More Fiber than Thread? Evidence on the Mirowski-Hands Yarn

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Introduction

Hands and Mirowski assert quite a lot in their pair of papers on Henry Schultz, Harold Hotelling and the stabilization of neoclassical price theory (1998a, 1998b). The papers are wonderfully provocative, in style and substance, and chock full of historical matter and interpretation. My concern is with but one of the three strands in what Hands and Mirowski call the "skein" of postwar neoclassical price theory. The three strands are the MIT revealed preference doctrine, Chicago Marshallian price theory, and the Cowles Commission general equilibrium program. My concern is with the Chicago Marshallian strand. My thesis is that despite their concern to provide a richer, less linear account of the twentieth-century American development of price theory than is found in other histories, the Chicago strand of their history is drawn more tightly and straighter than the historical evidence actually allow. In the following section I will sketch their account of the Chicago strand, including a number of quotations from the Hands-Mirowski papers. Then I will bring both evidence and the absence of evidence to bear on their interpretation.

According to Hands and Mirowski the material for all three strands originated in the collaborative relationship between Henry Schultz and Harold Hotelling from 1932 until May 1935. Schultz, who embarked several years earlier on an attempt to ground

empirical demand functions in theory from the mathematical school of Walras and Pareto, was the *Journal of Political Economy* referee for Hotelling's "Edgeworth's Taxation Paradox and the Nature of Demand and Supply Functions" (1932). Viewing both the phenomenological approach of his mentor Henry Moore and the British Marshallian partial equilibrium approach as limited, Schultz sought to find the laws behind the Law of Demand. Aside from assisting Holbrook Working on estimations of demand and supply of agricultural commodities, Hotelling had little background in demand analysis before writing the 1932 paper on the Edgeworth taxation paradox. When Schultz read this paper for the JPE there commenced a series of letters and visits in which he and Hotelling grappled with the theoretical restrictions on sets of interrelated demand functions, specifically the cross-equation or integrability conditions.[1] This was an important matter, for in Hotelling's words, "the difference of two symmetrically placed coefficients could be taken as a measure of the degree of inconsistency in buyer's [sic] judgments, or of the rigidity of an absolute limit on their money expenditures" (1932, p. 598) [354 in Hands & Mirowski]. Hotelling believed that his price potential model, where the equations' derivatives were quantities of the commodities, had empirical relevance. Among other implications this allows measurement of consumer surplus for welfare analysis of public policies.

Schultz came to their collaboration with a preference for what became the conventional model of constrained utility maximization. He pointed out that if the marginal utility of money is independent of prices Hotelling's integrability conditions are the same as the symmetry conditions derived from a model of constrained utility maximization. When Schultz tested demand equations empirically for the cross-equations

[1] $\delta p_i/\delta x_j = \delta p_j/\delta x_i$ for all $i \neq j$ or $\delta x_i/\delta p_j = \delta x_j/\delta p_i$ for all $i \neq j$ (Hands and Mirowski, 1998a, p. 330).

restrictions, he found that they failed to hold. Herein lay the crux of the Schultz-Hotelling problem from which Hands and Mirowski suggest the three strands of neoclassical price theory arose. Schultz, with Hotelling in the wings, put the general equilibrium vision of rational agents whose actions are coordinated within a system of markets to a clear-cut empirical test, and it failed.

Of the two approaches, constrained utility maximization, which was favored by Schultz, and the price potential model favored by Hotelling, the former became the standard. Hands and Mirowski characterize Hotelling's price potential model is a hopeful failure; they suggest that it could have resolved some of the difficulties troubling neoclassical price theorists through the 1930s and beyond. Yet they stress that this claim is secondary to their main point, which is that the impasse reached by Schultz and Hotelling was the crucible from which three dominant strands of post-war neoclassical price theory emerged. To make the argument that all would have been well if only the price potential model had not been cast aside would place Hands and Mirowski in an uncomfortable position given their historiographical preferences. They believe that it is not the role of the historian to champion one theory over others. Their purpose is rather to "tell a symmetrical story of rejected knowledge … and bursting asunder all those neatly stacked boxes on the storage shelf of economic knowledge" (1998a, p. 326). My purpose in this paper is to test one of their own neatly stacked boxes to see if it withstands the weight of evidence.

Hands & Mirowski's Neoclassical Dream

In "Harold Hotelling and the Neoclassical Dream," Hands and Mirowski begin weaving the Chicago Marshallian strand of neoclassical price theory on page 359, where Milton

Friedman first appears. The final known letter between Hotelling and Schultz was written by Hotelling May 17, 1935. In 1933 Schultz had published "Interrelations of Demand," which Hands and Mirowski portray as his version of the joint effort with Hotelling to reconcile theory and evidence on demands for substitutes and complements. Schultz discussed the price potential model favored by Hotelling, but relied more heavily on constrained utility maximization. The empirical results were disappointing, leaving Schultz to ponder which of several auxiliary hypotheses might save the theory from what on the surface was disconfirmation. Friedman was a student in Schultz's classes when the article appeared, taking Econ. 311, "Correlation and Curve Fitting," in the winter quarter 1933 and Econ. 312, "Statistical Graphics," the following, spring 1933, quarter. At the end of the academic year Schultz closed his statistical laboratory and departed Chicago for a year-long European tour, and Friedman, with Schultz's endorsement and a generous fellowship in hand, departed Chicago for Columbia University. In New York Friedman took Economics 117-188, "Mathematical Economics," and Economics 111-112, "Statistical Inference," from Hotelling in the autumn and spring semesters of the 1933-34 academic year. After a year at Columbia Friedman returned to the University of Chicago to complete course requirements for the doctorate and to work as research assistant in Schultz's newly reopened statistics laboratory.

On March 6, 1935, after Hotelling's update of his ideas on interrelated demands (1935) appeared in *Econometrica*, Friedman wrote to inform him of his and Schultz's discovery of the Slutsky paper on integrability conditions (1915). Hotelling had included both his price potential model and a model with constrained maximization in the article, which Hands and Mirowski portray as a relentlessly theoretical mess. During the year

that Friedman assisted Schultz he helped write "Interrerlations of Demand, Price, and Income" (1935), which put the Slutsky conditions into play empirically against the Hotelling integrability conditions. Neither set of cross-equations constraints was supported by the data, leaving Schultz in the position of making a theoretical plea for the Slutsky conditions as the more general of the two. Hands and Mirowski contend that this is the point at which the Hotelling economy was initially suppressed in neoclassical microeconomics. For his part, Hotelling did little new work on interrelated demands after 1935, and made no empirical estimations, yet he continued to insist his integrability conditions were theoretically fitting and empirically relevant.

