

Inventing Paradigms, Monopoly, Methodology, and Mythology at 'Chicago': Nutter and Stigler¹

Introduction & Summary

This paper focuses on Warren Nutter's *The Extent of Enterprise Monopoly in the United States, 1899-1939*.² This started out as a (1949) doctoral dissertation at The University of Chicago, part of Aaron Director's Free Market Study. Besides Director, O.H. Brownlee and Milton Friedman were closely involved with supervising it. It was published by The University of Chicago Press in 1951.³ In the 1950s the book was explicitly understood as belonging to the "Chicago School" (Dow and Abernathy 1963).⁴

By articulating the content, context, and reception of Nutter's monograph, this paper discusses four larger themes. First, I introduce the importance of Kuhnian conceptions of science to the methodological and institutional understanding of economics in the development of a 'Chicago' school of economics. While Thomas Kuhn was widely read and adopted in the social sciences and humanities in the 1960s and 70s (and thereafter), I argue that at 'Chicago,' proto-Kuhnian language can be found going back to the 1940s; in those early days it is partly used to *disparage* the achievements of economic theorizing as promoted by others. A more self-congratulatory Kuhnian self-understanding of economics as a mature paradigm starts to get adopted around 1955 by George Stigler. One important new claim is that the later Kuhnian language gets

¹ Material of this paper has been presented at the Amsterdam-Cachan workshop (2007), the Summer Institute for the Preservation of the History of Economic Thought at the University of Richmond (2009), a session sponsored by HES at ASSA, San Francisco (2009), where Professor M. June Flanders offered many comments, and the "Revisiting Chicago Economics" conference at Notre Dame, where many of my fellow, more knowledgeable participants provided extremely helpful insights and suggestions. David M. Levy called my attention to the Kuhn-Stigler correspondence and encouraged me to consider Nutter as a research topic worthy of interest. I am also especially grateful to Dan Hammond, Roger Backhouse, Spencer Banzhaf, and the editors of this volume for their diligent and encouraging comments on the penultimate, but by no means polished draft of this paper.

² Nutter (1923-1979) is an understudied character. Among economists he is probably best known for his founding role in the Virginia School with its influence on public choice and law and economics; he was also a leading expert on Soviet economy— against naysayers from all political angles together with (my old teacher at Tufts University) Franklyn D. Holtzman, he insisted that Soviet economic and military strength was vastly overestimated by the CIA (and the Soviets). More surprising, perhaps (if we forget Frank Knight's influence on Nutter), Nutter was strongly suspicious of economic imperialism; he believed in the reality of institutions; he was an early pioneer of analyzing path dependency in economics; Nutter was deeply suspicious of claims of value-neutrality in economics. As under-secretary of defense, he also was a strong critic of Kissinger's foreign policy, which he thought deeply romantic and dangerous. While I surmise he was on the wrong side of history when it came to Civil Rights issues, he is a remarkably independent thinker that played a surprisingly important role in shaping the ideological contours of the Goldwater/Nixon Republican party. A useful introduction to his views can be found in the essays collected in Nutter 1983.

³ It was later rewritten with Richard Einhorn and extended (published in 1969). Further study is needed in evaluating the changes between the editions.

⁴ Building on Chamberlin and Bronfenbrenner, they write: "Frank H. Knight, Milton Friedman, and George J. Stigler have been assigned the role of the "intellectual leaders" of the School, while the late Henry C. Simons has been referred to as its publicist. Other members include Aaron Director, G. Warren Nutter, Lloyd W. Mints, Harry Henig, Arnold C. Harberger, Simon Rottenberg, and Alfred Sherrard." (235) In their notes they cite Nutter 1951.

adopted in part to divest ‘Chicago’ from its shared roots with Institutionalist economics. So, this paper contributes to a better understanding of the formation of a shared narrative at ‘Chicago.’

Second, I introduce contextual themes from Milton Friedman’s writings in the late 40s and 50s to help us understand the nature of realism at Chicago. Nutter’s dissertation helps in reading and illuminating Milton Friedman’s famous 1953 methodology paper (hereafter F1953) in historical and intellectual context. (See Schliesser 2005 and Schliesser in press; this explains my focus on the 1951 edition and my neglect of later editions.)

Third, while this chapter notes some of the political ramifications of Chicago economics, my main aim is to help explain the manner in which Chicago attempted to chart a distinctive methodological course. This methodology has often been described as Marshallian with debts to the large-scale NBER studies. Rather than going over familiar territory, I call attention to the importance of proxies in Nutter’s empirical methodology. It is an unappreciated feature of the inductive, quantitative method that focused on the component structures of the economy that characterizes Chicago’s methodological outlook in this period. I show this by comparing Nutter’s dissertation to work done by Stigler, then at Columbia. We know from Stigler’s correspondence with Friedman that in this period they discussed methodological matters. What is less well known is that Friedman is explicitly credited for Stigler’s methodological insights in Stigler 1949. The fifth lecture, “Competition in the United States,” covers similar territory as Nutter’s project (this was noted by Fabricant 1953). Comparing the work by Stigler and Nutter sheds light on the nature of Chicago methodology as it was being developed away from foundations laid by Frank Knight and Henry Simons (both acknowledged as the source of Nutter’s “general outlook” in his acknowledgments) in the late 1940s and 1950s. I present my analysis through the published critical reception of both works among economists.

A fourth reason to focus on Nutter’s dissertation is that it was featured in a *Fortune* magazine article in January 1952. So, it provides a useful entry into how politically important ‘Chicago’ research was marketed to a wider audience. This connects to issues explored by Phil Mirowski and his students, Rob van Horn and Eddie Nik-kah. Nutter dissertation was later often cited in polemical work by Aaron Director, Ed Levi, and Milton Friedman (Van Horn forthcoming). So, Nutter’s dissertation can help us see how ‘sponsored’ research looks at ‘Chicago at the time. This is especially important because Mirowski and Van Horn have claimed that Director’s Free Market Study group promoted a change from classically liberal views on monopoly, which condemned labor and employer monopolies, to a more pro-business stance.

Section 1: The Construction of the Chicago “paradigm”

Before I focus more narrowly on Nutter’s dissertation, in this section I describe the continuity in the manner in which the scientific nature of economics is described at Chicago. The cluster of views that are prevalent at Chicago from the 1940s onward are remarkably similar to those made famous by Thomas Kuhn. So, for the sake of brevity, I call these views ‘Kuhnian.’ In fact, I provide evidence that George Stigler enthusiastically welcomed the publication of Kuhn’s

Structure and that he called attention to the similarity of Kuhn's views to those developed by Milton Friedman at Chicago.

Nevertheless, I argue that there was an important change in the deployment of this proto-Kuhnian language at Chicago: around 1955 there was a profound shift in the conception of economics from immature to mature science. In particular, there was a hardening of attitudes toward institutionalist competitors, which in many respects were closest in outlook to Chicago economics (see Stapleford, this volume). Stigler's historical and methodological work reflect these changes, including the construction of a narrative in which institutionalists' concerns increasingly came to be seen as not belonging to economics at all.

