectionism” (p. 110), and this viewpoint, according to McDonald, was grounded in the “remote ideal of a competitive society” with atomistic units pursuing their self-interest through which is generated “a high and stable level of efficiency in the use of resources” (p. 111). But the “scientific model” underlying this ideal, McDonald argued, was based on a set of “simplifying assumptions” that increasingly diverged from reality. While “orthodox classicists” clung to the model as an adequate representation of this reality—and here, McDonald cited Friedman’s defense in particular—most economists, he asserted, came to regard such a stance as having “aspects of dogma” (p. 111). In a profession where the evolution of views was in full swing and “doctrinal lines,” as McDonald (p. 121) put it, were blurring and “becoming increasingly difficult to find,” the Chicago school of 1950 stood out for its adherence to orthodox classicism.22

**E. Stein and Viner Redux**

If Stein’s memory was accurate, roughly seven years passed between his encounter with Viner and the first explicit references to a Chicago school in the literature. Viner’s own recollections, though, were rather different from Stein’s. In a 1969 letter to Don Patinkin, Viner said that it was not until after he left Chicago in 1946 that he “began to hear rumors of a ‘Chicago School’,” which, as he characterized the rumors, was supposedly “engaged in organized battle for laissez faire and ‘quantity theory of money’ and against ‘imperfect competition’ theorizing and ‘Keynesianism’.” Though initially “sceptical” of the school’s existence, Viner recalled that its reality was solidified in his mind when he attended a 1951 conference organized by a group of Chicago economists, the flavor of which reflected the tenor of the rumors he had been hearing.23 “From then on,” he said, “I was willing to consider the existence of a ‘Chicago School’ … and that this ‘School’ had been in operation, and had won many able disciples, for years before I left Chicago” (Patinkin 1981, pp. 265-66).

22 McDonald’s article does not seem to have led to a burst of ‘Chicago school’ references in the popular literature. For example, the first reference to the school in the *New York Times* did not come until July 31, 1964.
23 This conference is the subject of Director (1952), which is discussed in section IV.C., below.
Viner’s letter provides evidence both for and against Stein’s version of events. On the one hand, it supports the idea that a ‘Chicago school’ existed, as an approach or point of view, at the time of Stein’s alleged encounter with Viner. This, however, does not conclusively resolve the question of whether there was an entity known as the ‘Chicago school’ in the early 1940s, let alone whether Stein actually used the term in conversation with Viner at that time. It is obviously the case that either Stein or Viner was ‘misremembering,’ given that the encounter that Stein describes took place several years before Viner left Chicago. But the fact that both Stein and Viner were working in Washington, DC in the early 1940s—Stein at the OPA and Viner at the Treasury—lends some credence to Stein’s recollections, particularly given that the OPA, with its mandate for price controls, likely would not have shown much affection for what one might consider a ‘Chicago’ point of view.\footnote{24}

There is at least some additional reason to believe that the idea of a ‘Chicago school’ was ‘in the air’ in the early 1940s and that the designation may have originated among critics of Chicago. In his 1962 response to Miller, Bronfenbrenner said that he had “never heard of any ‘Chicago school’” during his time at Chicago, but that “Shortly after leaving the Midway” he “encountered the term full force.”\footnote{25} Moreover, he said, “It was usually used pejoratively,” as if the school’s members were part of “a sect or cult or clique” (1962, p. 72).\footnote{26} Given that Bronfenbrenner left the University in 1939, it is likely that he became aware of this label in the early 1940s—or around the time that Stein met up with Viner.

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\footnote{24 It also bears noting that Bassin’s Delicatessen, the location of the alleged encounter, had opened in 1939 and so was doing business at the time.}

\footnote{25 After completing his Ph.D., Bronfenbrenner held positions at Central YMCA College, Chicago (1939-40); the U.S. Treasury (1940-41); and the Federal Reserve Bank of Chicago (1941-43, 1946-47). His time at the Chicago Fed was interrupted by a period of wartime service (1943-46). (Source: \url{https://econ.duke.edu/uploads/assets/history/CV(2).pdf}).}

\footnote{26 Bronfenbrenner noted that at Wisconsin, where he had been on the faculty since 1947, the term “meant Pangloss plus Gradgrind, with touches of Peachum, Torquemada, and the Marquis de Sade thrown in as ‘insulter’s surplus’” (1962, p. 72). For the uninitiated, Gradgrind is the headmaster in Dickens’ \textit{Hard Times}, a utilitarian educator whose approach was emphasized facts, science and no emotions; Peachum a predatory businessman organizing and equipping beggars for their trade in the \textit{Threepenny Operas}; and Torquemada the first Grand Inquisitor in Spain.}
For the present, at least, the precise origins of the idea of a ‘Chicago school’ must remain a mystery. What we can say with certainty, however, is that the initial published references to it came in the late 1940s and from within the Chicago tradition itself. It is noteworthy that these references came from former Chicago graduate students rather than from within Chicago’s halls (unless Director’s reference is included here) and from individuals who still self-identified (and came to be identified by others) with this tradition, rather than from its critics. If the term did indeed originate with the critics, they presumably did not see fit to commit it to print. But if there indeed was a professional perception of the Chicago school in the early 1940s, its diffusion into the economics literature, particularly outside of Chicago, was rather slow in coming. This diffusion and the meanings ascribed to the label by those who employed it are the subjects to which we now turn.

III. Identifying a Chicago School: Diffusion by the Numbers

The term ‘Chicago school’ had a significant presence in the economics literature in the years leading up to the publication of Miller’s 1962 *JPE* article. A total of 56 published works referencing a “Chicago school” appeared between 1949 and 1962, with another nine making reference to a “Chicago” approach, tradition, etc. Of the 56 “Chicago school” references, nine came from economists trained at Chicago—six from Stigler and Bronfenbrenner and the others from Don Patinkin, Warren Nutter and Donald Watson. None, however, came from an economist actually working at Chicago until Stigler’s 1962 reference.

As figure 1 illustrates, the number of references to a “Chicago school” in a given year was never particularly large during this period, varying between zero and two from 1949 to 1956 before a marked increase beginning in 1957. Some of the impetus for this sudden acceleration, as we shall see, came from the publication of Friedman’s *Studies in the Quantity Theory of Money* (1956) and Edward Chamberlin’s *Towards a More General Theory of Value* (1957), but there was also a more general up-tick in the propensity to identify a Chicago school perspective on certain topics, as we shall see in section IV. Figure 1 also suggests that ‘Chicago’ may have become a shorthand way of
referring to the ‘Chicago school’ around 1957, and there is some evidence for that in the literature discussed in section IV, below.

Figure 1

![Graph showing the quantity of publications referring to the 'Chicago school' from 1948 to 1964.]