At this juncture in their account, with Hotelling on the verge of ending his pursuits in price theory and Schultz approaching completion of what tragically turned out to be his final work (1938), Hands and Mirowski begin to weave the strand of Chicago Mashallian price theory. Two of Hotelling's final articles on price theory were a piece written for *Columbia Alumni News* (1936) and his Presidential Address to the Econometric Society (1938). In these, "directly related to his welfare economics and perhaps of overriding significance, he proceeded to make use of the interdependence of demands in neoclassical theory in order to prosecute his own political agenda and to propose his own analysis of the causes of the Great Depression" (Hands and Mirowski, 1998a, p. 364). Hands and Mirowski continue this theme:

> Perhaps it is no accident that at the point at which Hotelling decided to become more insistent about the political content of the theory he favoured, the pipeline to Chicago and the correspondence were both shut off. Hotelling continued to provide employment opportunities for Chicago

> products such as Milton Friedman and George Stigler during the war, in the Statistical Research Group at Columbia (Stigler, 1988, p. 61)[2], but the bloom seemed to be off the rose, and their later works such as Friedman and Wallis's (1942) paper on indifference curves and his paper on the income tax (1952) were intended as reprimands of Hotelling's program. … It is here that reactions to the Great Depression and general economic conditions have a looming presence in understanding subsequent attitudes and responses to the appropriate elaboration and development of neoclassical price theory. … And then there were those who wanted to repudiate all of the above because of their not-so-hidden socialist tendencies, to suppress the interactive income terms, and return to a 'Marshallian' mode of theorizing in the sense that Schultz had disparagingly used the term in 1928. This position, of course, is exemplified by Frank Knight, Milton Friedman and the Chicago School from the late 1940s onwards (1998a, p. 365-6).

Hands and Mirowski wind more of their Chicago School strand:

> As for the other member of our duo, there are some indications that Schultz was not held in high exteem by his colleagues towards the end of his tenure at Chicago. … Frank Knight's contempt for Schultz was openly expressed; and what is of greater moment for our narrative, there is some

[2] The reference here to George Stigler's memoir seems to suggest, in support of Hands and Mirowski's thesis, that Hotelling hired Friedman and Stigler. Stigler does not say so. According to W. Allen Wallis (1980), Hotelling recruited Wallis, who had at least as large a role as Hotelling in recruiting others to the staff. Stigler and Friedman were Columbia graduate school colleagues of Wallis, and Wallis and Friedman, but not Stigler, were students of Hotelling. Wallis, but not Hotelling, was fulltime at SRG.

> indication that Schultz's favoured student may have begun to turn on him (1998a, p. 369).

Here Hands and Mirowski quote from Melvin Reder's article on the Chicago School (1982) to the effect that Friedman and Stigler were not favourably impressed by Schultz, and suggest that their attitudes toward him were the source of slurs cast on Schultz's mathematical ability in the literature on Chicago School economics.

Hands and Mirowski see clues to the development of postwar Chicago price theory in the reactions to Schultz of Frank Knight, and two students, Friedman and Stigler, who Knight shared with Schultz.

> This rush to press an indictment of crippling mathematical deficiencies against a man who brought the issue of the scientific pretensions of neoclassical theory to a head would seem, at least to an historian, itself to require some serious explanation. While Schultz clearly treats Hotelling as the superior mathematician in their correspondence, Hotelling treats him in return with unhesitant respect as an equal (Hands and Mirowski, 1998a, p. 369).

After dealing with Schultz and Hotelling, Hands and Mirowski move forward in time to the three postwar schools of neoclassical price theory, which they set in the context of the Schultz-Hotelling impasse over cross equation restrictions and the budget constraint. Their claim is that the postwar Chicago approach, along with the Samuelson-MIT revealed preference approach and the Cowles Commission (Arrow-Debreu) general equilibrium approach, developed from the Schultz-Hotelling impasse. Chicago price theory is "the Chicago approach of Frank Knight and Milton Friedman" (1998a, p. 373).

On the table for explanation are "Milton Friedman's profound about-face with regard to the centrality of the Slutsky relations and the goal of empirical recovery of utility functions … [and] his promotion of a 'Marshallian' over a 'Walrasian' approach to neoclassical theory, as well as help in decoding his confusing essay on the methodology of positive economics" (1998a, p. 389, n. 44). Three texts exemplify postwar Chicago price theory, which Hands and Mirowski claim "outlined the explicit response to Schultz" (1998a, p. 377). These are Friedman and Allen Wallis's "The Empirical Derivation of Indifference Functions" (1942), Knight's "Realism and Relevance in the Theory of Demand" (1944), and Friedman's "The Marshallian Demand Curve" (1949).

The thesis is that Friedman's role in the Schultz-Hotelling collaboration and impasse is the key to his price theory and methodology. Historians have overlooked that "he began by writing up part of Schultz's *Theory and Measurement* book, spent some time with Hotelling, and then spent the next decade or more criticizing, repudiating and perhaps even obscuring their entire project" (1998a, p. 377). Hands and Mirowski find irony in Friedman's response to E. B. Wilson's request for a list of economics books that Friedman admires. Friedman commented on *Theory and Measurement of Demand*:

> Another book I am somewhat uncertain about is Henry Schultz, *The Theory and Measurement of Demand*. It is an exceedingly careful and systematic attempt to put empirical content into a pre-existing theory. I have excluded it because there seems to me no reverse influence of the empirical work on the theoretical structure. Schultz took the theory as fixed and given, and tried to measure what he thought were essential functions of the theory. He imposed extremely high standards of care and

> thoroughness in the measurement process – but he nowhere attempted what seems to me to be the fundamentally important task of reformulating the theory so it would really generalize the observable data; he always tried to wrench the data into a pre-existing theoretical scheme, no matter how much of a wrench was required (1998a, p. 377l, quoted from S. M. Stigler, 1994, p. 1200).

Hands and Mirowski suggest that outside the context of Friedman's association with Schultz and Hotelling this statement about Schultz's book appears epistemologically "puzzling, confused, inconsistent, or worse" (1998a, p. 377). But in this context one can see that:

> in effect, Friedman's reaction to Schultz's impasse was to first repudiate the attempt to extract laws of utility from price data (Friedman and Wallis, 1942); next, extricate price theory from any binding commitment to the Slutsky relations (Knight, 1944), and as the *pièce de rèsistance*, deny the practical importance of income effects for demand theory. Now, this definitely was not the same move as suppressing the independent budget constraint, as Hotelling had done. Rather, Friedman packaged his programme as returning to Marshall – but not the actual historical personage of Alfred Marshall – no, the return was to the "Marshallians" that Schultz had declared his sworn enemies (1998a, p. 377).

Hands and Mirowski treat Knight's article on realism and relevance in demand theory (1944) and Friedman's late 1940s and early 1950s articles, along with his price theory textbook (1962), as being of a piece:

> Knight and his students read what they considered to be misunderstandings about income to actually be surreptitious statements about the nature of money ….
>
> The upshot of all these objections became clearer through the subsequent work of Milton Friedman (1998a, p. 378).