Nutter does not figure prominently in this section, but I show that Nutter and Milton Friedman embraced views that were (implicitly) targeted in Stigler's new narrative framework. Moreover, in section 3 I show that the kind of empirical work done by Nutter (and Stigler) in the 40s can be best described as engaging institutionalist economists with methods and tools very familiar to leading institutionalist economists of the day. This work was warmly welcomed by leading NBER economists. Of course, some institutionalist commentators did recognize that in practice 'Chicago' was ignoring certain questions and approaches off-limits. But as I show much of their criticism was internal to Chicago projects. So, the point of this first section is to claim that in effectively dissociating Chicago economics from institutionalism part of its own history becomes less easy to recognize.

This section consists of four parts. First, I provide evidence of the routine use of Kuhnian language in the self-conception of Chicago. Second, I call attention to two theses about the disciplinary autonomy of economics associated with Stigler's attempt to fashion a historical and conceptual narrative that promotes a more assertive self-understanding for economic science. Third, in particular, I show that Stigler attacks the view that economic problems and ideas are influenced by current events. Finally, I show that the function of Kuhnian language has evolved. For I argue that until around 1955 Kuhnian themes (*avant la lettre*) were used to show that economic science was immature.

1A: Kuhnian Paradigms at Chicago

I start my treatment with Melvin Reder's well known, influential 1982 retrospective essay, "Chicago Economics: Permanence and Change." One is immediately struck by the Kuhnian rhetoric that shows up in the paper: For example, Reder writes, "Chicago economics is a scientific sub-culture in the Kuhnian sense, and spoken of the "Chicago Paradigm" (or family of paradigms), or the "Chicago Scientific Research Program"..." (19); see also his claims that "Chicago-type innovations are "paradigm preserving" or "paradigm extending" rather than "paradigm shattering," (22; see also his use of "normal science," at 20).

In Reder's story Milton Friedman plays the crucial role in establishing a paradigm within Chicago; as he writes: "Being conscripted as his interpreters had the effect, in the 1950s and early 1960s, of making all Chicago economics appear to be dominated by Friedman and, I

suspect, had the further effect of bringing them much closer to his views than they would otherwise have come” (Reder, 32).

Given the central importance of Friedman to Reder’s story, one should not be surprised by the fact that this Kuhnian language even shows up completely unselfconsciously in Friedman’s 1976 Nobel lecture: “One consequence from the Keynesian revolution in the 1930s,” (Friedman 1992 [1976]: 282). As the reference to Keynes suggests, this language of “revolutions” was also used in the context of the reception of Keynes’ *General Theory* (Klein 1947). Proto-Kuhnian ideas were quite prevalent in reflection on methodology and structure of theories within the social sciences in the first half of the twentieth century (Backhouse 2006).⁵

The language of revolution and even incommensurability can be traced back well before the publication of Kuhn’s *Structure* to the 1940s in Friedman’s writings. For example, in his one major contribution to the history of economics, the 1955 piece on Leon Walras: “It is hard now for us to understand why this marginal utility analysis should have been regarded as so vital and revolutionary. We can repeat the formulae of the histories of economic thought that it gave a meaningful solution to the diamond-water paradox and so permitted demand to be assigned its proper role and the shackles of the cost of production or...labor theory of value to be overthrown...I do not believe such formulae carry real conviction or understanding. Partly, this is for the usual reason that an error once pointed out, seems obvious to those who never held it, though simply pointing it out did not make it obvious to those who had the error imbedded in the fabric of their thought. But I suspect the main reason is quite different, namely, that change in our general philosophical and methodological outlook that has been wrought, though by no means directly, by the developments in physical science, in particular by the replacement of the physics of Newton by the physics of Einstein. Surely this is why a chapter title like that of Lesson 10, “*Rarete*, the Cause of Value in Exchange,” strikes us as an anachronism” (Friedman 1955: 902). These are not isolated features of Friedman’s thought. For example, in his famous 1953 methodology paper he emphasizes the “tenacity with which hypotheses are held,” and why evidence is often so hard to document (F1953: 23).

This Kuhnian language is also to be found in Stigler long before the appearance of *Structure*. For example, in the fourth lecture of his *Five lectures on Economic Problems*, Stigler writes: “History is another eminent resource of the economist, but we have all revolted against historical economics” (38). See also his claim “Unless a science is thoroughly shaken up from time to time, its practitioners tend to become a spiritless and stultifying lot” (Stigler 1960: 301).⁶

Evidently proto-Kuhnian language has a long history within Chicago.⁷ What is the meaning of this? Now one thing we learn from Kuhn is that sciences with paradigms do not merely discard

⁵ See also Parsons 1938: 5, where pre-Keynesian developments are described. Parsons was closely studied by Stigler (Schliesser forthcoming).

⁶ This was one of the five papers that Stigler sent to Kuhn after *Structure* appeared.

⁷ Not just at Chicago, of course. As I noted above proto-Kuhnian ideas were debated throughout the first half of the twentieth century within and at the margins of economic thought.

history of their own development but also create mythic history.⁸ We see such mythic history tacitly at work in Reder's account. In discussing the precursors to Friedman, Stigler, Becker, and Lucas, Reder needs to deal with the Chicago department of the 1930s. He writes: "...some of whom were men of great distinction, were hardly Chicago economists—or economists at all—in the current sense of the term.* They represent the institutionalist tradition in American economics which was still very strong in 1940. * I shall not speak further of these men [i.e., Nef, Wright, Leland, Millis--ES], because they had little impact upon or interest in the theoretical and ideological skirmishes of their colleagues," (Reder 1982: 3).

Reder is by no means alone in his assessment. Thomas Sowell, one of George Stigler's PhD students, reports on one of his conversations with Stigler: "Commons...and his disciples were one sect in his cult and Thorstein Veblen and his disciples were another...[GJS:] "Institutional economics is dying out at a fantastic rate—though still not fast enough to suit me" (Sowell 1993: 788).

I call it mythic because it effaces the constructive and contrastive roles institutionalism played in the formation of Chicago economics. Malcolm Rutherford has already done much to show how indebted National Bureau research, including work done by important Chicago figures, was to institutionalist economics (see Rutherford in press, Hammond unpublished ms, Stapleford this volume).

Throughout this paper I offer further evidence for the claim that at least an important part of the empirical work done by 'Chicago' in the 40s can be best described as engaging institutionalist economists with methods and tools very familiar to leading institutionalist economists of the day. Reder's account is mythic because it cannot do justice to these facts. In fact, in what follows I argue that from the 1950s Stigler self-consciously promoted the template of Reder's narrative. Friedman became Chicago's face to the world, but the school understood him, in part, in terms framed by Stigler.

IB: The Continuity and Separability Thesis

In an article from 1955, Stigler argues for what we might call a "continuity" thesis: "...in the broad, *the boundaries* of the discipline have not varied much" (Stigler 1955: 36). There is no hint that Stigler would have excluded the institutionalists' writings from belonging to the discipline at that stage. In fact, even as late as his presidential address to AEA, Stigler favorably contrasts the empirical methods of early American Institutionalists like Commons and Clarke over English Classical and neo-Classical authors, see Stigler 1965: 11, although he makes sure to exclude the "denunciations of theory by the American Institutionalists" from being responsible for the "demonstrated successes of the pioneers of the quantitative method—the Jevons, the Mitchells, the Moores, the Fishers," 16).

⁸ See Kuhn 1996: 136-43. For more discussion of this see Schliesser 2008.