Note: The quantity numbers here are “publications.” Thus, a publication with multiple references to a “Chicago school” counts as one.

References to a Chicago school appeared in scholarly journals, books, and graduate theses during this period. The JPE is the outlet best represented among the journals, though only seven such references appear within its pages over our 14-year sample. As table 1 indicates, the range of journals including more than a single reference is reasonably diverse, though not terribly extensive.27 The appearance of Kyklos and Weltwirtschaftliches Archiv here highlights the international diffusion of the perception of a ‘Chicago school’ during this period. There were no fewer than 16 such references in non-U.S. journals—more than 25 percent of the total mentions—including journals published in England (1954),28 Australia (1955), France (1957), Germany (1958), the Netherlands (1957), and Argentina (1958). All but three of these were by non-American authors and usually in the native language—referring to, e.g., “die Chicago schule” and “l’école de Chicago”—which further reinforces the conclusion that the notion of a ‘school’ of economists at Chicago was spreading internationally.

27 One must bear in mind, however, that the number of journals in existence during this period was a small fraction of the number existing today. Among leading English-language economics journals of the period, only the Economic Journal and Econometrica contained no articles referencing a Chicago school.
28 The date appearing in parentheses here is the year in which the term first appeared in that country’s literature. LSE’s Lionel Robbins had referred to the “Chicago school” in a letter to Frank Knight in 1953.
One of the more curious aspects of these data is that more than one-third of references to a Chicago school appeared in book reviews. It may be that authors of book reviews considered that genre to be a less restrictive form and so felt more inclined to make such references in that context, whereas they would not do so in a traditional journal article. The comparative nature of book reviews may also have encouraged such references, as the review author is often attempting to situate the book and/or its author within a particular context. The author of a scholarly article, in contrast, may be more likely to compare his findings with those of, say, Milton Friedman than with the position associated with a particular school of thought.

The final issue that we must take up here is the perceived composition of the Chicago school during this time. Not all of those referring to a Chicago school put names with the label, though most did. Miller, as we have already noted, identified Knight, Viner, Simons, Friedman, and Stigler as members of the school in his 1962 article (with footnoted mentions of Coase, Kessel, and Rottenberg). The data, though, reveal that a much more extensive group of individuals was identified with the Chicago school in the larger literature, as table 2 indicates. Perhaps not surprisingly, references to Friedman dwarf those to all others said to be associated with Chicago, with Simons, Knight, Stigler, and Mints also receiving significant play. A host of others received

Table 1: Publication Outlets Including Multiple References to a “Chicago School,” 1949 - 1962

<table>
<thead>
<tr>
<th>Outlet</th>
<th>References</th>
</tr>
</thead>
<tbody>
<tr>
<td>American Economic Review</td>
<td>4</td>
</tr>
<tr>
<td>Economica</td>
<td>2</td>
</tr>
<tr>
<td>Journal of Farm Economics</td>
<td>2</td>
</tr>
<tr>
<td>Journal of Political Economy</td>
<td>7</td>
</tr>
<tr>
<td>Kyklos</td>
<td>2</td>
</tr>
<tr>
<td>Review of Social Economy</td>
<td>4</td>
</tr>
<tr>
<td>Southern Economic Journal</td>
<td>2</td>
</tr>
<tr>
<td>Weltwirtschaftliches Archiv</td>
<td>2</td>
</tr>
<tr>
<td>Books/Chapters in Books</td>
<td>7</td>
</tr>
<tr>
<td>Theses</td>
<td>2</td>
</tr>
</tbody>
</table>
two mentions, including Aaron Director, F. A. Hayek, D. Gale Johnson, H. Gregg Lewis, Warren Nutter, and Alfred Sherrard.\(^{29}\)

Table 2: References to Chicago Economists in Discussions of a “Chicago School,” 1949 - 1962

<table>
<thead>
<tr>
<th>Name</th>
<th>References</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frank Knight</td>
<td>12</td>
</tr>
<tr>
<td>Jacob Viner</td>
<td>3</td>
</tr>
<tr>
<td>Henry Simons</td>
<td>20</td>
</tr>
<tr>
<td>Lloyd Mints</td>
<td>7</td>
</tr>
<tr>
<td>Milton Friedman</td>
<td>35</td>
</tr>
<tr>
<td>George Stigler</td>
<td>11</td>
</tr>
<tr>
<td>T.W. Schultz</td>
<td>3</td>
</tr>
<tr>
<td>Albert Rees</td>
<td>3</td>
</tr>
</tbody>
</table>

The data in this table do not include individuals referenced in Stigler (1962) or Bronfenbrenner (1962).

Some may find it curious that Simons figures so prominently here, with mentions significantly greater than Knight and Stigler and vastly exceeding those of Viner. But as Stigler noted, Simons was, before his untimely death, “the Crown Prince of that hypothetical kingdom, the Chicago school of economics” (1974, p. 1). Nor was this simply an insider’s view. In his 1954 review of Friedman’s *Essays in Positive Economics* (1953), Oxford’s Peter Newman described Friedman as “perhaps the most able living representative of that school of Chicago economists associated with the name of Henry Simons” (1954, p. 259). Though he did not publish widely and is not nearly so well known among economists today compared to other Chicagoans of this period, Simons’ research, like that of Friedman, was often at odds with the mainstream and focused on subjects that came to have distinctive Chicago school positions associated with them—as we shall see in section IV. Viner’s positions, in contrast, were not nearly so distinctive. The fact that two of the three references to Viner came from Stigler and Watson adds to the sense that economists of

\(^{29}\) Sherrard is something of a curiosity here, in that, so far as this author has been able to discover, he had no connection to Chicago other than having taken a Chicago-esque position on monopolistic competition in a 1951 *JPE* article. See the discussion in section IV.E, below.
this period, at least outside of Chicago, did not associate Viner with the Chicago school—a result that no doubt would have pleased him.

The table 2 data largely validate Miller’s classification of Knight, Simons, Friedman, and Stigler as the school’s leading lights, as well as the significant emphasis he placed on Simons and Friedman in his discussion. Where Miller’s genealogy is at odds with the data and thus potentially misleading is in his contention that Viner was a major Chicago school figure and in his complete neglect of Mints, who received more that double the mentions of Viner in this earlier literature. The neglect of Mints may be an artifact of the time at which Miller wrote his article. An examination of timing of the references reveals while Simons and Friedman are cited consistently over the sample period, references to Mints all but disappear after 1957. Meanwhile, references to Stigler were largely absent until 1957, after which point he was cited annually in mentions of the Chicago school. This suggests that Miller’s characterization of the composition of the school would have looked rather different had he written his article five years earlier.