Mirowski & Hands's Paradox

In their second paper, "A Paradox of Budgets," (1998b) Mirowski and Hands reiterate their thesis that the Schultz-Hotelling impasse was the burr in the saddles of primary players in three postwar American schools of neoclassical price theory. Those who developed each of the three programs resolved the problem of how to treat the income constraint and income effects in interdependent demand systems in their own way. Although they present postwar neoclassical price theory as a skein with three strands, Mirowski and Hands view the skein as forming a consensus approach, i.e., price theory stabilized. "We can now see the postwar neoclassical consensus – that to be a mathematical economist, one must start with Walras and Slutsky and nowhere else – as a localized (and American) cultural prejudice highly correlated with prior exposure to the Hotelling-Schultz dialogue of the 1930s" (1998b, p. 266).

As in their neoclassical dream paper, in "A Paradox of Budgets" Mirowski and Hands see cohesion around Frank Knight ("Knight's group") in the diversity of the prewar University of Chicago economics department.

> And finally there was the group revolving around Frank Knight, including Aaron Director, Henry Simons, and Lloyd Mints. Because it was predominantly Knight's students and supporters, including Friedman,

> Stigler, and Wallis, who subsequently constituted the core of the postwar Chicago school, greater continuity is often imputed to the school than actually existed. Nevertheless, some direct affiliations can be drawn from Knight to Friedman and Stigler, especially with regard to the issues that surround the Hotelling-Schultz impasse (1998b, p. 267).

Knight's place is important, linking Friedman, Stigler, and Wallis's prewar experiences with the Schultz-Hotelling impasse with their postwar development of Chicago price theory.

> It is not irrelevant to our story that by 1938 he [Lange] was prime defender of planning in the socialist calculation debate, an earlier interpreter of Keynesianism, and a Marxist. … Knight assumed proprietary rights over graduate price theory during the latter part of the war, but it was Lange who taught John Hicks's *Value and Capital*. …
>
> Despite Stigler's (1988, 148) claim that in 1947 'there was no Chicago School of Economics' … it is clear that Knight and his group were successful in advancing their agenda during the war, and by 1944 they were engaged in open intellectual warfare with the Cowles Commission (Reder 1982, 10; Hammond 1993, 231). Friedman's role as self-appointed tormentor of Cowles and acknowledged leader of the Economics Department was paramount, beginning with his return to Chicago in 1946; thereafter the Chicago school was consolidated with Wallis's appointment as dean of the Business School in 1955 and Stigler's appointment to the Walgreen professorship in 1958. Although none of these figures can be

> regarded as carbon copies of Knight, the relationship of Chicago to the stabilization of neoclassicism can be triangulated as a moving equilibrium between Knight and Schultz, Knight and Friedman, Friedman and Schultz, Friedman and Hotelling, and Friedman and Cowles (1998b, pp. 268-9).

Mirowski and Hands acknowledge that Knight and Friedman have many differences, yet claim that "one cannot make much headway in comprehending Chicago price theory without beginning with Knight" (1998b, p. 269). For it was Knight who during the war launched an attack on Hicks, Slutsky, and Schultz in "Realism and Relevance in the Theory of Demand" (1944). Knight's attack "was fairly subtle and foreshadowed much of the later Chicago tradition in price theory" (1998b, p. 269). He argued that the concern over matters such as the possibility of upward sloping demand curves was unnecessary. Properly framed in terms of income-compensated relative prices, demand curves would surely slope downward. In Knight's article Mirowski and Hands see:

> the rough-hewn blocks of the Chicago orthodoxy that would ultimately be sculpted by Friedman, Stigler, Gary Becker, and others. The approach began with an ontological commitment: "The demand curve … [is] undoubtedly the most solidly real of all the functional relations dealt with in economic theory." ([Knight] 310) … For Knight, the "compensating variations" that would successfully isolate income effects most likely could never be carried out in actual practice, much less inferred from an observed sequences of actual exchanges; an income effect was a virtual (and rather dangerous) notion. That is why Knight and his group of students had become convinced that Schultz had been barking up the

wrong tree and why Friedman could not countenance the Cowles Commission's quest for the Holy Grail of "structure."

> The most influential statement of Chicago demand theory, Friedman's 1949 article, "The Marshallian Demand Curve," proudly displays its intellectual heritage by insisting that its conclusion is "identical with that reached by Frank Knight in a recent article" ([Friedman] 135), namely, the 1944 article. (1998b, pp. 270-71).

Mirowski and Hands think they have found the key to Friedman's Marshallian demand article, a key that has eluded other historians.

> One need not search for Friedman's demand curve in Marshall, any more than one needs to find his philosophy in Karl Popper or John Dewey. Friedman's demand curve grows out of Knight, via Schultz and Hotelling. It was the culmination of a whole sequence of reviews and articles, most notably Friedman 1941, Friedman and Wallis 1942, and Friedman 1946, seeking a way out of the impasse bequeathed him by Schultz's palpable failure. These articles are the traces of a mighty struggle with the meaning and significance of income for neoclassical price theory, and it is no coincidence that the work for which Friedman is most famous, such as the "permanent income hypothesis" and the mantra that "money matters," derives directly from his empirical work on the measurement of professional incomes and the estimation of relevant demand functions in the late 1930s (Friedman and Kuznets 1945) (1998b, p. 272).

Mirowski and Hands enumerate points of Friedman's demand theory that coincide or diverge from Knight's. The most important similarities are treating the demand function as the primary entity in price theory and integrating monetary theory into demand theory. They differ in that "Friedman the statistician … inverts Knight the 'ideal type' philosopher: Good empirical demand curves underwrite dubious quasi psychology rather than vice versa (1998b, p. 272). That postwar Chicago lost all interest in rigorous theory is evidenced by Friedman ignoring Martin Bailey's (1954) critique of his aggregation methods and Stigler's failure to even mention the Slutsky equation in the first edition of *The Theory of Price* (1946). "Curiously enough, in retrospect one can still discern how this all began with Hotelling. What were needed were simple stories with clean empirical implications that could be retailed in concise phrases to clients of the economics profession. This, said Friedman, was the sine qua non of a useful model" (1998b, p. 273). Hotelling's price potential model would have served this purpose for Chicago price theory, according to Mirowski and Hands, but was rejected after the Schultz-Hotelling impasse. "But all through the blooming, buzzing confusion, [of the 1940s] Chicago never lost sight of its most immediate rival in demand theory: the Cowles Commission" (1998b, p. 274).

Threads That Aren't But Should Be

The Hands and Mirowski thesis that the evolution of price theory at Chicago was motivated by the Schultz-Hotelling impasse requires supporting evidence. Evidence should show links between the Schultz-Hotelling collaboration, in particular their failure to obtain empirical support for the integrability conditions, and the representative texts of post-war Chicago price theory. The possibility of finding these data is there. Milton

Friedman was present at the birth of the Schultz-Hotelling impasse, in the classrooms of both men and in Schultz's statistical laboratory. Depending on how much credit one gives to Friedman for Schultz's 1935 article and 1938 book, one might even change the label to the Schultz-Hotelling-Friedman impasse. Friedman and Stigler were both students of Frank Knight, who found fault with Schultz's attempt to ground the Law of Demand in more fundamental laws. Allen Wallis, like Friedman, began graduate work at Chicago and spent a year at Columbia, where he took courses from Hotelling [presumably?]. Friedman, Stigler, and Wallis all worked at the Statistical Research Group, where Hotelling was the principal investigator. Stigler wrote his Ph.D. thesis (1941) under Knight's supervision, and his *The Theory of Price* (1946) includes references to Schultz's *Theory and Measurement of Demand*, but does not discuss the integrability conditions. Friedman's Marshallian demand curve article (1949) bears a striking resemblance to Knight's realism and relevance article (1944) which preceded it by five years.