Moreover, in the same 1955 article Stigler forcefully argued for a distinctness of economics from psychology: "...[Adam] Smith's professional work on psychology...bears scarcely any relationship to his economics, and this tradition of independence of economics from psychology has persisted," (Stigler 1955: 44). I call this a "separability" thesis. Economics is autonomous from other sciences, especially psychology.⁹

This concern with the continuity and separability theses also shows up in Stigler's correspondence with Friedman:

"1. After a theory has been developed and tested and much used, its applicability to certain classes of problems becomes established. These classes of problems may be completely specific or objective as in the use of engineering formulas. Or they may be more loosely specified.
2. At all times there will also be many questions that do not clearly fall within or without the domain of the theory, and only further experiment can tell us whether a given problem should be handled by a given theory.

This distinction is perfectly inaccurate, in that even under 1 the theory will be less than perfectly precise, and it is also trivial, in that it says only that some things are known better than others...You are clearly thinking of class 2 most of the time; whereas I put more weight on class 1. The routine work of a science falls mostly in 1; the improvement of a science in 2," (GJ Stigler to M Friedman, 30 November 1952).¹⁰ Leaving aside the interesting biographical and psychological issues this raises, it is clear that Stigler recognizes that the kind of potentially revolutionary science that Friedman is engaging in may lead to re-drawing of theoretical boundaries.

I quote from Stigler's 1955 essay for a reason. In an hitherto unpublished letter from March 14, 1963. Stigler contacted Kuhn shortly after *Structure* (1962) appeared. This is not the place to analyze the Stigler-Kuhn correspondence in detail or to investigate what role Stigler may have had in getting Kuhn's *Structure* published (Stigler was on The University of Chicago Press oversight board in the period).¹¹ In the correspondence Stigler shows great warmth toward Kuhn's project. At one point Stigler invites Kuhn to The University of Chicago to give an unlimited number of Walgreen lectures (at \$1,000/per lecture). Kuhn declines.

In his correspondence with Kuhn, Stigler claims that much of what Kuhn had argued in *Structure* had already been pioneered by Stigler and Friedman. There is some (although by no means complete) truth to this. Stigler includes five of his papers as supporting evidence for this claim. One of these is the 1955 *Economica* paper.

Now in Stigler's letter to Kuhn he raises a dilemma for Kuhn: if 'revolutions' are only "displacements" then "many of Kuhn's statements are tautologies;" if revolutions are extensions of methods then it is "not true...that any large part of the previous paradigm was replaced, for a more or less comfortable reconciliation was achieved after a period of time....It is part of a

⁹ Backhouse and Fontaine 2010 argue that among the social sciences economics was the exception in resisting psychology's foundational claims.

¹⁰ All my quotes from the correspondence between Stigler and Friedman are from Hammond and Hammond 2006.

¹¹ I will tell the larger story in Schliesser Unpublished ms.

theory's formulation that it have a domain. The theory can be applied to problems of a type to which it has been applied before, or it isn't a theory, and it need not answer questions outside the domain." The second fork of Stigler's dilemma (to which he inclines) is required for Stigler's continuity thesis for economics. This is all to say, Stigler has very clearly thought through what the continuity (and distinctness) thesis entails. In the next sub-section I show that Stigler's continuity and separability theses are developed in opposition to both institutionalism and fellow Chicago-economists.

IC: Against the "Environmental Theory"

In the 1955 *Economica* paper, Stigler is keen on attacking the historicist claim that economic theorizing is influenced by current social events. Stigler calls it the "environmental theory." Now the only two named opponents in Stigler's piece are the historian of economics (and sociologist of knowledge) W. Stark and Wesley Mitchell, one of the leading institutionalists of the previous generation (who had died in 1948) and one of Friedman's mentors at NBER. So, at least in 1955 Stigler still thinks the institutionalists are worth engaging with, even though he is arguing against them.¹²

In response to the "environmental theory," Stigler develops an opposing historical narrative: "Beginning with the Physiocrats, economics began to be cultivated increasingly by scholars, and scholarly values such as consistency, generality, precision, and elegance began to be introduced. In the period of the classical economics, this disciplinary aspect of economic study became increasingly more prominent. Hume, Smith, Malthus, Senior, Whately, Longfield, and Cournot all had scholarly, and usually academic, orientations towards economics, and after 1870 this orientation became, not merely dominant, but well-nigh exclusive. Thus it is a sign of the maturity of a discipline that its main problems are not drawn from immediate, changing events." The last line is really remarkable in anticipating Kuhnian language.

In his response to Stigler, Kuhn singles out this aspect of Stigler's argument against the "environmental theory" as in accord with his own views. Kuhn writes (implying he had Stigler's papers), "I invariably point to just the characteristics of the discipline that you emphasize so often in the articles you have sent me. The relative immunity to external changes is one of these," (Kuhn to Stigler October 24, 1963).¹³

So, we see Stigler constructing a Kuhnian, mythic history of his own discipline with a continuity and separability thesis. I call it mythic because Stigler was undoubtedly aware that in Hume and

¹² The passage that I quoted from Sowell's memorial to Stigler, starts with "The once widespread assumption that changing theories reflected contemporaneous events was part of a more general school of thought-half economics, half sociology, and all mush-known as "institutional economics.""

¹³ The correspondence can be found in the George Stigler Papers at The University of Chicago. I thank Stephen Stigler for permission in accessing these.

Smith the economic theory is founded on a theory of the passions;¹⁴ not to mention that Stigler once quoted Talcott Parson's analysis of the philosophical presuppositions of Marshall approvingly—of course, Parson's main point in his interpretation of Marshall is that Marshall was committed to a moral-psychological ideal that influenced how Marshall thought of progress.¹⁵

There may be also significant unnamed targets for Stigler's criticism because we have seen that in the passage from Friedman's piece on Walras (also from 1955), Friedman toys with an intellectualized version of the environmental thesis.¹⁶ (Recall: changes in physics influence changes in economics.) This is indicative of Friedman's own Institutionalist heritage. Friedman is not the only person at Chicago embracing the environmental thesis. In "Is Competition Decreasing in Our Economy?" (1954), Warren Nutter, too, uses an "environmental theory" to explain the popularity of the decline-competition-thesis during the Great Depression: "The thirties were a time ripe for acceptance of any simple explanation of what seemed then to be a catastrophe of indefinite duration...it was natural for [the economist] to look to monopoly, among other things, as the villain of the piece," (quoted from Nutter 1983: 73).

In the 1951 book, Nutter was a bit more restrained in espousing "the environmental theory:" "A substantial revival of interest in the problems of market monopoly occurred in the United States during the 1930s. The renewed interest was reflected in the enthusiastic reception accorded to the Robinson and Chamberlin works on imperfect and monopolistic competition. It was also reflected in the increased vigor of anti-monopoly policy under the direction of Thurman Arnold. Developments in theory and antimonopoly policy plus searches for explanation of the depression stimulated the output of empirical research," (Nutter 1951: 11).

Of course, early in his career *even* Stigler held to a version of the "environmental theory!" I quote from the concluding paragraphs of Stigler's third LSE Lecture: "therefore, the technical apparatus of the classical economics was best precisely in those areas, and on precisely those subjects, where the issues were posed by concrete problems of the day" (Stigler 1949: 35).