While these data provides ample evidence of the professional perception of a Chicago school during this period, they tell us little about what this school stood for in the larger professional mind. To get at this information, we must turn to an analysis of the contexts within which these references were made.

IV. Characterizing the ‘Chicago School’

The first of the post-Stigler/Bronfenbrenner references to a ‘Chicago school’ in the economics literature occurred in 1952 and, like all but a small handful of references over the next decade, came from outside the Chicago tradition. Most of these were of a critical nature, in the sense that the author was disagreeing with what he perceived to be the Chicago viewpoint. The characterizations of a ‘Chicago school’ here were also narrowly drawn, for the most part, being tied

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Simons and Friedman were each mentioned more than a dozen times by Miller, as against three mentions each of Knight, Viner, and Stigler. One might question whether this larger body of literature served as Miller’s source material for his essay and thus whether these data points are truly independent. The fact that Miller did not reference any of the 58 articles in our data set suggests that this is not an issue.
to particular areas of economic theory or policy rather than to some sort of larger ‘Chicago approach’ to economics as a whole. Coincidentally (or perhaps not), the various ways in which the Chicago school was characterized in these discussions track quite neatly with the elaboration of the Chicago school laid down by Bronfenbrenner at the start of the decade. That said, the Stigler and Bronfenbrenner articles were not citing to any significant extent by authors referencing a Chicago school, suggesting that their articles were not the impetus for the sudden tendency to apply this label.

A. Monetary Theory and Policy

Though monetary economics received the smallest amount of attention in Bronfenbrenner’s (1949, 1950) discussions of a Chicago school perspective, it was with this area of analysis that a ‘Chicago school’ was initially identified in the subsequent literature. The first two of such references appeared in 1952 and, coincidentally (or so it would seem), in the same issue of *Review of Social Economy*—the journal of what then was known as the Catholic Economic Association. The authors—Notre Dame’s George Wallace and Georgetown’s Josef Solterer—came from outside of the Chicago tradition, and they provided rather different perspectives on what was perceived as the Chicago approach to monetary matters in the early 1950s.

Wallace, reviewing the AEA’s *Readings in Monetary Theory* (edited by Princeton’s Friedrich Lutz and Chicago’s Lloyd Mints), suggested that the volume “leans heavily upon the Cambridge tradition of the cash balance approach as modified and supplemented by the Chicago variety of monetary economics” (1952, p. 170). He identified the latter with the ‘rules’-oriented approach found in the Simons (1936) and Friedman (1948) articles included in the volume, an approach that was a hallmark of the ‘Chicago Plan’—a stance echoed in Patinkin’s (1956, p. 237n.11) identification of the “Chicago school” monetary viewpoint some four years later. Wallace,

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31 Stigler, recall, had not referenced any particular areas of economics when referring to a Chicago school.
32 That association is now known as the Association for Social Economics.
33 Lutz, though not affiliated with Chicago, was heavily influenced by the German ordoliberal tradition and was listed by Simons among those outside the University of Chicago whom one could “trust” to ensure that his proposed Institute of Political Economy would continue to promote the classical liberal political philosophy that he associated with Chicago economics (Simons, “Memorandum I,” p. 11, cited in note 21, above).
though, was less than enamored of the overall tenor of the AEA volume, arguing that its “heavy bias in the direction of the Chicago school of monetary reform, buttressed by the latest Pigouvian theoretical apparatus,” was not representative of the monetary field (Wallace 1952, p. 172).

Solterer, in contrast, linked the “Chicago School” with “the concept of money neutrality,” citing a 1947 article by “Simons” as his reference for the Chicago position (Solterer 1952, p. 136, emphasis added).34 While the identification of the Chicago school with the quantity theory of money and the associated concept of monetary neutrality rings true to the modern ear, Simons was not an advocate of neutral money, and the quantity theory was at that point not a distinguishing feature of Chicago monetary economics.35

Subsequent references to a “Chicago school” in the monetary context did not come until the second half of the decade and evidenced an increasing tendency to identify the ‘Chicago’ approach with the quantity theory and the person of Milton Friedman. Some of this was likely due to Friedman’s own claims for a quantity theory “oral tradition” at Chicago—a tradition that, he said, included Simons, Mints, Knight, and Viner and was kept “alive and vigorous” by both faculty and students—in his classic “Restatement” of the theory in 1956 (Friedman 1956, pp. 3, 21).36 That the perception of a Chicago school was being cultivated at Chicago in certain quarters, at least, is evidenced in the publisher’s advertisement for Friedman’s book, which referred to Studies in the Quantity Theory of Money as “the only systematic statement of the theoretical position of the ‘Chicago School’ on monetary economics.”37

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34 Solterer’s comment is curious in that the “Simons” citation he provided here referred the reader to an article by “E.C. Simons” entitled, “The Relative Liquidity of Money and Other Things” (1947). While this might lead one to conclude that Solterer had mis-typed Henry C. Simons’ first initial, the article in question was in fact written by Edward C. Simmons, and Simmons had no connections to Chicago.


36 Stigler, in contrast, described the state of monetary theory at Chicago prior to Friedman’s arrival as “moribund” (1988, p. 150). See also the references cited in note 35, above. It should be noted that Friedman’s was not the first reference to an “oral tradition” at Chicago. Simons had implied the existence of an oral tradition in 1935, and George Leland Bach spoke specifically of “an ‘oral tradition’” at Chicago in his 1940 University of Chicago Ph.D. dissertation. See Simons (1935, p. 555) and Bach (1940, p. 36n.1). Neither of these references, however, has the same quantity theory specificity as Friedman’s comment.

37 This ad appeared in the back matter of the of the December 1956 issue of the JPE.
Perhaps not surprisingly, references to a Chicago tradition identified with the quantity theory began to proliferate almost immediately following the publication of Friedman’s volume. Both Robert Roosa (1958, p. 85) and Ralph Turvey took note of this “Chicago tradition” in their reviews of the book, with LSE’s Turvey adding that, despite Friedman’s claims about an oral tradition, “Some readers may be surprised to find that [his essay] is a rather agnostic document, not a Chicago manifesto” (1957, p. 367). While Turvey’s comment suggests that economists did indeed associate the quantity theory with the ‘Chicago school,’ Bronfenbrenner made the link explicit in a 1957 *AER* review of Alexandre Chabert’s *Structure Économique et Théorie Monétaire*, chiding Chabert for his “noteworthy omission” of the “rehabilitation of a generalized form of the quantity theory” by “the newer ‘Chicago School’ (L.W. Mints and Milton Friedman in particular)” (1957, p. 441). Over the next several years we find numerous mentions of the “Chicago school” in the context of the quantity theory, all but one of which are accompanied by a reference to Friedman.38