Yet more is required to establish the effects under question than Friedman and other postwar Chicago economists being on the premises in the 1930s. Historical data on individuals' actions, including what they wrote, and their personal relationships should give evidence of influence, not just proximity and similarities. In the remainder of this section I will outline some of the evidence that needs to be there but is missing.

Consider the passage quoted earlier from pp. 365-6 of "The Neoclassical Dream":

> Perhaps it is no accident that at the point at which Hotelling decided to become more insistent about the political content of the theory he favoured, the pipeline to Chicago and the correspondence were both shut

> off. Hotelling continued to provide employment opportunities for Chicago products such as Milton Friedman and George Stigler during the war, in the Statistical Research Group at Columbia (Stigler, 1988, p. 61), but the bloom seemed to be off the rose, and their later works such as Friedman and Wallis's (1942) paper on indifference curves and his paper on the income tax (1952) were intended as reprimands of Hotelling's program. … It is here that reactions to the Great Depression and general economic conditions have a looming presence in understanding subsequent attitudes and responses to the appropriate elaboration and development of neoclassical price theory. … And then there were those who wanted to repudiate all of the above because of their not-so-hidden socialist tendencies, to suppress the interactive income terms, and return to a 'Marshallian' mode of theorizing in the sense that Schultz had disparagingly used the term in 1928. This position, of course, is exemplified by Frank Knight, Milton Friedman and the Chicago School from the late 1940s onwards (1998a, p. 365-6).

In this passage we find the following conjectures:

1. Hotelling's connections with Chicago were severed because of the politics in his 1936 and 1938 articles.

2. The articles, Friedman and Wallis (1942) and Friedman (1952), were intended to be reprimands of Hotelling's program.

3. Knight, Friedman and the Chicago School rejected general equilibrium price theory in favour of Marshallian price theory because of the latter's socialist tendencies.

Moving through the remainder of the passages from "The Neoclassical Dream" that are quoted above we find the following claims:

4. Friedman "turned on" Henry Schultz.

5. Friedman and Stigler were unfair to Henry Schultz in criticism of his mathematical abilities, and in this there is more than meets the eye.

6. Friedman did an about face from being an advocate for and participant in the Schultz-Hotelling enterprise to being a staunch opponent, "criticizing, repudiating, and perhaps even obscuring their entire project."

7. Friedman's article on the Marshallian demand curve (1949), along with Friedman and Wallis (1942), and Knight (1944), was an explicit response to Schultz.

8. Friedman's comment to E. B. Wilson on Schultz's book does not make sense unless interpreted as part of an attempt to criticize, repudiate, and obscure Schultz's work.

9. Friedman packaged his approach to demand theory as a return to Marshall, but it was in fact a return to the Marshallians who Schultz had declared his sworn enemies, i.e., Friedman was an apostate.

10. Knight and his students (Friedman, Stigler, and Wallis) colluded, tacitly or explicitly, in their attacks on the Schultz-Hotelling program.

Mirowski and Hands repeat a number of these conjectures in their second paper and emphasize Knight's role as a sort of godfather of the postwar Chicago economists. Knight also provides a crucial temporal link between the younger cohort's experiences as Chicago students and their later experiences as established scholars. "One need not search for Friedman's demand curve in Marshall, any more than one needs to find his

philosophy in Karl Popper or John Dewey. Friedman's demand curve grows out of Knight, via Schultz and Hotelling" (1998b, p. 272).

How much evidence is there in support of these conjectures? The first place we will look is in the text of articles that Hands and Mirowski see as reactions to the Schultz-Hotelling impasse: Friedman and Wallis (1942), Knight (1944), Friedman (1949), and Friedman (1952). The most direct way to approach these is to minimize interpretation, looking at what the authors wrote or did not write and at their citations. Friedman and Wallis (1942) asked why it is that empirical workers do not make greater use of indifference curves. Their opening paragraph uses Schultz's book as an example of a "brilliant presentation of indifference curve analysis … that finds almost no occasion to employ it in the statistical portions of the book" (1942, p. 175). They argue that indifference curves are not put to greater use because of analytical problems inherent in the theory. There is a second reference to Schultz, for inspiring a psychological experiment with indifference curves, and none to Hotelling. They do refer to Stigler's "The Limitations of Statistical Demand Curves" (1939), a paper that itself contains a brief comment on the integrability conditions as one of the "few specific, and not very useful, theoretical criteria" from general equilibrium theory for empirical analysis. Stigler has several references to Schultz's book, along with other works in mathematical economics, but none to any of Hotelling's articles.

Knight (1944) criticized the "Hicks-Slutsky school," including Schultz, Oscar Lange, Jacob Mosak, and Paul Samuelson. His paper has no references to Hotelling, and Knight's criticism is aimed at the entire Hick-Slutsky school rather than particularly at Schultz. There is no reference to Wallis and Friedman (1942) which appeared in the same

book as the papers that Knight cites by Mosak and Samuelson. Friedman's article on the Marshallian demand curve (1949) contains one reference to Schultz (1938), two to Knight (1944), and none to Hotelling. Friedman thanks a number of people for criticism and suggestions, including "especially, Jacob Viner, to whose penetrating discussion of the demand curve in his course in economic theory I can trace some of the central ideas and even details of this article" (1949, p. 463). Knight is not among the individuals whom Friedman thanks. As references for the "current interpretation" of Marshall's demand curve, which Friedman seeks to overturn, he cites "followers of Marshall," including Edgeworth (1926), Hicks (1939), Stigler (1946), and Boulding (1948). We will return to the origins of Friedman's article later in this paper.

Friedman's article on the welfare effects of income and excise taxes (1952) grew out of his work on the Marshallian demand curve. He remarks at the beginning of the paper that although he is doubtful of the value of the "new" welfare economics, this is not his concern. Rather, the paper is intended to illustrate the implications of the two different approaches to economic analysis that he discussed in "The Marshallian Demand Curve," one being "arithmetical and descriptive" and the other "analytical and problem-solving." Among the citations he gives for the invalid proof of the superiority of an income tax over and excise tax are M. F. W. Joseph (1939), Hicks (1939), M. W. Reder (1942), and Stigler (1946) and Boulding (1948). With regard to Hotelling, Friedman says:

> The analysis of this problem by Joseph and Hicks is often considered identical with the earlier analysis of the same problem by Harold Hotelling in "The General welfare in Relation to Problems of Taxation and of Railway and Utility Rates," …. But this is a serious error, since Hotelling

> avoids the fallacy that mars the analyses listed in the preceding paragraph (1952, pp. 25-6, n 3).