So, Stigler's rejection of the "environmental theory" is as much a rejection of the self-understanding of institutionalist economics as it is of leading Chicago figures, including him. But Stigler does this in a series of papers constructing a historical narrative that makes it impossible for his students and followers to recognize the institutionalist contribution to Chicago economics.

¹⁴ See, for example, Rotwein's introduction in Rotwein 1955. It is somewhat ironic that Stigler's hero, Adam Smith, who was no stranger to constructing self-serving histories of political economy, insisted that most of the theories he was arguing against were drawn from the predominant interest of the times in which they were formulated (see the general Introduction to *Wealth of Nations* and also the Introduction to Book IV); hence the mercantile system was drawn from the mercantile class; the agricultural, or physiocratic system from landed interests, etc.

¹⁵ Stigler writes in his study of Alfred Marshall, "No attempt will be made to discuss the numerous commentaries [on Marshall]...there is no need to reproduce Parsons' path-breaking analysis of Marshall's philosophical preconceptions and their influence on his doctrines." (Stigler 1941: 61-2). Stigler cites Parson's early articles on Marshall from the *Quarterly Journal of Economics* not *Structure*.

¹⁶ We know that Friedman and Stigler exchanged papers for comment, but I cannot prove this in each instance.

It also makes it very hard to recognize that in the 40s and 50s, Chicago's stance toward institutionalism, while critical, was not as hostile as it became in Sowell's recollections of Stigler. But the reason for this lack of hostility is quite surprising: in the 1940s 'Chicago' claimed that economics was by no means a mature science—one might say that Milton Friedman thought economics was still in its pre-paradigmatic phase. Or so I argue now.

1D: The immaturity of Economic Science

While from circa 1955 onward Stigler was inclined to argue that economics was a mature science, this was not the prevailing rhetoric at 'Chicago' in the 40s and 50. For example Stigler's stance in the fourth of the *Five Lectures* at LSE was quite deflationary: "Economics is still a primitive discipline as compared with the more advanced of the natural sciences in that it possesses relatively few tested uniformities of economic phenomena. This primitive state is revealed by the lack of specificity and of accuracy of economic predictions. The chief reason for economics' undeveloped state are the objective study of economic phenomena began relatively recently, and the phenomena to be explained are in their totality very complicated. In the present early stage of economic study, the economist as scientist must be largely occupied with the isolation of these uniformities in his subject matter. This view has implications for the type of research that is most urgently needed, but it has none for the impossible choice between "deduction" and "induction". Until we possess many uniformities, we cannot erect broad analytical systems which are likely to be illuminating in the areas where uniformities have not yet been isolated. This is true because it is a variety of uniformities calling for systematization that gives rise to a *useful* analytical system; with only a few uniformities, too many plausible (but vague and conflicting) generalizations are a hand... The economist as a scientist is where the physicist was when he was discovering the properties of the lever, not at the stage when he was discovering the laws of motion," (Stigler 1949: 41). Stigler has no doubt that economics is still immature. For him this means that economists should try to isolate stable economic regularities (phenomena) that can be the foundation for future economic theorizing.

Again, this is by no means a view exclusive to Stigler. We find very similar sentiments in Milton Friedman's referee report about the Cowles commission to the Rockefeller foundation: "we someday hope to have a general theory of economic fluctuations.... a general model must be based on precise tested knowledge of the behavior of component segments of the economy, on a reasonably exact and comprehensive knowledge of the phenomena generalized. In the absence of such knowledge, there will be such a wide variety of general models capable of explaining the limited number of observed phenomena that it will not be possible to choose rationally among them...it will take decades of careful monographic work in constructing foundations before we shall be ready to put up the kind of superstructure that the Cowles commission hope to create full blown...The Cowles commission staff itself includes... able people who have an almost religious belief in the unique correctness of their approach.... Almost without exception, the people listed are primarily mathematicians or statisticians rather than economists and have had

no occasion to do careful scientific quantitative work on a limited segment of the economy...” (Friedman, May 26 1948; Rockefeller Archives).¹⁷

Thus, in the late 1940s Friedman and Stigler are united in the claim that economics is by no means a mature science. Both insist that this is due to a lack of adequate generalizations that can provide a robust analytical core that can generate further generalizations. In the absence of this, any number of theories can explain the same data; what’s more without such robust core there will be no rational way of choosing among possible alternatives. (Stigler and Friedman are here inching up toward a statement of what is known among philosophers as the Quine-Duhem thesis, more about which in section 3C below.)

So, in the 1940s, Chicago types are claiming that the maturity of economics as a science is a long way off—requiring a lot of empirical, “quantitative work” about the component segments of the economy, that is, to establish Marshallian demand. Without the benefit of this archival material (nor drawing on Stigler), Daniel Hammond (1996) and Kevin Hoover (2004) have rightly called attention to the deep Marshallian roots of Friedman’s methodological claims in the famous 1953 paper. So I am not going to discuss their provenance further here. But in these quotes note a tantalizing bit of evidence for my claim about the changing self-understanding of “Chicago” in this period. In 1949 Stigler is still comparing the status of economics with that of the (pre-Galilean) scientific knowledge of the “law of the lever.” By contrast, by 1953 Friedman is confident in comparing economics to the Galilean law of Fall. (F1953, 16-19; he returns to the example elsewhere in the paper at 24, 36). That is, economics is very close to discovering its equivalent to the laws of motion.¹⁸

In his Presidential Address to the American Economic Association, Stigler can confidently state that economics is undergoing a “scientific revolution of the very first magnitude--indeed I consider the so-called theoretical revolutions of a Ricardo, a Jevons, or a Keynes to have been minor revisions compared to the vast implications of the growing insistence upon quantification. I am convinced that economics is finally at the threshold of its golden age--nay, we already have one foot through the door,” (Stigler 1965: 17).

So, without wishing to claim exactitude for these dates, in the early 1950s ‘Chicago’ changed its tune from a Knight-ian skepticism about the status of economics as an empirical science. While maintaining Kuhnian language, it shifted its conception of economics from immature to mature science.¹⁹ With the departure of Knight-ian caution came a hardening of attitudes toward institutionalist competitors, which in many respects were closest in outlook to Chicago

¹⁷ I thank Marcel Boumans for calling my attention to it.

¹⁸ For more on the philosophic significance of this, see Schliesser (2005). Of course, Friedman is not the first to offer such analogies; as Roger Backhouse pointed out to me it also shows up, for example, in the Koopmans-Vining debates over measurement (Koopmans 1947 and 1949; Vining 1949a and 1949b) or Robbins 1945. Incidentally, Vining was also a Chicago PhD and a colleague of Nutter at Virginia. See Brady 2007.

¹⁹ Cherrier, this volume, has argued for the consistency in Friedman’s views from the 1930s onward. My argument suggests this requires modification with regard to some of the details, although these need not impact her overall claim.

economics. One central reason for the rejection of the Institutionalist economics was its commitment to the (historicist) environmental theory of economics. Stigler's historical and methodological work reflect these changes, including the construction of a narrative in which institutionalists' concerns increasingly came to be seen as not belonging to economics at all.

Before I turn to the details of Nutter's dissertation and Stigler's argument in his fifth LSE lecture both directly engaging institutionalist arguments, I wish to clarify something about Friedman's methodology.

