

***The Development of Chicago Price Theory: Evidence from the  
Early Friedman - Stigler Correspondence\****

Claire Holton Hammond and J. Daniel Hammond  
Professors of Economics  
Wake Forest University  
Winston-Salem, NC 27106  
Voice: 336-758-5334  
fax: 336-758-6028  
hammonc@wfu.edu, hammond@wfu.edu

Paper for the History of Economics Society Meetings  
University of British Columbia, July 2000

***How in the world do you ever find time to write so many things, all good?***  
*(Milton Friedman to George Stigler, April 7, 1948)*

---

\*Note: This is a working paper and is not to be quoted. The correspondence has been collected from the Friedman Papers in the Hoover Institution Archives, the Stigler Papers at the University of Chicago's Regenstein Library, Milton Friedman's personal office files, and files in the possession of Stephen M. Stigler at the University of Chicago. We would like to thank Milton Friedman and Stephen Stigler for making the correspondence available to us.

What if you shared an office and developed a close friendship with a future Nobel Laureate in economics? How would the development of your career and your ideas be influenced? In 1945-46 Milton Friedman and George Stigler, two future laureates (Friedman in 1976 and Stigler in 1982), were colleagues and officemates at the University of Minnesota. After their year at Minnesota they corresponded regularly with each other and their extant correspondence gives us insights into how the two economists and friends developed their ideas, interacted with colleagues and institutions, and juggled family lives and careers.<sup>1</sup>

This paper concentrates on price theory issues contained in their earliest correspondence beginning in 1946 and continuing into the early 1950s.<sup>2</sup> During these years Friedman and Stigler were building their reputations as formidable price theorists and heirs to the Chicago price theory tradition. It is clear from the letters that each did not develop his ideas in a vacuum for they discussed with one another almost every price theory question that either was investigating. In addition, the correspondence illustrates that understanding and correctly interpreting Marshall's *Principles of Economics* was important to both economists. Discussions of Marshall consumed a significant portion of their early correspondence.

We begin by allowing two letters to tell us what topics they considered as falling under the price theory nomenclature. We then use other letters to trace the influence of each economist on the ideas ultimately published in price theory articles and textbooks. Particular attention is given to the Friedman-inspired revisions to Stigler's *The Theory of Price* and to Friedman's comments on Stigler's work on the Giffen paradox, monopolistic competition, and the development of utility theory. Similarly, the paper highlights Stigler's influence on Friedman's famous methodology essay and Stigler's criticism of Friedman's interpretation of Marshallian demand theory. Throughout, the paper highlights the process of collaboration as practiced by these two giants of twentieth-century economics.

---

<sup>1</sup> Stigler and Friedman first met in 1934 when they were graduate students at the University of Chicago. After World War II Stigler returned to the University of Minnesota where he had taught since 1938. He helped Friedman get a job there for the 1945-46 year. In 1946 the University of Chicago hired Friedman (passing over Stigler) and Stigler accepted positions at Brown University and then Columbia University. The two friends would not teach at the same institution again until 1958 when Stigler finally joined Friedman on the faculty at the University of Chicago.

<sup>2</sup> This is part of an ongoing project that will culminate in the publication of the entire correspondence between Stigler and Friedman. In this paper we identify letters by record number (rec) and date from our database. Excerpts from the letters are transcribed without correcting misspellings or typographical errors.

### **Stigler and Friedman on Price Theory in General**

In January 1949 Stigler wrote Friedman asking his opinion on the appropriate scope for an upcoming volume of readings in price theory that he and Kenneth Boulding were editing for the American Economic Association.<sup>3</sup> Stigler wrote,

Price theory can be made to embrace almost every branch of economic theory, but it is conventionally interpreted to embrace utility and demand, costs and pricing, and perhaps distribution. The doubtful areas are:

1. Welfare and "socialist" economics
2. Economic dynamics
3. Distribution
4. Empirical studies and studies of empirical methodology (illustrated by say articles on the supply curve of labor and on statistical demand curves, respectively) (rec 28, 1/26/49).