All in all, the authors' direct references provide little support for the Hands-Mirowski thesis.

Threads That Are But Shouldn't Be

In this section I shift from evidence that would support the Hands-Mirowski thesis, but that is missing, to evidence that weakens their thesis. I begin with a sketch of Friedman's career from 1935 until his return to Chicago in 1946. Table 1 lists Friedman's employment from 1935 until he became a faculty member at the University of Chicago in 1946.

Table 1

Date	Employment
Aug. 1935 – Sept. 1937	Associate Economist, National Resources Committee
Sept. 1937 – July 1940	Member, Research Staff, NBER & Lecturer, Columbia University
1940 - 1941	Lecturer in Statistics with Rank of Professor, University of Wisconsin
1941 - 1943	Principal Economist, Division of Tax Research, U.S. Treasury & Professor, U.S. Department of Agriculture Graduate School
Mar. 1943 – Aug. 1945	Associate Director, Statistical Research Group
1945-46	Associate Professor of Economics & Statistics, University of Minnesota

Allen Wallis preceded Friedman in taking a summer job with the National Resources Committee. Upon his recommendation Hildegarde Kneeland offered Friedman a job in her group that was planning a study of consumer purchases. Friedman recalls the feeling of being a part of the New Deal scene: "There was a sense of excitement and achievement in the air. We had the feeling – or illusion – that we were in at the birth of a new order that would lead to major changes in society" (Friedman and Friedman, 1998, pp. 60-1). The purpose of the study was to produce data for use in setting weights in cost of living indices. Kneeland's group were responsible for design of survey questionnaires,

planning the survey schedule, sampling techniques, and data tabulation and analysis. Friedman "became completely immersed in the project. It was challenging, different from anything I had ever done, and greatly widened my perspective, both substantively and personally" (Friedman and Friedman, 1998, p. 62). The project was primarily statistical, and the biggest challenge was design of the sample.[3] This work on the consumption study for the New Deal NRC was the seed of Friedman's *A Theory of the Consumption Function* (1957), which he regards as his best piece of scientific work.

In early 1937 Friedman represented Kneeland's group at the National Bureau of Economic Research's Conference on Research in National Income and Wealth, which was organized the previous year by Simon Kuznets. His participation in this meeting on national income and its distribution, and a recommendation from Arthur Burns, led to Friedman's next job. In mid-1937 Friedman began his career-long association with the National Bureau of Economic Research.

Simon Kuznets was participating for the NBER in development of national income accounts. When Friedman went to work for him, he was given the task of completing estimates Kuznets had begun of the incomes of independent professionals: physicians, dentists, lawyers, accountants, and consulting engineers. The task was similar to that which he had done for the Kneeland group at the NRC in that the final product was estimates of income and its distribution. But in this project Friedman was further down the production line, analyzing survey data rather than designing surveys. The end

[3] Rose Director left Chicago for her home in Portland, Oregon in August 1935, and in April 1936 returned to Chicago to work as an assistant for Frank Knight and work on her dissertation under his direction. She too later went to Washington where she held jobs at the National Resources Committee, the Department of Agriculture's Bureau of Home Economics, and the Division of Research and Statistics in the Federal Deposit Insurance Corporation. In the latter position her boss was Homer Jones, a fellow student of Frank Knight.

product for Friedman was *Income from Independent Professional Practice* (1945), which served as his Columbia Ph.D. dissertation. In the course of explaining changes over time in the distribution of income in the professions, Friedman developed the concept of permanent income, which was to become so important in his and others' work in consumption and monetary economics.

While working in New York for the NBER Friedman taught in Columbia University's Extension. His fellow student and friend from Columbia, Eli Ginzberg, arranged this appointment. It was in this teaching experience that Friedman began to develop what came to be his version of Chicago price theory. In 1937 and 1938 he taught elementary economics. Also, beginning in the fall semester 1938 and continuing through spring 1940 he taught Economics ub-171 and ub-172, a course that carried graduate credit and with mostly graduate students. The course title was Structure of Neo-Classical Economics. Table 2 shows the reading list for this course, which matches closely reading lists for his Economics 300 at the University of Chicago in the late 1940s.[4] Not only do the readings match, but Friedman's lectures for his Columbia course also were the core of his price theory courses at Chicago and of *Price Theory: A Provisional Text* (1962).

[4] See (Hammond, 1999).

Table 2

Assignments in course given at Columbia by M. Friedman entitled

"Structure of Neo-classical Economics

(listed in order in which assigned)

First Semester

Author	**Title**	**Page Information**
Alfred Marshall	*Principles of Economics*	Book III, ch. 2,3,4; Book IV, ch. 1,2
Henry Schultz	*The Meaning of Statistical Demand Curves*	pp. 1-10
E.J. Working	"What do Statistical 'Demand Curves' Show?"	QJE Vol. XLI (1927), pp. 212-27
Frank H. Knight	*Risk, Uncertainty, and Profit*	Ch. 3
Frederic Benham	*Economics*	pp. 89-100

J.R. Hicks	*Value and Capital*	pp. 11-37
Marshall	*Principles of Economics*	Book V Ch. 3,4,5,12 Apendix H
A.L. Meyers	*Elements of Modern Economics*	Ch. 5,7,8,9
Joan Robinson	*Economics of Imperfect Competition*	Ch. 2
J.M. Clark	*The Economics of Overhead Cost*	Ch. 9
Jacob Viner	"Cost Curves and Supply Curves"	Bd. III (Sept., 1931) Zeitschrift fuer Nationaloekonomie pp. 23-46
Edward Chamberlin	*The Theory of Monopolistic Competition*	Ch. 3, sec. 1,4,5,6, Ch. 5
M. Abramovitz	"Monopolistic Selling in a Changing Economy"	Q.J.E., Feb., 1938 pp. 191-214

R.F. Harrod	*Doctrines of Imperfect Competition*	Q.J.E., May 1934 Sec. 1, pp. 442-61

Suggested readings for Mathematicians

Author	Title	Page Information
O. Lange	"On the Determinateness of the Utility Function"	Vol. I (1993-34) Review of Economic Studies pp. 218ff
R.G.D. Allen	**"**The Nature of Indifference Curves**"**	Vol. I (1993-34) Review of Economic Studies pp. 110ff

Suggested readings in Mathematics

Author	Title	Page Information
R.G.D. Allen	*Mathematical Analysis for Economists*	Ch. 2, pp. 28-30, 54-55 Ch. 5, pp. 107-14 Ch. 6; Ch. 4

Second Semester

Author	Title	Page Information
Marshall	*Principles of Economics*	Book V Ch. 6
J.B. Clark	*The Distribution of Wealth*	Preface, Ch. 1,7,8,11, 12,13,23
John Stuart Mill	*Principles of Political Economy*	Book II, Ch. 14
J.B. Hicks	*The Theory of Wages*	Ch. 1-6
Adam Smith	*The Wealth of Nations*	Book I, Ch. 10
Marshall	*Principles of Economics*	Book VI