2: Friedman's Methodology

In order to understand and evaluate the methodology of Nutter's research, we need to remove a persistent misinterpretation of Chicago methodology. For a variety of reasons Friedman's famous 1953 methodology essay was read as advocating instrumentalism in economics. Now while this understanding of Chicago methodology post Becker-Stigler 1976 might be justified, this once standard reading of F1953 is mistaken. Others before me, notably Kevin Hoover, have already argued that as a straightforward reading of the paper, attributing instrumentalism to Friedman is problematic.²⁰ I read Friedman's essay as setting out a realist methodology that is meant to contrast favorably with multiple targets: i) what he calls Walrasian (general equilibrium) theory; ii) the econometrics of the Cowles Commission; iii) Monopolistic competition theory as espoused by Robinson and Chamberlin. (Many of his targets combine these, of course.)²¹ What Friedman objected to was an insistence on psychological realism.

But rather than arguing the case here, I want to call attention to features of Friedman's methodology that are tangential to the realism-instrumentalism debate and that tend to get ignored if we focus on it.

First, I quote again from Friedman's referee report to the Rockefeller foundation about the Cowles' commission: "I have no confidence in [Koopman's] judgment about realistic economic problems or about techniques for attaining sound knowledge of economic processes" (Friedman, May 26 1948). It is clear that for Friedman economic theory should allow the theorist to get a grip on significant economic phenomena. Stigler writes Friedman: "I am coming to believe that you are more consistently abstract and a priori-ish than I. But it's cloaked over by your emphasis on realism," (19 August, 1946).²² As should be clear, Friedman was known for emphasizing the application of economic (price) theory to real problems. Stigler, too, argued this in a letter to Chamberlin: "as if my task is to do justice to theories instead of to reality... I am prepared to

²⁰ A competing approach, defended by Mäki (1986), is that Friedman is a confused, bad philosopher. More recently (2009) he has come around to the realist interpretation of F1953.

²¹ For a more rounded analysis of Friedman's immediate targets, see Hammond (2008) and Backhouse (2009). Backhouse draws on Friedman's correspondence with Patenkin; it together with the unpublished drafts of F1953 show that many characteristic views of Friedman were articulated from 1947 onward.

²² In responding to Patenkin, Friedman found Stigler (1947) not sufficiently concerned with empirical facts. (See Backhouse's op cit. treatment of the correspondence and of Stigler 1947.)

argue (1) that your theory is indeterminate, and (2) that it not useful (often) in realistic analysis. I do not recall a single consistent application of it to a real problem, and this is the ultimate failure of a theory” (GS to Chamberlin, 8/1947).

Thus, Friedman and Stigler are against general abstract theorizing and formalizing that is too far removed from concrete (real) economic problems. This they associated with the Walrasian (general equilibrium) program. What they denied, in particular, was that one could correct the Walrasian program in the direction of more realism by giving it more realistic psychological assumptions. Stigler is very lucid on this in his criticism of Triffin’s reformulation and extension of Chamberlin: “Dr. Triffin’s failure...seems to me attributable to his attempt to make the general theory an accurate description of all reality...Dr. Triffin should have been warned by the Walrasian theory of general equilibrium he sought to generalize....

“The theory [Chamberlin-Triffin] has nothing to learn from the study of specific problems because these problems are so diverse that no single inductive generalization is possible. Conversely the study of specific problems has nothing to gain from the general theory, for the theory can provide no apparatus to raise relevant questions, to indicate relevant type of facts, or to guide the economist in handling the facts to reach useful conclusions...theory is studied only as an aid in solving real problems, and it is good only in the measure that it performs this function” (Stigler 1949: 22).

That is to say, ‘Chicago’ rejects a correspondence account of economic theories. The language of Chicago’s anti-realism is really directed at a form of naïve descriptivism, that is, the account of theory in which it is understood as giving a kind of photographic reproduction of reality. As Stigler writes, (explicitly crediting both Milton Friedman and Talcott Parsons’s *The Structure of Social Action* in an accompanying footnote): “description...is a most unreasonable burden to place upon a theory: the role of description is to particularize, while the role of theory is to generalize---to disregard an infinite number of differences and capture the important common element in different phenomena” (Stigler 1949: 23).²³

Hence, Chicago is not against theoretical generalization, as long as it can help analyze and solve known economic problems. ‘Chicago’ is also not against theory building. In fact, Friedman and Stigler often adopt the Marshallian language of theory as a research-engine. Friedman puts it succinctly in his criticism of Walras: “[Walras’] problem is the problem of form, not of content: of displaying idealized picture of the economic system, not of constructing an engine for analyzing concrete problems,” (Friedman 1955: 904).

So, the issue is, as Stigler points out to Friedman: “It surely is possible to say something about assumptions being more promising than others,” (9/1948). But the development of even promising theory takes long hard work. By this Stigler and Friedman have in mind careful

²³ Stigler’s writings in the 1930s and 1940s are full of references to Parsons, and even F1953 echoes examples and images from Parson’s *The Structure of Social Action*. (Schliesser Forthcoming.) There is no evidence that Friedman was familiar with Parsons or Weber beyond what he read in Stigler and, perhaps, picked up from Frank Knight. But thanks to an interview that Daniel Hammond conducted with Rose and Milton Friedman (1989) we do know that Friedman was very interested in Pareto, where he would have encountered very similar ideas.

“quantitative” studies of “component segments of the economy, on a reasonably exact and comprehensive knowledge of the phenomena generalized.” I turn to Nutter’s 1951 book for a closer view of this method in action.

3: Nutter’s *The Extent of Enterprise Monopoly in the United States, 1899-1939*.

Nutter’s 1951 book has three short chapters and a fourth with brief “summary and qualifications.” It has also two longer appendices with data and graphs. The title page of the 1951 version offers a sub-title: “A quantitative Study of Some Aspects of Monopoly.” The modest side of this (“some”) fits with Chicago’s self-conception in the 1940s that general theory will be built up out of focused inquiries. The “quantitative” element fits with the Chicago methodological stance of the 1940s, privileging data-driven inquiry over highly theoretical modeling.

On p. 4 Nutter claims that the “primary purpose of the present inquiry is to study monopoly as it may be somehow related to economies of scale.” This vague phrasing does not do justice to Nutter’s aims. It is more appropriate to claim that Nutter conceives of his study as test for the then popular “hypothesis that monopoly is automatically generated in a private-enterprise system” (36). Nutter does not mention the ‘test’ early in his book, but it is hinted at from the first page. According to Nutter this hypothesis was stated “most uncompromisingly” by Arthur R. Burns in “his book, *The Decline of Competition*” of 1936 (1).²⁴

It is not clear to me that Nutter offers a genuine test of the “hypothesis that monopoly is automatically generated in a private-enterprise system” (36) if we conceive of the hypothesis as a *ceterus paribus* claim. For, Nutter offers several arguments to the effect that that increases in (overseas) markets and (technological) innovations may have worked against the growth of monopoly (29). So even by his lights *ceterus* is not *paribus*. Of course, the negative result can be taken to mean that the neo-classical (Marshallian) apparatus need not be given up, and can form the basis of future theoretical understanding.

Incidentally, Nutter draws two opposite poles with on the one pole Burns and at the other pole Simons and Hayek (1-2).²⁵ Rather than seeing institutionalism as non-economics beyond the disciplinary boundaries, Nutter understands it as one of the centers of the discipline—belying Reder’s later judgment and reinforcing my claim that within Chicago in the 40s and early 50s institutionalism was a rival to be taken seriously.