Friedman replied on February 22 with his suggestions.

As to the scope of the volume, it seems clear to me that distribution should definitely be included under price theory. I am also inclined to argue that some material under your heading four should also be included. The line to draw here is between studies or articles about empirical work that are of interest primarily for substantive conclusions they contain and studies or articles that are of interest primarily for their discussion of concepts on which the statistician is to work or for their discussion of the implications of the statistical work for theoretical constructs. To illustrate I think E.J. Working's article on "What Do Statistical Demand Curves show" in the Quarterly Journal of Economics for 1927 is an article that would well deserve reprinting. It deals with the problems of statistical demand curves but in the process it has a good deal to say indirectly about the theoretical concepts involved.... I am somewhat more dubious about welfare and "socialist" economics. Again there are some articles of this character such as some of Lange's which really are exercises in economic theory.... On the whole I should be less inclined to include items under your heading one than under your heading four. (rec 29, 2/22/49).<sup>4</sup>

In the following pages some of Friedman's and Stigler's notions on "utility and demand, costs and pricing, and perhaps distribution" will become apparent.

---

<sup>3</sup> *Readings in Price Theory*. Edited by George Stigler and Kenneth Boulding. Homewood, Ill.: Irwin (for the American Economic Association), 1952.

<sup>4</sup> Following Friedman's suggestion, Stigler and Boulding included E.J. Working's article in their volume.

### **Friedman on Stigler's *The Theory of Price***

"As you know, I have been reading Stigler to prepare for teaching; I have been also reading Marshall" (rec 128, 8/12/46). With these words, Friedman began what turned out to be a lengthy correspondence with Stigler regarding the first edition of his *The Theory of Price* (1946).<sup>5</sup> From summer 1946 through winter 1947 Friedman and Stigler debated several points that Stigler had made in his book. Their debates were flavored by 1) Friedman's quest to understand price theory as explained by both Stigler's and Marshall's textbooks, 2) both economists' efforts to be good teachers, and 3) good natured ribbing about who knew more price theory and who could pose and answer the most interesting questions.<sup>6</sup>

#### **1. Convexity and Diminishing Marginal Utility**

The first issue on price theory that engaged the two economists at depth concerned the clarity and correct interpretation of a footnote on utility theory found on page 71 of *The Theory of Price* (1946). Stigler and Friedman wrote six letters from summer 1946 to December 1946 on this issue and we have found all but two of these letters. Their debate centered on whether or not diminishing marginal utilities were sufficient and/or necessary for convex indifference curves (increasing marginal rate of substitution) and thus a stable consumer maximum and, vice versa, whether convexity (increasing marginal rate of substitution) implied diminishing marginal utilities.

The footnote on p. 71 reads:

The principle of an increasing  $S_{yx}$  [marginal rate of substitution] corresponds to the older theory of diminishing marginal utility of a commodity as its quantity increases. The two principles are equivalent in the special case where the marginal utility of X is independent of the quantity of Y; in general, however, neither necessarily implies the other (Stigler, 1946).

In the first letter, which we have not found, Friedman had apparently taken Stigler to task concerning the rigor of this footnote. Stigler defended his

---

<sup>5</sup> The references are to Stigler, *The Theory of Price*. New York: The Macmillan Co., 1947 and to Alfred Marshall, *Principles of Economics*. *The Theory of Price* was an augmented version of *The Theory of Competitive Price* (New York: The Macmillan Co., 1942). Newly covered topics included imperfect competition, multiple products, and interest theory. Regarding Marshall, we do not know the edition of Marshall's *Principles* that Friedman was reading. However, some evidence from the letters suggests it was the 8<sup>th</sup> Edition, originally published in 1920.