		Ch. 15
Simon Kuznets and Milton Friedman	*Incomes from Independent Professional Practice*	Bulletin 72-3, National Bureau of Economic Research, section 5, appendix
F.H. Knight	*"Interest"*	*Encyclopedia of the Social Sciences, also in Ethics of Competition*
J.M. Keynes	*The General Theory of Employment, Interest and Money*	Ch. 11-14
Gustav Cassell	*Fundamental Thoughts in Economics*	Ch. 1,2,3

While at the National Bureau Friedman was an advisor to state income studies in Wisconsin and Delaware. Through his role in the Wisconsin study Friedman came to know Harold Groves, a public finance specialist in the Department of Economics of the University of Wisconsin. In early 1940 Groves inquired if Friedman would have interest in a position teaching statistics in the department. On February 24, 1940 Friedman wrote to Groves saying that he was very much interested, but needed to think about it a few days. At this point Friedman had changed his mind about completing his degree at

Chicago, for he told Groves that he had passed the Columbia Ph.D. preliminary exams and lacked only the thesis. For this he had received permission to use the professional incomes manuscript he and Simon Kuznets were preparing. He expressed hope that they would have it in good enough shape by June (1940) for circulation to National Bureau's Board of Directors. If this went well he would take the exam on the thesis and receive his degree before end of calendar year (M. Friedman to H. Groves, 2-24-40).

Groves's hope was that Friedman would come to Wisconsin as a statistician, taking primary responsibility for teaching statistics. But he ran into problems. At Wisconsin the faculty who taught the statistics course, less than adequately in Groves's view, were jealous of their prerogatives. For his part, Friedman was not excited at the prospect of specializing this narrowly. He wrote to Groves on March 2:

> As you know my interests extend to other fields, in particular econ theory and the relation between theory and quantitative analysis. I would be happy teaching only statistics for a year or two, but could I substitute another field for some of the statistics in a year or two? I would be glad to come out to Wisconsin so that my questions and those some of the Wisconsin people have about me could be cleared up (M. Friedman to H. Groves, 3-2-40).[5]

In mid-March Friedman sent Groves a vita for use in negotiating the position within the department.[6] This vita provides us with a picture of how Friedman identified himself professionally in 1940. He listed his fields of specialty at the University of Chicago as economic theory, statistics, and mathematics. Likewise, he listed Columbia

[5] {This is from the author's notes. Confirmation of accuracy needed.}
[6] {Add bibliographic information for citation.}

fields as economics and statistics. He had passed the preliminary examinations for the Ph.D. at both Chicago and Columbia. When he prepared the vita for Groves he was on the research staff at the NBER and Instructor in the Columbia University Extension. He also continued to serve as a consultant for the National Resources Planning Board (formerly the National Resources Committee). He listed fourteen publications (aside from book reviews), with five in analytical statistics and four in theory and estimation of demand. Friedman had three completed but unpublished manuscripts: his 1933 master's thesis on the relationship between railroad stock prices and earnings[7], a paper he wrote on fitting indifference curves while working for Henry Schultz, and a paper on use of regression in studies of family expenditure for the 1937 meeting of the American Statistical Association. In addition he was working on three manuscripts: the professional income study with Kuznets, a statistics textbook with Allen Wallis, and another paper with Wallis on R.A. Fisher's *Statistical Methods for Research Workers*. Friedman gave his interests in terms of "broad fields" and "specific substantive fields:

Broad fields --

1. Statistics: primarily, statistical research in economic problems and application of statistical methods to economic data; secondarily, statistical methods and mathematical statistics.
2. Economic theory: primarily, relationship between theoretical and empirical analysis, problems involved in reformulation theory in terms subject to empirical testing and in bridging the gap between theory and "facts"; secondarily, pure theory and mathematical economics.

Specific fields –

[7] Henry Schultz was his supervisor.

1. Measurement of national income and its distribution
2. Business cycles
3. Theory of capital and interest
4. Statistical analysis of family budget data

His eight references reveal his ties at the time:

1. Arthur F. Burns, NBER and Rutgers University
2. Harold Hotelling, Columbia University
3. Hildegarde Kneeland, National Resources Planning Board
4. Frank H. Knight, University of Chicago
5. Simon Kuznets, NBER
6. Wesley C. Mitchell, NBER
7. Jacob Viner, University of Chicago
8. R.C. McCren, Columbia University

Unable to wrestle the elementary statistics course away from Professors Erwin Gaumnitz and Philip Fox of the School of Commerce, Groves wrote on April 9 suggesting that Friedman offer mathematical economics, a course on income and wealth, and one on business-cycle economics. Friedman approved, saying that he was as happy with business cycles as with elementary statistics. In negotiations through the spring of 1940 a plan was worked out for Friedman to offer three courses during the fall semester: Mathematical Economics (Economics 197), Business Cycles (Economics 176), and Seminar in Wealth and Income (Economics 232).

No sooner was this plan made than another problem arose. Few of the graduate students were prepared mathematically for the mathematical economics course Friedman

envisioned. He wrote to Joseph Pechman, who appealed on the part of the students for keeping the mathematics "simple," that he and Groves had grappled with the problem of how to do justice to the course in one semester with students who were not advanced in math.[8] Their decision was to make calculus a prerequisite. Pechman's request persuaded Friedman that this was impractical. If he required calculus this would close the course off to the majority of the students. Friedman assured Pechman that he intended to keep the mathematics as simple as possible, but knowledge of calculus would be necessary. The outline of the course would be the same as his course at Columbia, except for the mathematical approach. He described the Columbia course as entirely non-mathematical. Friedman's accommodation to the circumstances was to require calculus or his consent for enrollment, while offering to have extra meetings through the first few weeks for students needing a primer in the requisite mathematics.

But there was yet another twist. In August Professor James S. Earley received a semester's leave, which meant Economics Theory (Economics 150) was not covered. Witte asked Friedman to teach that course in place of Mathematical Economics. He described their goal in the course: "What we have attempted to do in this course is to give a systematic modern theory course, stressing the orthodox approach but indicating major points of deviation in other modern theories" (E. S. Witte to M. Friedman, 8-5-40) So in the end Friedman taught neither statistics nor mathematical economics.