Yet, from the evidence that Nutter provides Burns is committed to a weaker hypothesis: “the very competition that induces the most economical utilization of the means of production has

²⁴ There were two Arthur Burns. Both were on the Columbia faculty. Arthur R. Burns wrote *The Decline of Competition*. Arthur F. Burns was Friedman’s mentor and Chairman of the Board of Governors. I thank Dan Hammond for disambiguating these for me.

²⁵ The mention of Hayek suggests that at least some young, ambitious PhD students in economics were still willing to include him within the field even after he had become a public intellectual.

induced the survival of firms so large and so few that perfect competition itself no longer survives in a number of industries” (quoted on p. 1 of Nutter). Burns does not state that competition always induces monopoly; not to mention that there is considerable daylight between the demise of perfect competition and monopoly. But let’s leave that aside. Here I focus on Nutter’s self-understanding of his methodology.

My aim is to analyze it so we can characterize Chicago economics in action. I focus on three important features: first on Nutter’s use of proxies as a way to handle limited data in complex environments; second on Nutter’s self-limitation about what can be investigated through economic analysis; third the practical response to the skeptical threat posed by the Duhem-Quine problem shared by Nutter and Friedman. As will be clear in what follows all three features have non-trivial political consequences.

3A: From Defeatism to the use of Proxies: on Indirect Evidence

While Nutter’s book reads as a test of a claim about the development of monopoly by private enterprise, the concrete aim of Nutter’s study is “to develop a meaningful quantitative index of the extent and growth of monopoly” (Nutter 1951: 4). Nutter is, thus, not in the business of developing new theory or offering an empirical study in order to improve theory. Rather he designs a theoretically informed measure to track a politically important economic institution. There is, in fact, in Nutter’s book no direct theoretical pay-off, except in so far as he undermines the claim that increasing returns automatically lead to monopoly.

Nutter’s data driven approach is designed to create an index that can help one track changes over time in the nature of monopoly within an industry and the economy as a whole. It is, thus, as much a contribution to economic history and economic theory (see Fabricant 1953: 94). This fits the general character of NBER inspired work done by Friedman throughout his life.²⁶

Nutter is explicitly reacting to a certain kind of defeatism about the very possibility of empirical measures of competition and monopoly. He quotes Clair Wilcox as a “representative of that attitude: “No sort of an estimate concerning the comparative extent of competition and monopoly in American markets is justified by the available evidence. Such an estimate must wait upon the articulation of usable definitions, the development of techniques of measurement, and the collection of a body of data much larger than anything that is now at hand. Indeed, it may be doubted if such an estimate can ever be made with any assurance”” (Nutter [quoting Wilcox] 1951: 3). No doubt it is claims like this that Friedman and Stigler have in mind when they

²⁶ “[T]he Chicago School economists have traditionally worked with long-period analysis,” (Dow and Abernathy : 240). Dow and Abernathy also focus on methodology and read Stigler 1949, F1953, and Nutter 1951s in light of each other and call attention to many methodological parallels among them. (I only learned of their work as I was putting finishing touches on this essay.) Unfortunately, they show no interest in analyzing the way in which ‘Chicago’ handles data. Rather, they focus critically on the (lack of) rules in model selection in the analysis of data at Chicago. Here I am agnostic about their claim.

consider the “immature” aspects of economics. Yet, Nutter’s book is meant to contribute to the process of evolving economics from an immature and mature science.

In response to Wilcox, Nutter concedes that “some arbitrariness is involved in all definition” (Nutter 1951: 4). But in argument later made familiar by Friedman, he continues: “The primary objectives of a particular inquiry will dictate the way in which [definitional] lines will be drawn.” That is, the “meaning of [definition] chosen will depend primarily on the particular set of social problems that the investigator is interested in. Recall that for Friedman a theory has several components: a) a tautological “language,” which serves as a “filing system for organizing empirical material and facilitating our understanding of it”; this filing system comes with b) “criteria by which it is to be judged...appropriate to a filing system. Are the categories clearly and precisely defined? Are they exhaustive? [etc]”; and c) a “body of substantive hypotheses designed to abstract essential features of complex reality” (F1953, 7). The main question for Friedman to be asked about these distinctions is: do the “categories of the ‘analytical filing system’ have a meaningful empirical counterpart, that is, whether they are *useful* in analyzing a particular class of concrete problems” (F1953, 7, my emphasis).

With regard to defeatism, Nutter’s strategy is to accept that “accurate measurement of long-run elasticity of demand is impossible” (Nutter 1951: 7).²⁷ When faced with complex or “limited data and serious conceptual difficulties” (8) many scientists create models. This has been studied quite intensely recently by historians and philosophers of science (see, e.g., Boumans 2005). Yet, Nutter’s work (and also Friedman’s work on *Monetary History*) reminds us of alternative strategies. Instead of modeling, Nutter relies on what he calls “indirect evidence” (8).²⁸ The technical term for this (among philosophers) is ‘proxy’ evidence. The use of proxies has been less studied in the philosophy of science.

In practice, Nutter will derive indirect evidence from the study of “the structure of industries—in terms of number of firms, concentration of output, and so on” (8). Of course, Nutter realizes that this must involve considerable “practical judgment” (8; Nutter puts scare-quotes around this phrase. Friedman also calls attention to the importance of the practitioner’s judgment in F1953, (25).) Besides the use of good judgment, the employment of indirect evidence is constrained if other “types of evidence strongly contradict the structural evidence” (8). So, the use of proxies involves considerable evidential risks.

Interestingly, Stigler had confronted the same problem in his fifth LSE lecture, “Competition in the United States.” Stigler is forthright about the problem, too: “the dividing line between competition and monopoly is not only subjective, which is to be expected, but also has a drastic effect on the proportion of industries classified as monopolies” (47).

²⁷ “Demand elasticity provides an asymmetrical test [of monopoly and collusion]. A high elasticity, in the absence of effective collusion, is a sufficient condition for competitive behavior; a low elasticity is a necessary, but not a sufficient, condition for competitive behavior” (Nutter 1951: 7).

²⁸ Another work coming out of the Free Market Study, Weston, follows the same evidential strategy. It cites the work of the Federal Trade Commission and Stigler 1950 (Weston 1953: 7).

In fact, Nutter’s fiercest critic, Stanley Lebergott, exploited this fact. On Lebergott’s analysis, if we change the classification of only a few industries, Nutter’s results are reversed: Suppose, however, that we define the industry consistently - either as monopolized or "potentially 'workably competitive'" at both dates. The result is spectacular: monopoly now increases clearly and substantially (Lebergott 1953: 350). This drew a rebuttal by Nutter: “Although estimates of concentration at the turn of the century are subject to serious question to say the least, there is no reason to presume that the net bias is toward overstatement. For each case of alleged overstatement cited by Mr. Lebergott, there may be an equivalent understatement,” (Nutter 1953: 352).²⁹

Of course, the use of proxies means that one is not constructing “an economic measure that conforms in all important respects with the best measure that can be conceived. We must usually be satisfied with a measure that is the best of those that can be constructed, being careful at the same time to interpret the measure with caution,” (Nutter 1951: 10; see also his “extreme caution” at Nutter 1951: 47). This is why when Nutter sums up his own conclusions, he writes these “are all subject to serious qualification which result from probable inaccuracies in measurement and from the arbitrary nature of definitions of monopoly” (Nutter 1951: 46). But even Lebergott, then at work in the Bureau of the Budget, admits that “the basic monograph is a very detailed factual study” (Lebergott 1953: 349); while accusing Nutter of introducing “technical bias,” he notes “the great care and detail in Dr. Nutter's estimates,” (351).