<sup>6</sup> The flavor of their ribbing is displayed in an early Stigler comment to Friedman: "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" (rec 14, 8/19/46).

footnote: "Thus the so-called stability conditions [for a consumer maximum] require that the marginal utilities diminish and more besides, and this is what my note on p. 71 says. Your criticism is similar to mine this spring that the stability conditions are wrong, and why isn't your answer to me then an answer to yourknow" (rec 131, Summer 1946)?

Friedman was not convinced. He wrote to Stigler in November and again in December 1946 (rec 162 and rec 163). Friedman described two convex utility functions. The first utility function was with diminishing marginal utilities, the second with increasing marginal utilities, and, according to Friedman, "this contradicts your footnote. Q.E.D." (rec 162, 11/27/46). He challenged Stigler, "Redouble that if you can" (rec 163, 12/2/46).

Stigler replied later that month with a two-page, typewritten clarification of the utility problem. In this reply he conceded the point to Friedman. "You'll notice that I do not dissent from anything you say, poor footnote" (rec 12, 12/20/46).

In the revised edition of *The Theory of Price* (1952) Stigler replaced the footnote on page 71 with Mathematical Note 8 (p. 301). There he made it clear that "diminishing marginal utility does not imply convexity...nor does convexity imply diminishing marginal utility" (Stigler, 1952, p. 301). These were exactly the points that Friedman had doggedly pursued in their 1946 correspondence.

Moreover, Stigler also made sure to be clear about these points in his article, "The Development of Utility Theory. I" (Stigler, JPE, Aug 50). He wrote, in apparent response to Friedman's points, "It is clear that diminishing marginal utility...is not necessary for convexity, since  $\phi_{12}$  can be positive and large, and it is not sufficient, since  $\phi_{12}$  can be negative and large" (Stigler, 1950, p. 323, note 83).<sup>7</sup>

## 2. The Law of Diminishing Returns

Friedman next took Stigler to task regarding his proofs for the law of diminishing returns. Stigler's (1946) text discussed two types of proofs for the law of diminishing returns, which Stigler called the Law of Variable Proportions. He labeled the first type of proof "a priori" (p. 118) and defined it as "attempt[s] to deduce the law from self-evident propositions" (p. 118). He labeled the second type of proof "empirical: no one has discovered any important exceptions to the doctrine." (p. 120). He found "a priori" proofs unsatisfactory, and noted that they are "essentially tautological." In arguing in favor of the "empirical" line of proof he referred readers to examples of "quantitative verification" in numerous agricultural experiments, which, for example, showed that applying increasing amounts of fertilizer to a fixed plot of land leads to diminishing returns (Stigler, p. 120).

---

<sup>7</sup> The formula is 
$$\frac{d^2x_2}{dx_1^2} = -\frac{\phi_2^2\phi_{11} - 2\phi_1\phi_2\phi_{12} + \phi_1^2\phi_{22}}{\phi_2^3} > 0$$

Friedman considered these statements to be an attack on Marshall's method of proving the law. He paraphrased Marshall:

To restate [Marshall]: If the law of diminishing returns were not valid; i.e., if the application of additional units of labor and capital to a piece of land yielded constant or increasing returns, then individuals would have no incentive to get additional land and we should observe in fact that individuals used and wanted very little (rec. 128, 8/12/46).

Friedman continued:

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 (rec 128, 8/12/46).

Friedman proceeded to "rehabilitate Marshall" by setting up alternative hypotheses under different assumptions with respect to increasing and decreasing returns to variable proportions. He tested each hypothesis against observed fact, and drew his conclusions using the following method, "Our hypothesis leads us to expect a certain result, we find that result, hence our hypothesis is not contradicted" (rec 128, 8/12/46).

Friedman then posed a rhetorical question:

You may ask, why all this fuss when Stigler accepts the law on other grounds, namely, technological experiments [which Stigler had labeled empirical proofs]. The reason is that economic empirical evidence of the kind given by Marshall [that farmers are willing to pay for more land] is intellectually far more satisfying to an economist than technological evidence. In addition, part of my purpose is to show that Marshall here as elsewhere, was proceeding on a truly scientific basis, not on that tautological, formal basis that enervates so much of modern theory (rec 128, 8/12/46).