If we look at Friedman's contacts with friends and teachers in 1940 Allen Wallis stands out, as do his colleagues at the NBER. Of the latter, most had Columbia connections. Friedman knew Wallis from Chicago, but Wallis also had studied at

[8] Pechman conveyed that the students were enthusiastic about the opportunity to take mathematical economics, but with little emphasis on mathematics in the Wisconsin program feared their limitations (J. A. Pechman to M. Friedman, 5-8-40)

Columbia and worked at the National Bureau. In 1940 Wallis was teaching statistics at Stanford, and he and Friedman were collaborating on two statistics projects. Friedman expected to soon take his degree from Columbia, using his and Kuznets's National Bureau professional income study as his thesis. Two of Friedman's teachers who were influential at this point were Wesley Mitchell and Harold Hotelling. Mitchell, his boss at the National Bureau, became involved in the fall of 1941 in controversy over Friedman and Kuznets's claim that physicians' incomes were derived partly from monopoly power of the American Medical Association. Friedman was also following Mitchell's approach in teaching business cycles. He did not get to teach statistics at Wisconsin, but he did prepare a proposal for statistics curricula that was inspired by Hotelling's "The Teaching of Statistics" (1941?). This led, through Wallis as an intermediary, to his being appointed to the committee on the teaching of statistics of the Institute of Mathematical Statistics, of which Hotelling was chair. In the period immediately preceding and during his tenure at Wisconsin Friedman wrote two book reviews that set themes for what came to be his methodology, reviews of Jan Tinbergin's *Business Cycles in the United States of America, 1919-32* and Robert Triffin's *Monopolistic Competition and General Equilibrium Theory*. The ideas in these reviews developed out of the work he was doing in New York, at the NBER and the Columbia Extension.

Early in his first semester at Wisconsin Friedman gave a talk to the Wisconsin Graduate Club on "Neo-Classical Economics." His opening point was that there was no conflict between neoclassical economics and institutional economics. He said the difference between them was in how they reacted to not having access to experiments, like the natural sciences. Neoclassical economists relied principally on intellectual

experiments with "conceptual controls" and institutional economists fell back on comparative history. Friedman defended the neo-classical approach, giving as an example Marshall's analysis of taxes on printed matter and printing presses, and arguing that conceptual controls are essential for discovering indirect effects. Foreshadowing arguments he would develop later in his methodology essays, he told his student audience that:

> (a) to make [a] conceptual experiment [one] must decide on other things to be held constant – [we are] interested only in other 'relevant' things.
>
> (b) holding of other things same = provisional.
>
> [We] must not underestimate this contribution – major fallacies involve neglect of other things (M. Friedman, 10-18-40).

Friedman's judgment was that past contributions of neoclassical economics were larger than those of any other approach, but that its future contributions would be smaller.

Friedman unknowingly stepped into a bitter intradepartmental struggle at Wisconsin. Although invited to remain, he chose to leave Wisconsin after the one year (Friedman and Friedman, 1998, ch. 6). In the summer of 1941 the Friedman family returned to the East Coast and their Columbia – NBER contacts. Friedman had committed to a summer project in collaboration with Carl Shoup and Ruth Mack, to study use taxes to prevent inflation. This study was prompted by the United States' growing involvement in the war in Europe (Shoup, Friedman and Mack, 1943). Shoup was a Columbia professor whom Friedman had met through

the Conference on Income and Wealth. In the fall of 1941 Shoup, Mack, and Friedman submitted their study to the U.S. Treasury, and Friedman accepted an offer of a position in the Treasury's Division of Tax Research.

The Friedmans moved to Washington in late 1941 with friends from Columbia, Lowell and Agnes Harriss. The two couples took apartments in the same building, and Lowell Harriss and Friedman worked together at the Treasury. The Friedmans also reestablished Chicago and family contacts, for Aaron Director was living in Rockville, Maryland. Friedman's two years at the Treasury provided his first experience in actually shaping public policy. He notes with surprise in *Two Lucky People* that testimony on how to avoid inflation that he gave to Congress in May 1942 was "thoroughly Keynesian."

> The only 'methods of avoiding inflation' I mentioned in addition to taxation were 'price control and rationing, control of consumers' credit, reduction in governmental spending, and war bond campaigns.' Equally Keynesian is a comment I wrote somewhat earlier on a paper titled 'The Inflationary Gap,' by Walter Salant … in which he contrasted the OPA approach with the Shoup-Friedman-Mack approach.
>
> Until I reread my statement to Congress in preparing this account, I had completely forgotten how thoroughly Keynesian I then was. I was apparently cured, or some would say corrupted, shortly after the end of the war (Friedman and Friedman, 1998, p. 111.

In the spring of 1943 Allen Wallis recruited Friedman to the Statistical Research Group at Columbia. When he began work on March 1 Friedman rejoined both Chicago and Columbia acquaintances. Wallis himself embodied both. George Stigler was there, along with Rollin Bennett, who was a fellow Columbia student. Also, as we have seen, Harold Hotelling was at the Statistical Research Group. Economics played no direct role in Friedman's work at the SRG from 1943 to 1945 – he was a mathematical statistician.[9] Among the statistical projects to which he contributed were finding the optimum size and number of pellets in antiaircraft shells, design of proximity fuses for antiaircraft projectiles, sequential analysis, and sampling inspection.

Stigler left the SRG several months before the group was dissolved at the war's end, returning to the University of Minnesota from which he had been on leave. Once there he set about trying to obtain an appointment for Friedman. Friedman wrote to him from New York in May 1945, suggesting that he could offer courses in sequential analysis and sampling inspection. He received an offer, and taught both economics and statistics in his year at Minnesota. He and Stigler shared an office there and together wrote a critique of rent controls, "Roofs or Ceilings" (1946). Friedman recalls his year at Minnesota in *Two Lucky People*:

> What I remember best about the time I spent at the University of Minnesota is sharing an office with George Stigler. We had been close friends since our student days at Chicago, but sharing an office sealed an intimacy that was to last until George died in 1991 (Friedman and Friedman, 1998, p. 149).

[9] However, the statistical problems on which he worked did influence his economics. See (Friedman and Friedman, 1998, ch. 8)

During the year they were together at Minnesota Stigler revised on his price theory textbook (1946). After receiving an offer from the University of Chicago in the spring of 1946 Friedman began preparations for teaching price theory there. It was then that Friedman and Stigler began in discussions of price theory that continued for the next six years. Stigler wrote to Friedman in August 1946 after an exchange of letters on Marshall's *Principles*: "And in any case, only a crackpot would spend 7 months staring at the ceiling and then suddenly begin to read a book by the now ex-officemate when he knew in advance that he wanted to argue about it" (G. J. Stigler to M. Friedman, 8-19-46, in Hammond and Hammond, forthcoming)

Who were Friedman's closest friends during the period this sketch covers? Three individuals stand out, Rose Director, Allen Wallis, and Arthur F. Burns. Allen Wallis's career through the period is a mirror of Friedman's. Wallis followed Friedman in starting graduate school at Chicago and transferring to Columbia. His career likewise ran along the edge between economics and statistics. If Friedman leaned more to the economics side, Wallis leaned to the statistics side. As for other friendships, Friedman had more contact with Columbia people than Chicago people – Moses Abramovitz, Lowell Harriss, Eli Ginzberg. He met George Stigler, as he did Wallis, when he returned to Chicago from New York in 1934, and no doubt became friends then. Yet there is little evidence of regular contact until they were in New York together at the Statistical Research Group. Thanks to Stigler's efforts Friedman obtained a faculty position at the University of Minnesota in 1945. It was during that year sharing an office that the two became fast friends.