In the most serious sustained, critical engagement with Nutter’s book that I am familiar with, Salomon Fabricant highlights two important features of Nutter’s strategy. “First, it is clear, therefore, that Nutter is forced to lean heavily on concentration as the revealing characteristic, simply because census tabulations make it available for all industries.” (Fabricant 1953: 91) That is to say, Nutter’s particular proxy strategy is (sensibly) informed by the availability of data.

Second, Fabricant brings out how prone to varying levels of arbitrary judgment Nutter’s index of structure is. He does so by cleverly comparing Nutter’s results with Stigler’s fifth LSE lecture: “Competition in the United States.” He introduces a table in which he compares their judgments on the Relative Extent of Monopoly in 1939. (The numbers reflect income originating in monopolized industries as a percentage of income originating in the entire group). This is Fabricant’s table:

Group	Nutter	Stigler
Contract construction	100	0
Communication	100	100
Public utilities	100	100
Manufacturing	40	40
Mining	26	57
Finance	22	21
Agriculture	11	6

²⁹ Long after the Nutter-Lebergott exchange, one of Nutter’s main targets, Gardiner Means, called attention to Lebergott’s criticism of Nutter (Means 1970).

Trade	9	9
Services	5	12
Transportation	3	96

With a hint of mischief Fabricant comments on the table as follows:

“Construction is treated by Nutter as monopolized because of "evidence of output concentration in many local markets" (p. i9); while Stigler treats it as competitive, with a note that "this is a classification of firms, not labor markets" (p. 58, n. d). To Nutter the railroads were competitive in 1939, "with the exception of facilities to some comparatively isolated communities" (p. 20); Stigler places them all in the monopoly group. Stigler treats soft-coal mining as monopolistic (because it operated under a compulsory cartel set up by Federal legislation), while Nutter does not. On the other hand, both Nutter and Stigler recognize dairy farming as a compulsory cartel and therefore monopolistic in nature; but Nutter over-weights it greatly by using gross value added in milk production as a measure of income originating, while Stigler is more careful. Stigler read *Incomes from Independent Professional Practice*, by Friedman and Kuznets, which led him to classify medicine as monopolistic; Nutter apparently did not-or got contrary advice from Friedman, under whom he studied,” (Fabricant 1953: 92).

Despite these criticisms, Fabricant is very supportive of Nutter’s and Stigler’s work: “Yet, whatever the outcome, the essential validity of their conclusion must stand. All the doubts that can be raised do not destroy, rather they support, the conclusion that there is no basis for believing that the economy of the United States is largely monopolistic and has been growing more monopolistic,” (Fabricant 1953: 93).

3B: Economic vs Political Monopoly

In this chapter I emphasize the methodological similarity between Nutter and F1953. However, I do not wish to overstate this. For example, in his book Nutter distinguishes the “political” and “economic” aspects of monopoly. “In a political or power sense, monopoly may refer to situations which provide a concentration of privileges or advantages in making and enforcing the effective rules of society. In an economic or market sense, monopoly refers to situations in the market that, within the existing effective framework of rules, lead to a particular pricing process” (Nutter 1951: 4). Nutter is explicit that his study “will be limited to market monopoly” (1951: 5). It appears that Nutter is relying on some kind of positive/normative distinction of the sort defended by Friedman in his F1953 and derived from the writings of Neville Keynes.³⁰ Yet, when we read Nutter in context it is by no means obvious that his distinction can be grafted onto Friedman’s. This becomes clear when we focus on the response to Nutter’s research.

The application of this distinction between political and economics aspects of monopoly drew serious fire from A.A. Berle Jr., who in 1932 had published *The Modern Corporation and*

³⁰ N. Keynes was very popular at Chicago, but a study of his influence must await another occasion.

Private Property, co-authored with Gardiner Means (who is one of Nutter's main targets)³¹ in a comment on a 1951 piece by Morris Adelman that drew heavily on Nutter's 1949 dissertation. (Nutter thought Adelman's piece vindicated his approach; see Nutter 1956; reprinted in Nutter 1983, note 8.) Berle argued as follows:

“Depending on the definition of monopoly, Professor Nutter considered that 11.0 per cent of national income (on a rigid definition) or 19.3 per cent (under a broader definition) of national income originated in monopolistic industries in the year 1937; on the Department of Commerce income data the fraction of national income originating in monopolistic industries would be 20.6 per cent in that year, and 20.4 per cent in 1939. Again measured by the national income data, Nutter concluded that in 1899 17.4 per cent of all American national income originated in monopolistic industries, while in 1937 11.0 per cent of the national income originated in such industries. Certain students have accordingly enjoyed the hope that maybe the problem did not seriously exist; certainly it did not need to engage governmental attention. This conclusion, particularly the latter part of it, is very pleasing in some quarters.

But at this point, it would appear, the legal student and the political scientist had best go to work. For the question is not merely one of economic measurement. It is also one of net sociological and political effect. The concentration of 45 per cent of all manufacturing assets in the hands of 139 companies (Adelman's conclusion), in a country the size of the United States, represents a vivid concentration of economic power by any standards. (American manufactures are, roughly, half the manufactures of the world” (in Edwards and Edelman 1952: 172).

From the passage quoted it might seem that Berle is disagreeing with Nutter over some normative issue. After all, Berle decides not to attack Nutter on his factual conclusions. All he appears to be doing is giving different weight to a different set of facts (as found in “Adelman's conclusion”). But this misreads Berle seriously; Berle is contrasting Nutter's scientific credentials negatively with Adelman's. This becomes clear from a passage just before this long quote. “The largest 139 manufacturing corporations,” Berle writes, “in 1947 held 45 per cent of the assets of all American manufacturing corporations—a large absolute increase in asset value, but proportionally a slight decline since 1931. *This is a realistic, solid, and scientific* appreciation of the problem as Adelman defines it. Thus, it is an improvement over a similar study by Dr. G. Warren Nutter, of Yale, stimulated by the University of Chicago Law School” (Edwards and Edelman 1952: 172; emphasis added).

Thus, while Berle is not shy about calling attention to the roots of Nutter's work in the Free Market Study and hints (“some quarters”) at the political agenda associated with it (something Adelman passes over in his piece for *Fortune*), Berle conceives his debate with Nutter as a debate within “positive” science. (After all “political science” and “law” are by his light also sciences! [No doubt there is a German notion of '*wissenschaft*' lurking in the background here.]) It's only once the institutionalist background is written out of history of economics that Berle's

³¹ On Berle and Means, see Stapleford this volume.

comment appears as a claim about normative or policy aspects of economics. In context Nutter's distinction is a move *within* positive economics one that is challenged on scientific grounds. I have been unable to find a direct response to Berle by Nutter.