Even with the growing emphasis on Friedman and the quantity theory, references to earlier features of Chicago monetary analysis did not entirely disappear. Marquette’s Walter Froehlich, reviewing Friedman’s *A Program for Monetary Stability* for the *Review of Social Economy*, pointed to Friedman’s preference for monetary rules rather than discretion, noting that, in this, he “follows Henry Simons and the tradition which is known as the Chicago School” (1960, p. 189). Others, meanwhile, continued to associate a 100 percent reserve requirement (and, at times, related institutional reforms) with the “Chicago school” into the 1960s.39

All of this suggests that there were not one, but two ‘Chicago schools’ of monetary analysis being described (independently) in the literature in the 1950s and early 1960s, with some measure of commonality—but also important distinctions—between them. Though the notion that one could think in terms of two epochs of Chicago monetary analysis rather than a Chicago approach is generally associated with the retrospective analysis of Patinkin (1969, 1981), this demarcation actually was first raised already in 1960 by North Carolina’s Clarence Philbrook in a talk.

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39 See Oliver (1960, p. 147), Egle (1962, p. 6), Rothbard (1962, p. 113), and Watson (1960, p. 72).
subsequently published in Leland Yeager’s *In Search of a Monetary Constitution* (1962).

Philbrook, who had earned his Ph.D. at Chicago in 1949, referred not to a ‘Chicago school,’ but to the “Chicago Heads”—individuals singing a Chicago “song”—with an earlier generation, associated with Mints and Simons, holding to a “neoclassical theory of money” (Philbrook 1962, p. 27), and a later one, with the “now-leading Chicago head” Friedman, grounded in the quantity theory (p. 57).

**B. Agricultural Economics**

Oddly enough, the second set of ‘Chicago school’ characterizations to emerge during the 1950s referenced a field not even mentioned in Bronfenbrenner’s discussion, agricultural economics. This field, though largely absent from contemporary Chicago, played a significant role there from the time that T.W. Schultz was hired in 1943 and was the subject of one of the original Chicago workshops (Emmett 2011). The two initial mentions of a “Chicago school” perspective on agricultural economics occurred in 1955, and this literature also provides two of the first pieces of evidence for the internationalization of the perception of a Chicago school.

It was Stephen McDonald of the University of Texas who first described a “Chicago School” perspective on agricultural economics, doing so in *Social Forces*—a sociology journal with an interdisciplinary social science focus—in 1955. For McDonald, this school was identified with Schultz and D. Gale Johnson, and their argument that the low returns to agricultural labor in the U.S. were caused by deficiencies in out-migration from farming rather than by forces such as differing productive capabilities of farm and non-farm workers, union effects, or minimum wage laws (McDonald 1955, p. 117). This link between the “Chicago school” and agricultural over-employment is also found in Willard Cochrane’s (1959, p. 705) essay on supply control in the *Journal of Farm Economics*. The fact that Cochrane (Minnesota) wrote this paper during a stint as a visiting professor at Chicago and thanked Schultz and Zvi Grilliches for their comments in the

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40 Schultz’s deployment of standard price theory in agricultural economics was one of the contributions cited in his award of the Nobel Prize.

41 The only other reference to a “school” of agricultural economics prior to the 1970s came from Alexander Gerschenkron (1945, p. 685), who referred to “[Aleksandr] Chaianov and his school of agricultural economics” in Russia.
obligatory opening note suggests that the ‘Chicago school’ label was not considered particularly objectionable by those at Chicago when applied in this context.

The other two references to a ‘Chicago school’ perspective on agricultural economics during this period came from outside the U.S. The first was by G.O. Gutman of the Australian Bureau of Agricultural Economics and appeared in the Australian journal, *Review of Marketing and Agricultural Economics* in 1955. Gutman was dealing with issues of investment and productivity in Australian agriculture and associated the “over-employment and under-investment” thesis with “Schultz’s group” (among whom he included Johnson), which he referred to as “the ‘Chicago School’ of agricultural economists” (Gutman 1955, p. 239). But this internationalization was not limited to U.S.-U.K. circles. Two years later, when introducing his essay outlining a prospective framework for French agricultural policy in *Économie Rurale*, Denis R. Bergmann of the Institut National Agronomique spoke of the significant influence on his analysis of ideas imported from other nations. “In particular,” he said, “it is impossible to ignore—to mention only one name—work of T. W. Schultz and the Chicago school” on this work (Bergmann 1957, p. 4). These references, along with the earlier one by Newman (1954) in another context, suggest that the perception of a Chicago school was diffusing internationally by the mid-1950s.

**C. The Market and the State**

Bronfenbrenner’s characterization of a “Chicago school of economic policy” with its faith in the optimality of a competitive market system free of government controls also found no small amount of support in the literature of the 1950s and early 1960s, though regular references a ‘Chicago school’ position on this front took some time to develop.

The identification of a ‘Chicago’ policy perspective, albeit not under the ‘school’ label, was on full display in an April 1951 conference on “The Economics of Mobilization,” spearheaded by Aaron Director and sponsored by the University of Chicago Law School. The participants included a some seventy academics, policy makers, civil servants and businessmen, with healthy

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42 “En particulier, il est impossible d’ignorer — pour ne citer qu’un seul nom — les travaux de T. W. Schultz et de l’école de Chicago.” Bergmann (1957, p. 12n.17) also referenced Johnson in his discussion.
representation from Chicago past and present, and the focus of the discussion—published under the title, *Defense, Controls, and Inflation* (Director 1952)—was on the degree to which governmental controls should be operative in the process of mobilizing for war—in this case, the Korean conflict.

That the conference had a particular “Chicago” flavor and even agenda was noted repeatedly by the participants—perhaps most pointedly by Eugene Rostow, who was at that time a professor at Yale Law School. The prepared questions the group was to address during the conference, Rostow asserted, were “leading questions,” ones that he surmised must have been “drafted by economists in Chicago, and not by lawyers” because they did not “conform to the usual standards of Socratic neutrality which prevail in our law schools” (Director 1952, 196).

Northwestern’s Richard Heflebower reached a similar conclusion about the conference in his *AER* review of the proceedings volume, associating the tenor of the discussion with the influence of the “Chicago School”:

> From the opening session to footnote dissents to the one-chapter summary of the discussions, two sharply divergent policy programs were argued. One which was labelled that of the “Chicago School,” consisted solely of vigorous monetary control carried out by Federal Reserve open market operations. No direct controls over wages, prices, or uses of materials, or over total amount or direction of use of investment funds should be imposed.