Of the two Friedmans, Rose and Milton, Rose had the deeper Chicago connections. She was an undergraduate there, and then a graduate student. She had more direct contact with the people who represent pre-war Chicago economics, taking courses from Henry Simons, Knight, and Viner. She worked as a research assistant for Knight, and started a thesis under his direction. Her brother, Aaron Director, was on the Chicago faculty when she transferred from Reed College to Chicago, and he prompted a rift between Knight and Paul Douglas by becoming a follower of Knight. While she was a student Rose Director had Christmas and Thanksgiving dinners with the Knight family (Friedman and Friedman, 1998, pp. 50-1). The Friedmans' move to Chicago in 1946 was more of a homecoming for Rose Friedman than for Milton Friedman. She had spent more time at Chicago than he and she was reunited with her brother, who returned the same year. The Friedmans were invited by Rose's mentor Frank Knight to stay with his family while they looked for a place to live. So it appears that the main tie between Milton Friedman and Frank Knight up to 1946 was Rose Director Friedman.

I conclude this part of the paper by considering evidence on the what, when, and why of "The Marshallian Demand Curve." Friedman completed the first draft in June 1948, sending copies to several people for comments, including Stigler. Stigler wrote his response on June 21, saying:

> I am overwhelmed with last minute chores before leaving for Canada, which we plan to do in a week, so I have given your note less thought than it deserves. Here are a few reactions, however:
>
> 1. First, you should read Marshall's earlier editions and other works, not that I expect any harvest.

> 2. I think you are wrong in attributing to Marshall this meaning of his demand curve. Viz.
>
> You take the positions (1) he was realistic, and (2) he was a magnificent logician, and seek for an internally and externally consistent interpretation of what he says. In this I think you are too generous. If your interpretation is correct, you have convicted him of complete illiteracy; not even in his mathematical appendix does he give explicit support to you (G. Stigler to M. Friedman, 6-21-[48], in Hammond and Hammond, forthcoming).

The letter gives every indication that this was Stigler's introduction to Friedman's ideas on the Marshallian demand curve. In August 1946 Friedman wrote on another matter:

> As you know, I have been reading Stigler to prepare for teaching; I have been also reading Marshall. And this noontime I was comparing what Marshall and Stigler had to say on the law of diminishing returns. Stigler, pp. 116-25; Marshall, Bk IV, ch. III, par. 1, pp. 150-3 in my edition. Marshall is very convincing; Stigler says, in effect, that Marshall is guilty of "question-begging", that his "and similar proofs are essentially tautological"; yet Marshall sounds anything but tautological, he sounds realistic and as if he were basing his results on sound observation. As nearly as I can figure it out, Stigler has a sound point; but with little trouble Marshall can be rehabilitated, and, when he is, is far more convincing than Stigler. I thought you might be interested in a brief discussion of the point, and it gives me an excuse to get it down on paper

> (I am making a carbon of this letter for my files) (M. Friedman to G. Stigler, 8-12-46, in Hammond and Hammond, forthcoming).

Their correspondence about various aspects of price theory including the convexity of indifference functions and diminishing marginal utility, the Giffen paradox, utility theory, and methodology continued into 1952, with Friedman invariably Marshall's advocate and Stigler either taking on or being placed by Friedman in the role of critic.

Friedman's was introduced to Marshall's economics before he took price theory from Jacob Viner at Chicago. The introduction was from Arthur Burns when Friedman was an undergraduate at Rutgers. He recalls in *Two Lucky People*:

> Another impact [of Burns] -- and I do not recall how it came about -- was to introduce me to the great nineteenth-century economist, Alfred Marshall. Arthur was a great admirer and thorough student of Marshall's *Principles of Economics*, and we spent many a pleasant hour then and in later years discussing the precise interpretation to be placed on passages from that magnificent book (Friedman and Friedman, 1998, p. 30).

The Rutgers catalogue suggests how this introduction may have taken place. Economics 111 and 112 were Current Economic Theory. The content of this course and the instructor changed from year to year. The 1930-31 catalogue lists Burns as the instructor and Marshall's economics as the topic. The course description relates:

> The course will be based on Alfred Marshall's *Principles of Economics* and, if time permits, his *Official Papers*. The purpose of the course is to acquaint students with the substantive contents of a rounded system of economics, to inquire into the adequacy of this system of theory as an

> interpretation of economic behavior, and to train students in the art of economic reasoning.

The course is not on Friedman's Rutgers transcript. He must have either sat in on Burns's lectures, a practice he later adopted at the University of Chicago and at Columbia, or as they became friends Burns talked with Friedman about the topic that he was teaching.

Friedman's first published brief for Marshallian price theory was his review of Robert Triffin's *Monopolistic Competition and General Equilibrium Theory* (1941). He wrote the review while on the faculty at the University of Wisconsin, having been away from Chicago, Knight and Stigler for over five years. In this short review we find the germ plasm of "The Methodology of Positive Economics" and the methodological component of "The Marshallian Demand Curve." In November 1947 Friedman sent Stigler an offprint of the review, thinking that it might help him decide how to respond to Edwin Chamberlin's review of *Price Theory* (1947). That this was Stigler's first exposure to Friedman's developing methodology is suggested by how Friedman introduced the piece:

> The main additional point I would like to make is that you do not really go at all far enough. I have gotten involved for various irrelevant reasons in a number of discussions of scientific methodology related to the kind of thing you are talking about (M. Friedman to S. Stigler, 11-19-47, in Hammond and Hammond, forthcoming).

This evidence suggests that "The Marshallian Demand Curve" developed from two threads, Friedman's returning to Marshall to prepare for teaching the Chicago graduate course in price theory in 1946 and his methodological objections to the Walrasian

tendencies in the theory of monopolistic competition. Knight's 1944 article appears to have had little minimal influence as does Friedman's experience a decade earlier with the Schultz-Hotelling project.

Conclusion

My focus has been for the most part on Milton Friedman. I see a gap that needs to be closed between Friedman's work as a University of Chicago student with Henry Schultz on price theory in 1934-35 and his return to price theory as a Chicago faculty member in 1946. Without closing this gap the thread that Hands and Mirowski run from the Schultz-Hotelling impasse with the income constraint and integrability conditions does not reach Friedman's part in the development of postwar Chicago price theory.

The Hands-Mirowski thesis is a tidy historical box for explaining the evolution of neoclassical price theory in the United States over the middle third of the twentieth century. As with any thesis that explains much, there is the danger of explaining too much – more than the historical evidence will bear. I have attempted to put their thesis to a test, albeit with regard to only one of the three parts of their historical account, the Chicago School. I have extracted from their papers conjectures about historical details, and suggested that many of the conjectures are unsupported. It may be that there is evidence that I have overlooked or that the weight of the evidence is other than I have suggested. But at this point it appears to me that Hands and Mirowski have stacked boxes on the storage shelf of historical knowledge of American economics all too neatly.

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