Regardless of the substance of Berle's criticism, the spirit of his remarks alerts us to the fact Nutter's economic vs political monopoly distinction need not map on to a positive vs normative distinction. In fact, there is no reason to believe that Nutter thinks that political monopoly is not a subject appropriate to economics. It is just that he thinks the quantitative tools and indirect evidence available to him in his particular study cannot shed much light on it. After all, Nutter also claims that "in an economic or market sense, monopoly refers to situations in the market that, within the existing effective framework of rules, lead to a particular pricing process." This emphasis on the effective framework of rules, that is the 'political' dimension of monopoly, is closer to Knight's Weberian approach (which has an important after-life in Virginia political economy) than Friedman's so-called positivism. But Nutter's and Buchanan's links to 'Chicago' of the 30s and 40s must be told elsewhere.³²

It is possible that Nutter's deliberate self-limitation was driven by political motives that lurked behind the research and its outcomes as Berle implies. Anticipating Mirowski and his school, Berle clearly thinks that Nutter's association with Director's Free Market Study gives away the game. While one cannot rule out this reductive interpretation of Nutter's approach (and everything I say is compatible with it), it is not the one I pursue here. One can also understand Nutter's strategy in more legitimate, scientific fashion.

3C: Duhem-Quine thesis; Nutter's Realism

Nutter is leery of applying the theory of "perfect competition" without qualification. As he writes in a passage that foreshadows Friedman's famous strictures: "It is particularly misleading to judge productive conditions on the basis of the presence of absence of the basic formal assumptions of perfect competition. The theory of perfect competition, like all theories, is an artificial system, constructed for the purpose of analysis and prediction. As, such the simplest possible set of assumptions has been chosen that is consistent with the processes and results of that system...The purpose of simplification of assumptions is to facilitate analysis, not closely to approximate actual conditions," (Nutter 1951: 6).

In context, Nutter is justifying his use of a category 'workable competition.'³³ This notion was labeled, "Pickwickian phraseology," by Nutter's critic, Lebergott (1953: 350). But rather than investigate how Nutter attempts to operationalize this imprecise notion,³⁴ I focus on one more methodological similarity between Friedman and Nutter. For example, in the quote from page 6, I omitted Nutter's claim that "It is always possible to construct alternative sets of assumptions,

³² Besides Brady 2007, see Medema and McCloskey in this volume on the links between Virginia and good old Chicago.

³³ Somewhat surprisingly, Nutter does not mention Clark 1940, cited in Stigler 1949, 48-9.

³⁴ In light of Backhouse (2009) of Friedman and debates over monopoly, this question merits further clarification.

with any desired degree of complexity, equally consistent with processes and results” (Nutter 1951: 6). This is a version of what is known among philosophers as the Duhem-Quine thesis. Friedman has a very nice statement of it: “If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are.”* (F1953: 9); *See also “Lange on Price Flexibility,” 282-3.) As my earlier quotes from Stigler and Friedman indicate (section 1D), versions of the Duhem-Quine thesis were repeated regularly around ‘Chicago.’ As the context of Nutter’s employment of the Duhem-Quine thesis indicates, it is meant to defend theoretically informed simplifications (or abstractions) to further the application of a favored theory to ‘real’ problems and simultaneously to prevent appeals to the realism of assumptions of competing theories to undermine in advance ongoing research. This is an unappreciated fact of Chicago methodology (see Schliesser 2005).

4: Coda by way of Conclusion

In the conclusion of his study of Nutter’s and Stigler’s findings, Salomon Fabricant writes: “The start made by Nutter and Stigler is only the first in a series of approximations, as I said at the outset. Yet even when completed, the results could provide only a clue to the really significant measures of level and trend of monopoly. Nutter and Stigler recognize that monopoly is significant, and its importance is truly measured only by its effects. When we are concerned with the efficiency of the economy, what we want to ask is: By how much would real national income per capita be raised if monopoly were somehow eliminated-after deducting the costs of eliminating it? It is this question at which Nutter and Stigler are really aiming. But it is not the only question. If we are concerned with the rate of economic progress, what we want to ask is: By how much would the rate of increase of real income per capita be stepped up if monopoly were eliminated?” (Fabricant 1953: 93)

One can understand Harberger’s much cited and influential “Monopoly and Resource Allocation” (1954) as an attempt to answer these questions. Harberger estimated “some quantitative notion of the allocative and welfare effects of monopoly” (Harberger 1954: 77) and concluded that the “welfare cost of monopoly in present values to \$1.50 per capita, but not significantly higher” (Harberger 1954: 86). There is a sense in which Harberger’s results cohere with Stigler’s and Nutter’s. They were often cited together by critics and friends alike (Dow and Abernathy 1963; Arrow 1977: 389; Weintraub (1955, (although Weintraub distinguishes among them, too); Reder 1982: 16). But at the same time Harberger’s estimate deflates the whole issue. As Harberger observes, “it seems to me that our literature of the last twenty or so years reflects a general belief that monopoly distortions to our resources structure are much greater than they seem in fact to be,” (Harberger 1954: 86). This has obvious political implications (Dow and Abernathy: 236).

Moreover, with its aggressive application of Hotelling’s formal work (including adoption of diagrams that focus on triangles that represent welfare loss) and its avoidance of the nitty gritty

empirical details of the component aspects of the economy, Harberger's paper was also a significant step away from the Knightian and NBER roots at Chicago.³⁵ Instead of careful empirical industry-specific research, Harberger turned the question of monopoly at Chicago into an issue within welfare economics; his upshot is that "we can neglect monopoly elements and still gain a very good understanding of how our economic process works and how our resources are allocated," (Harberger 1954: 87). One is now allowed to assume, in Nutter's words, that "competition is the normal condition in our economy," (Nutter 1954: 76).³⁶ This heralds a new (as-if) working assumption that influences all subsequent Chicago research (Reder 1982).

So far I have emphasized the methodological discontinuity between Harberger's article and the work done by Nutter, Stigler, and Friedman in this period. We should not forget that Stigler had started his career as a critic of the new welfare economics (Stigler 1943; see Levy & Peart 2008 and Schliesser Forthcoming). In fact, Stigler is quite critical of Harberger's 1954 estimate, concluding his discussion of Harberger sarcastically: "No one knows the amount of welfare loss that would be found if all the appropriate modifications could be carried through. Perhaps it would come to only \$2,000,000 a year for every economist. Whatever it may be, we may still properly devote much attention to monopoly," (Stigler 1956: 35). By the time of Reder's retrospective this has been long forgotten and is Chicago understood in terms of Pareto optimality (Reder 1982: 11ff).

Of course, once Chicago accepts the new welfare economics it opens the door to social engineering (for a careful account of Chicago's acceptance of the new welfare economics, see Banzhaf forthcoming; Harberger 1954 plays a crucial role in his narrative). This requires further argument, of course, but it is surely no coincidence that Harberger was the main educator of the Chilean "Chicago Boys." But that story must be told elsewhere (Schliesser in press).

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³⁵ Cf. Reder 1982: 12, who treats Harberger as a problematic case in his story.

³⁶ Nutter tacitly relies on his and Stigler's earlier results. Nutter and Harberger do not cross reference each other. Nutter's piece cites no other author. One wonders what audience members at the annual meeting of the AEA and readers of the *proceedings* must have been thinking in seeing such diverging approaches lead to the same conclusion.

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