Stigler replied in a letter from August 1946 by posing a question to Friedman and then answering it himself. He wrote,

You say that economic empirical evidence is intellectually far more satisfying than technological evidence. I cannot claim even an intuitive understanding of this statement. Diminishing returns is technological, so you prefer an indirect to a direct proof....Because (rec 14, 8/19/46)?

Stigler's reaction to this question was

As a matter of fact, I am coming to believe that you are more consistently abstract and a priori-ish than I. But its cloaked over by your emphasis on realism, which I would like to have you define. I shall conjecture, if

only to hasten the enlightenment, that you like a firm skeleton of rigorous theory well skinned with concrete illustrations, in the manner of Marshall and Burns, all oriented in accordance with your general view of how economic life runs. In any case, I do (rec 14, 8/19/46).

It is clear from the revised edition (1952) of Stigler's textbook that Friedman's criticism was influential. There Stigler states a preference for what he had labeled in the first edition as "a priori" proofs. He wrote in the revised text

The law of diminishing returns was first demonstrated with...the following argument. No one would resort to the cultivation of inferior lands if he did not 'run into' diminishing marginal returns on fertile land. Since inferior lands were in cultivation...diminishing returns must be present....

**This early proof has been discussed because it is illustrative of the type of proof we shall give later. In essence we postulate diminishing marginal returns, deduce the consequences of this postulate for observable entrepreneurial behavior...and then test the consequences (predictions) against observation"** emphasis ours (Stigler, 1952, pp. 119-120).

In the revised edition Stigler no longer referred to a second type of proof, the "empirical" proofs. Instead, Stigler declared experimental evidence useful but for reassurance purposes only. "Nevertheless is it reassuring to notice that diminishing marginal returns has been found in direct experiments in a considerable number of cases, and that continuously increasing marginal returns has not been found in such experiments" (Stigler, 1952, pp. 121-122).

The methodological issue is how one distinguishes a priori proofs from empirical tests. In his letter quoted on the previous page (rec 14) Stigler gave no ground, but his revision of the discussion for the revised edition of *The Theory of Price* suggests that Friedman won the argument that what Stigler had labeled "a priori" was after all an empirical test.

### 3. Pedagogical Issues

Several of Friedman's letters challenged Stigler to explain why he had organized his textbook the way he did and to consider the usefulness of Marshall's text to students in the 1940s. Stigler explained,

On my organization [of *The Theory of Price*], I wrote with a view of cleaning up technical details in print so I could spend my time in class on economics and that is what I do. But I do much more of this now than formerly and would undoubtedly approach things differently if I were to start anew. But the organization isn't so important (to me); I get into things like rationing already on demand. The reason I put income analysis first is simply to acquaint the dreadfully ignorant students with

some general features of our economic system. You will feel this need too<sup>8</sup> (rec 131, summer 1946).

On Marshall as a textbook Stigler proffered,

I would like to point out that Marshall means much to you only if you know a good deal. I have used him consistently for text or supplementary reading and all young students, good and bad, have one hell of a time getting much out of him. Sentences that strike you (and me) as luminous generalizations seem to them space-filler. It wouldn't be fair, but I'd bet money that students reading only my book would get better grades on your exams than those reading only M (rec 131, summer 1946).

In other letters Friedman and Stigler became involved in developing interesting questions and then answering them. Some of these questions ended up as examination questions for their students, others became problems included in their textbooks. Friedman started one exchange in November 1946,

I am going to start picketing you long distance. "Stigler is unfair to teachers of economic theory". I wanted to assign some standard problems -- dumping & price leadership & index no. -- & lo & behold, they are all worked out in Stigler [*The Theory of Price*]. I am enclosing a couple of problems which I finally worked out to get around Stigler's unfair competition. (rec 162, 11/27/46).<sup>9</sup>

In early 1947 Stigler posed a question that led to a lengthy exchange. It became one of the applications of utility analysis that Stigler included in the 1952 revision of his textbook (pp. 83-87). It also became part of the collection of problems that Friedman routinely assigned in his classes and was published in Friedman's *Price Theory* (1962, Appendix B, pp. 268-9) under the title "Point Rationing Plus Price Rationing."<sup>10</sup> The question was "prove that when there are 2 rationing systems, all consumers gain if one is convertible into the other -- i.e., if points may be purchased & sold" (rec 336, 1/27/47, in this letter Friedman is quoting Stigler).