(Heflebower 1953, p. 457)

As respects price controls, at least, Heflebower would have gotten no objection from Director, who argued during the conference that there *was* a distinctive ‘Chicago’ point of view on this front:

> All I plan to do is to state the position that price control should not be used. I apologize for the dogmatic character of the statements I shall make. My excuse is that I find it very difficult to argue the position. This in turn maybe due to the fact that the position is so

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3 Attendees with a Chicago background included Director, Friedman, Knight, Mints, Wallis, Stigler, Viner, Schultz, Hayek, and Homer Jones.

4 This “Chicago School” reference is actually Heflebower’s language; the term, ‘Chicago school” does not appear in the conference proceedings. The several other published reviews of this volume make no reference to Chicago.
much a part of the Chicago tradition that we have forgotten how to argue the issue. At Chicago the advantages of the market as a method of organizing economic affairs are valued too highly to be laid aside during so-called emergency periods. (1952, p. 158)

Director went on to quip that the skepticism regarding price controls on the part of some who had experience with implementing them during World War II “only shows that there is a hard way of learning such things, by going to Washington, and an easy way of doing it, by staying at Chicago” (p. 158).

The participants’ attitudes toward the perceived Chicago stance on price controls were far from sympathetic in many instances. RAND’s Charles Hitch, for example, commented that while his wartime experience with price controls had left him “with a considerable skepticism and dislike of them,” Director’s remarks on the subject made it clear to him that he had not “moved all the way to Chicago” (p. 217). Hitch was particularly concerned with attempts to ground the opposition to controls in price theory, as Stigler (p. 202-217) had done, arguing that this theory was inadequate for the analysis of wartime mobilization (p. 218)—a comment that Heflebower approvingly noted was “aimed directly at the logic of the ‘Chicago School’s’ position” (1953, p. 458). Striking a more practical note of objection, Michael DiSalle, then Director of Price Stabilization in the Truman administration, emphasized that he and his colleagues would return to their jobs following the conference because price controls are “a necessary implement in a time of dislocation, whether or not the University of Chicago experts think that [such] controls are necessary” (Director 1952, p. 248). Not all of the commentary by non-Chicagoans was critical, however. Leon Henderson, who had been the first director of the Office of Price Administration and continued to support price controls in wartime and peacetime, thanked “the University of Chicago … for keeping the flame alive and letting the term ‘free market’ not disappear” (p. 327).

Not everyone, though, accepted Director’s characterization of the Chicago point of view. We have already noted Viner’s adverse reaction to the proceedings, the organization and funding of which he subsequently labeled “ideologically loaded” (Patinkin 1981, p. 266). 45 Schultz, then

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45 See also section II.D, above.
chairman of the Chicago economics department, took issue with Director’s implied suggestion of a singular ‘Chicago’ position during the conference itself:

I may start by correcting the impression that Mr. Director may have left when he perhaps inadvertently implied there was only one point of view in economics at the University of Chicago. I can identify several, of which the particular one that is emphasized here today would be only one. We might call it the emphasis on price, as Mr. Director put it, and decentralization of economic activities in society. Certainly, there is another—the approach which emphasizes income and which emphasizes somewhat less decentralization. It is not represented here today. There is still a third. I would call it the historical or empirical approach, where one does look at history, political experience, and statistics and tries to draw lessons from these and, in doing so, uses both micro- and macro-theories as tools. (p. 191)

But Schultz’s stance found at best lukewarm support in Frank Knight’s assessment near the end of the conference:

it seems appropriate that I should say a word, at least, to express my loyalty to the Chicago tradition about which you have heard something. And I think there actually is a tradition in the economics group at Chicago to lean in the direction of free enterprise and of freedom rather than the opposite direction. This does not, of course, mean absolute freedom. We are not anarchists, and I think that is really the main point. In matters of principle it is always a question of how far and how; and it is a question which cannot be answered by formula. We recognize as a matter of course that the market system will solve some problems and not others, at least by itself. Many must be handled in part or entirely by governmental agencies and many burdens borne by these—that is, through them, at the cost of private citizens. (p. 295)

The pragmatism suggested by Knight did not hold water with Heflebower, however. The neglect of certain control-related issues crucial to the mobilization problem at the conference, Heflebower said, indicated “the viewpoint of those who prepared the agenda for they are of the ‘Chicago
School” and believe that “controls may damage the free market system permanently” (1953, p.
458).46

While these 1951 conference proceedings and Heflebower’s commentary on them reveals a clear perception that Chicago was associated with an opposition to governmental controls on the marketplace, explicit associations of Chicago with a free-market/laissez-faire/neoliberal point of view were otherwise absent from the literature until the late 1950s—at which point they began to appear at a rapid clip and almost uniformly in a critical vein. The “Chicago school” was associated with a “laissez-faire solution” to problems of commodities price stabilization (Kindleberger 1959, p. 605), placing “increased reliance on the market mechanism” (Lubell 1960, p. 601), and advocating “a radical return to a laisser-faire policy” (Weißkopf 1961, p. 13)—the last being a reference to Simons’ proposal to use, as Weißkopf put it, “the most radical government interference in order to preserve and restore competitive markets wherever possible” (p. 13n.23).47

It was at this stage, too, that we find more overtly political terms such as ‘liberal’ and ‘neoliberal’ entering the discussion as Chicago school descriptors. Indiana’s Louis Dow associated “Chicago School liberalism” with the “maximand … of optimum freedom for the individuals making up the society” in his 1960 Ph.D. thesis, saying that the Chicago school made this freedom both an end in itself and the means to the successful operation of a competitive system (1960, p. 199). Henry Oliver, Dow’s mentor at Indiana, associated the policy program of German neoliberalism—e.g., Walter Eucken, Wilhelm Röpke, and Friedrich Lutz—with that of the “Chicago School,” arguing in the QJE that the Germans resembled Simons and Chicago much more than they did the Austrian approach of Ludwig von Mises and others with whom they were sometimes associated (1960, p. 133; see also p. 118). In fact, the identification of the Chicago school with neoliberalism even made its way into an undergraduate textbook during this period. Writing on

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46 Heflebower noted the contrast between the Chicago position and that of the non-Chicagoans present: “In general those who had had active roles in World War II or Post-Korea mobilization, or had participated in the policy studies by such groups as the Committee for Economic Development or the RAND Corporation, favored the composite program which included at least some use of direct controls” (1953, p. 459).

47 Yale’s Robert Lee Hale took a similar line at the 1951 mobilization conference. Referring to Simons’ position that large-scale industry should be broken up into small competitive units, Hale argued the virtues of some monopolies, suggesting that “even members of the University of Chicago Economics Department” might agree to support them in certain instances (Director 1952, 293).
“Contemporary Economic Philosophies” in Economic Policy: Business and Government (1960), Donald Watson, who had completed his Ph.D. at Chicago twenty years earlier, associated neoliberalism with Vienna and Chicago, informing his student readers that “Neo-liberalism in the United States has even been called ‘the Chicago School of economic policy’” (1960, p. 72). Watson singled out Knight as “The leader and teacher of the Chicagoans” (p. 84), Simons as “perhaps the most outspoken and vigorous” of the American neo-liberals (p. 72), and Viner, Friedman, and Stigler (who Watson called “a caustic neoliberal”) as other members of this “Chicago school.” It was Simons, though, whom Watson (p. 84) identified as providing the “most nearly complete and explicit exposition of the neo-liberal philosophy,” that coming in his Positive Program for Laissez-Faire.48

D. The Economic Impact of Unions

By the late 1950s, we begin to see the emergence in the literature of references to a fourth Chicago school point of view—this toward unions and their impact. But here, as with monetary analysis, there were two distinct perceptions of the ‘Chicago’ position. The first, identified by Virginia’s James Schlesinger, associated the “Chicago view” with the position that “the economic power of industrial unionism is negligible” and, in particular, that the ability of unions to influence wages “has been grossly exaggerated” (1957, pp. 15-16). A year later, Schlesinger added that this viewpoint “emanat[ed] to so great an extent from Chicago” that “Chicago view” could be used as a term of convenience (1958, pp. 296-97). Gottfried Haberler (1958b, p. 476n.1) and Cyril Zebot (1961, p. 116n.5), too, associated this union neutrality position with the “Chicago School,” Haberler doing so in a 1956 address to the First Congress of the International Economic Association in Rome (Haberler 1958b) as well as in a paper published in Revista de Economía y Estadística (1958a).49 Both Schlesinger and Haberler cited Friedman (1951) as emblematic of this

48 Takashi Kiuchi’s (1960, p. 38) UBC M.A. thesis followed a similar line, identifying the “Chicago School” with the Mont Pelerin Society, founded by Hayek, and a focus on individualism and opposition to planning.

49 Haberler noted here, after bringing up the opinion that unions have no impact on wages under certain monetary policy conditions, “Esta opinion esta sostenida por los miembros de la ‘Chicago School’” (1958a, p. 90n.3). After having this position described to him at the 1951 mobilization conference, Otis Brubaker, who had earned his Ph.D. in political science at Stanford and was Director of the Research Department of the United Steelworkers of America, called it “the sheerest and utterest non sense that I have ever heard” and
point of view, though in his 1958 exposition Schlesinger asserted that it was Knight (1951) who had provided the “most forthright espousal of this position,”\textsuperscript{50} with Friedman and and Albert Rees (1951) named as other “proponents” of it (1958, p. 296n.2).\textsuperscript{51}

But there was a second perspective on unions attributed to the Chicago school, one that characterized them as anything but benign owing to the inefficiencies that they were said to introduce. Berkeley’s Walter Galenson, writing in the \textit{Industrial and Labor Relations Review}, argued that the “Chicago school” had “no particular interest in unionism \textit{per se}” (1959, p. 308). Instead, he said, its members, led by Simons,\textsuperscript{52} were concerned mainly with resource allocation in a market context and regarded unions “as rather unfortunate obstructions to full market freedom, thus, by definition, impairing the efficiency of the economic system.” Whereas other economic approaches were much more union-affirming, Galenson believed that the adoption of the “Chicago approach,” would likely result in a “drastic curtailment” of union activity (p. 308) One finds an even more stridently expressed illustration of this perception in Allan Flanders’ review of \textit{The Public Stake in Union Power}, edited by Virginia’s Philip D. Bradley.\textsuperscript{53} Flanders, of Oxford, criticized the volume for its “radical hostility to trade unions on the simple, if familiar, grounds that they obstruct the beneficial working of a free market economy” (Flanders 1960, p. 119). The source of this hostility, he suggested, was the strong representation of the “Chicago School” within its pages.\textsuperscript{54}

Such was the sense for a distinctively ‘Chicago’ view of unions in the late 1950s that Indiana University graduate student Louis Dow could write a Ph.D. dissertation entitled, “A

\textsuperscript{50} Schlesinger noted, though, that Knight had “never formulated his position thoroughly” (p. 296n.2).
\textsuperscript{51} Schlesinger (p. 297n.3) and Georgetown’s Cyril Zebot (1961, p. 116n.5) offered contrasting views on the extent to which this Chicago point of view was accepted in the profession.
\textsuperscript{52} Though it is Gregg Lewis who came to be most prominently associated with this Chicago school position, his name is noted only once in this literature—by Dow (1960).
\textsuperscript{53} Bradley had earned his Ph.D. at Harvard.
\textsuperscript{54} Chicago was represented in the volume by Knight, Hayek, Rees, and Lewis, though there was no shortage of other contributors who might be classified as fellow travelers. Ironically, Flanders did not mention a single member of the Chicago faculty in his review, but instead hung his anti-Chicago opprobrium on Harvard’s Edward Chamberlin, who, in the opening chapter, made the case for restricting union power. As we shall see below, Chamberlin was at least as hostile to Chicago as was Flanders—though for other reasons.
Critical Evaluation of the Wage Theories and Wage Policies of the Chicago School.” Dow picked up on the two strands of Chicago thinking just outlined, noting that the school’s members were “far from forming a perfectly homogeneous group” (Dow 1960, p. 3). The more pessimistic view, he said, owed to “Simons and his close followers” and located union-induced distortions in their monopoly power (p. 29). The wage-impotence thesis, Dow argued, came only later, through the work of Friedman and Stigler. While both groups believed that the demand side of the labor market was highly competitive, Simons felt that unions severely restricted competition on the supply side. Friedman and Stigler, in contrast, were of the mind that “competition abounds” (p. 4) on the supply side, too—or at least that “as if” competitive modeling was sufficient to capture the relevant features of the market in all but rare cases (pp. 6-13). What was consistent across these Chicago viewpoints, according to Dow, was the view that union activity should be restricted—by antitrust action if necessary—to the extent that unions have monopolistic impacts on wage rates (p. 2). Dow attributed this stance to the school’s “liberalism,” doing so in a way that dovetailed with the Chicago perspective on governmental controls, described above. Union coercive power, he said, was seen to run afoul of the individual freedom maximand, interfering with the operation of the competitive pricing system and thus adversely affect the interests of non-union workers, business owners and consumers alike (p. 17).

These divergent perspectives on the Chicago school view of unions were on full display in the reviews of Albert Rees’s *The Economics of Trade Unions* (1962). Princeton’s Richard Lester emphasized the impotence thesis when reviewing the book for *Challenge*, saying that, Rees’s approach “is generally that of the Chicago School, stressing market analysis and forces, and minimizing the consequences of expanding the role of collectives (unions, corporations and the state).” Lester went on to argue that the “appeal of the Rees type of approach”—and thus, presumably, that of Chicago—“is that it uses the concepts and methods of traditional economic

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35 Dow’s discussion, which ranges over theory, policy and methodology, is far too broad to examine in any significant detail here. As its title suggests, however, he had little sympathy for the views that he associated with Chicago.

36 Rees had been at Chicago since the mid-1940s, first as a graduate student working under Gregg Lewis and then as a faculty member.
theory.” But its “weakness,” he said, “is that it relies on dubious assumptions concerning management, unions, labor competition and the nature of employment under unionization” (Lester 1962b, p. 40). Solomon Barkin, Director of Research at the Textile Workers Union of America, came away from Rees’s book with a very different impression—though one no less critical. Writing in the *Industrial and Labor Relations Review*, he labeled the book a “competent exposition of the neo-orthodoxy of the ‘Chicago School’” in which unions are “a power group which, like other power aggregates, including capital, farmers, government and others, interferes with the operation of free markets and thereby produces economic inefficiencies and waste.” The result is higher wages, costs and prices, as well as reduced labor mobility and resource misallocation (1963, p. 319). Ironically, still a third take on the book was provided by Duke’s Allan Cartter, writing in the *AER*. Rees, said Cartter, provided a “balanced presentation” that was at once “sympathetic and objective.” Because of this, he concluded that “Those economists who like to categorize their colleagues and tilt at ‘Chicago School’ windmills will find little here to occupy their fancies” (1962, p. 1208).

**E. Price Theory**

The existence of a Chicago tradition in price theory has long been considered—by insiders in particular—one of the most distinctive elements of the Chicago School. Yet, apart from a passing reference to the “Chicago School” in the microeconomics context by Kenneth Boulding in 1953, the wider professional recognition of a Chicago school of price theory was rather slow in coming as compared with the monetary side, not emerging in any significant way until the late 1950s.

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57 Curiously, Lester (1962a) also reviewed the book for *Economica* but made no “Chicago school” references there.

58 The roots of this tradition are said to lie in Knight and Viner. Friedman and Stigler mark a second generation of this tradition, and Gary Becker and his students a third. On the history of Chicago price theory, see Medema (2011), Hammond and Hammond (2006), and Hammond, Medema, and Singleton (2013).

59 See Boulding (1953, p. 263), reviewing Heinrich von Stackelberg’s *The Theory of the Market Economy*. Noting that Stackelberg was both “a highly skilled and original economist and a Nazi,” Boulding echoed Bronfenbrenner’s allusion to Gradgrind in suggesting that Stackelberg’s background does not come through in the book, the “emotional content” of which he equated with “the most simon-pure representatives of the Chicago School.”
When it did appear, most of the focus was on a single aspect of the subject—the theory of market structures.  

The stimulus here was Edward Chamberlin, whose 1957 book, *Towards a More General Theory of Value*, attempted to make the case for a theory of value grounded in his model of monopolistic competition (Chamberlin 1933). Chamberlin devoted the book’s penultimate chapter to “The Chicago School,” which he considered very much the villain in his battle to more properly ground the analysis of competitive behavior.  

Though Knight, Simons, Friedman, and Stigler had been contending with Chamberlin’s theory for some years, it was only with the publication of his 1957 book that this opposition became identified in the literature as a “Chicago school” view.  

Citing Bronfenbrenner’s 1950 essay, Chamberlin said that “A ‘Chicago School’ of thought, with particular reference to *economic policy*, is familiar to economists, and has been frequently referred to as such,” and he approvingly noted Bronfenbrenner’s acknowledgment of the influence of Knight and Simons. But, he continued, “Such a school is recognizable too in the field of *economic theory*” (1957, p. 296, emphases added). Chamberlin identified Knight as the school’s “intellectual parent” here as well, adding that Friedman, Stigler, and “a number of others” must also be listed among its members (p. 296). What united these individuals and thus justified the “Chicago School” label, he said, was that each had “taught or studied at the University of Chicago” and subscribed to an interpretation of the theory of monopolistic competition that revealed “this common intellectual origin” (p. 296n.2).  

The distinguishing feature of this school, according to Chamberlin, was “the zeal with which the theory of monopolistic competition has been attacked, and in particular … the extraordinary set of misconceptions as to the nature of this theory which have emerged in the process.” In light of this, he elected to label this school “the Chicago School of Anti-Monopolistic

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60 Of course, we should not overlook the fact that much of the commentary on the Chicago view of competitive markets in the contexts of price controls and unions, discussed above, also speaks to the application of price theory and the inferences drawn from it, as does the agricultural economics of Schultz.  
61 Chamberlin also made a handful of references to the “Chicago School” earlier in this volume, but the meat of his discussion is found in the chapter discussed here. See Chamberlin (1957, pp. 4, 139, 293-94).  
62 See, e.g., Stigler (1949a) and Hammond and Hammond (2006), as well as the several references found in Chamberlin’s chapter.
Competition” (p. 296). The purpose of his essay, he said, was to “indicate the existence of such a school” (p. 296)—a statement indicating that, in Chamberlin’s mind at least, the economics profession did not at that point associate the “Chicago school” with this position and perhaps that he wanted to piggyback on the existing critical perceptions of the Chicago school in defending his theory.

Chamberlin located Chicago’s opposition to the theory of monopolistic competition in its conviction that the theory “denies” the that “the economy is ‘highly competitive’.” Based on this conviction, he said, Chicago prescribes that the theory “must therefore be up-rooted, cast in the fire, and burned” (p. 297). Chamberlin located the roots of these beliefs in Knight’s “extreme” view that economic theory is “identical with, and restricted to, the theory of ‘perfect competition’” (p. 297) and a particularly narrow interpretation of Marshallian market analysis. Especially bothersome for Chamberlin were the moves by Knight and Simons to characterize monopolistically competitive behavior as “fundamentally irrational,” a position he said was not held by “anyone outside the Chicago School” (p. 299). The Chicago defense of perfect competition was only further reinforced by a “tradition … strong in the Chicago School” that “the less economic theory has to do with the economic world we live in the better,” and exemplified, for Chamberlin, in Friedman’s (1953) methodological analysis (Chamberlin 1957, p. 298). Yet, he said, while “The Chicago School speaks ad nauseam” about the virtues of simple models, it did not “seem to occur to them that there might also be simplifying assumptions other than those identified with perfect competition” (p. 304).

The reviewers of Chamberlin’s book were quick to pick up on his attack, with “Chicago school” references occurring in no fewer than seven of the nine reviews appearing in JSTOR journals. This is noteworthy given the brevity of the reviews—only two of which extend beyond two pages—and the fact that Chamberlin’s “Chicago School” essay occupies barely 11 of the book’s 312 pages. Some reviewers—including those from Germany and France—referenced the

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63 See Simons (1944, p. 12n.8).
64 The exceptions are the reviews written by Frank Hahn and James Meade, both of which focus on the Chamberlin versus Robinson/Cambridge debate.
“Chicago school” only in passing; other commentators, however, took up Chamberlin’s discussion in a more expansive way.

While LSE’s G.C. Archibald followed Chamberlin’s lead in giving pride of oppositional place to the “Chicago School” in his *Economica* article on “Chamberlin versus Chicago” (1961), Virginia’s Warren Nutter (1958), who had been trained at Chicago under Knight and Viner, chastised Chamberlin for his focus on “sterile exegetic controversy” in singling out the “Chicago School” as “the heart of the opposition” (Nutter 1958, p. 1004), particularly given the long list of prominent economists—“a formidable opposition,” as he put it—arrayed against Chamberlin’s ideas. Avery Cohan (UNC-Chapel Hill) and Jerome Rothenberg (Chicago) took issue with Chamberlin’s attempt to defend his theory on the grounds of its greater realism, with Cohan arguing the virtues, in certain circumstances, of the “less ‘realistic’” industry supply and demand models favored by the “Chicago School” (1958, p. 487) and Rothenberg asserting that “the so-called ‘Chicago School’ (Knight, Stigler, Friedman)” was “correct in suggesting that realism per se is not sufficient justification” for Chamberlin’s approach (1959, p. 310-11). Archibald, meanwhile, charged the Chicago school with inconsistency on the methodological front: Though “‘Chicago’ methodology prescribes that theories be judged by their predictions” rather than by their conformity with reality (1961, p. 2), the school’s members, he said, failed to subject the theory of monopolistic competition to empirical testing and instead dismissed it on a priori grounds (p. 3).

While monopolistic competition references were the most prominent of those on the price theory front, we also find references to a “Chicago” approach to topics as diverse as demand theory (Yeager 1960), the analysis of costs (Peston 1961, p. 88, pointing to Friedman and Stigler in particular), and utility theory (Dantzig 1957, p. 3), as well as to a “Chicago school” opposition,

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66 The several mentions of Friedman and Chicago on the methodology front do not speak to the “measurement without theory” controversy in which Chicago may be thought of as central. The only reference of this sort is William Burke’s (1961, p. 177) characterization of Wesley Claire Mitchell as “the enfant terrible of the Chicago School of his day.”

67 Van Dantzig was writing on Leonard Savage’s approach to expected utility theory and included among Savage’s influences “the Chicago school of econometricists.”
led by Friedman, to Walrasian analysis in favor of the Marshallian (Yeager 1960; Kiuchi 1960, p. 46). Yeager’s *JPE* article, which dealt with the controversy stimulated by Friedman’s essay on “The Marshallian Demand Curve” (1949), referred to compensated demand curves as “‘Chicago’ demand curves” (Yeager 1960, p. 55) and was liberally sprinkled with references to a “Chicago approach,” “Chicagoans,” and “Chicago methodology”—describing the “Chicago” label, like Schlesinger had, as a “convenient shorthand” not intended to imply a “monolithic unity among all ‘Chicagoans’” (p. 55n.11). Stuart Greenbaum (Johns Hopkins), too, pointed to Chicago’s heritage in Marshall when writing on the controversy in economics over deductive mathematical theorizing in the *American Economist*. For Greenbaum, though, it was deductive theorizing that was most characteristic of this approach, and in the U.S., he said, “the contemporary Chicago School holds the banner of this tradition aloft most conspicuously” (Greenbaum 1962, p. 1).

V. Conclusion

In his 1988 *Memoirs*, Stigler contended that “There was no Chicago School of Economics when the Mt. Pelerin Society first met at the end of World War II” (1988, p. 148). That meeting took place in April 1947, or exactly two years before Stigler’s first Chicago school-referencing article was published. Stein’s recollection suggests that Stigler was off the mark, at least in terms of professional perceptions, and the widespread use of the label during the 1950s—in language suggesting that the readers were very familiar with it—only adds to the sense that economists understood that there was a ‘school’ of thought—distinctive but variously defined—associated with the University of Chicago already in the early 1940s. By 1962, the term was sufficiently topical—and perhaps also sufficiently ambiguous—that Miller felt compelled to provide a delineation of the school and its attributes, and the *JPE* to publish it.

Kiuchi’s M.A. thesis is the source of the most unusual characterization of the Chicago school found in our data. He portrayed the school as advocating a “Sociological Institutional Approach” to economic development and included Friedman (whom he identified as the “nucleus”), Bert Hoselitz, S.H. Frankel, J.S. Furnivall, Fritz Machlup, and Charles Wolf as members of the school. See, e.g., Kiuchi (1960, p. 32).

That same year, Philip Thomas, writing on “The Training of Economics in the United States” in *Weltwirtschaftliches Archiv*, noted that “economics departments at certain universities … have come to espouse particular points of view.” The first institution that he mentioned here was Chicago, saying that
Though the initial published references to this ‘school’ came from within the Chicago tradition, it was not long before the its existence and attributes were being discussed by scholars around the world and in outlets ranging from the leading American journals to graduate theses to general and specialized journals in Europe and elsewhere. And while Stigler and Bronfenbrenner had originally used the term in positive fashion, it very quickly became a preferred label of Chicago critics. But this school was also quite clearly a force with which the critics felt they had to contend. As Newman noted of Chicago already in 1954, “However much one may disagree with their assumptions and conclusions, it is none the less true that they constitute one of the few bodies of economists with a coherent, logically consistent, and well developed approach to the problems of economic policy, and their views command respect” (1954, pp. 259-60). The widespread critical use of the label may account for the hostility to it expressed by Stigler and Bronfenbrenner in their 1962 reactions to Miller, but it was not long before Chicago once again embraced it—though there is a strong case to be made that the divergence between Chicago and the rest of the profession on any number of fronts only narrowed in the ensuing decades.

References

“Reference is often made to ‘The Chicago School’ of neoclassical economic thought, a designation that originated because of the stature and influence of Frank Knight, but which has continued because of such men as Milton Friedman, Friedrich Hayek, and George Stigler” (Thomas 1962, p. 151). Thomas did not identify any other ‘schools,’ pointing only to Wisconsin and Texas as having once had “the reputation of being strongholds of institutional economics” and MIT (owing to “the eminence of Paul Samuelson”) as having “the reputation of excellence in mathematical economics” (p. 151).