For Friedman the question was a "neat one, since it seems speciously plausible." He spent two days working to construct a proof, failed, and then constructed a counterexample to disprove it. He challenged Stigler, "I now have one [a counter example] & I refuse to give the question to my class until you

---

<sup>8</sup> This is a reference to Stigler's chapter 3, section 2, "The Structure of an Enterprise System," (1946) beginning on page 35. In the revised edition, Stigler (1952) moves this discussion forward to Chapter 1, section 2, beginning on p. 5.

<sup>9</sup> We have not found the enclosure.

<sup>10</sup> Problems on monopoly and patent owner fees were also discussed briefly in the correspondence from early 1947 (see rec 336 and rec 11).

explain it away (if you can, ah hah)" (rec 336, 1/27/47). Friedman's counterexample covered over three handwritten pages.

Stigler responded with his own three-page explanation that granted to Friedman that "what you say is alright in theory but doesn't work out in practice. I had in mind, but not on paper, a much weaker (and stronger) statement: that given the rates of conversion between points and goods and money and goods, each party gains (or in a limiting case breaks even) from conversion between money and points." He conceded to Friedman, however, that "your generalization raises interesting questions" (rec 11, 1/31/47). He ended his letter, "And now I must leave the formal playthings of the Chicago school and turn to the hard realism of reading some history of doctrine" (rec 11, 1/31/47).

This correspondence clearly informed Stigler as he wrote a new section "Notes on Non-Price Rationing" for the 1952 revision of his textbook. The relevant pages read, "Given, then, a positive money price for points, it is possible to show that two persons with equal money and point incomes but different tastes may gain (and cannot be injured) by permitting the sale of points for money" (p. 86). He concluded the section by noting, "The analysis is inconclusive on the effects of conversion of points and money when the individuals have different incomes." (p. 87). This final point was exactly the point Friedman had proved with his counterexample.

Friedman also had a role in Stigler's decision to drop the comparison of a personal income tax and an excise tax in the section on applications of indifference curves (Stigler, 1946, pp. 81-82). In the 1946 edition, Stigler asserted that "the individual taxpayer will be on a higher indifference curve if a certain sum of money is taken from him by a personal income tax than if it is raised by means of a commodity (excise) tax" (Stigler, 1946, p. 81). He referred the reader to an article by M. F. W. Joseph as the source for this application.<sup>11</sup>

In December 1946 Friedman wrote Stigler, "I've become concerned about the allocative effect of a progressive income tax, & am about ready to assert that it has allocative effects of the same order as special excise taxes. This is particularly nasty (1) because we like the income tax; (2) because it brings out a serious blunder in prof[essional] incomes. I should have compared earnings in diff[erent] occupations net of income tax, rather than gross, as I did" (rec 163, 12/2/46).<sup>12</sup>

---

<sup>11</sup> M.F.W. Joseph, "The Excess Burden of Indirect Taxation," *Review of Economics Studies*, VI (1939): 226-231.

<sup>12</sup> Friedman is referring here to the monograph *Income from Independent Professional Practice* (New York: National Bureau of Economic Research, 1945) which he co-authored with Simon Kuznets.

Friedman ultimately proved this point in his article, "The 'Welfare' Effects of an Income Tax and an Excise Tax" published in early 1952.<sup>13</sup> In this article he also cited Joseph as the originator of the incorrect proof and noted that Stigler's textbook (and others) had repeated Joseph's error. While writing the article Friedman gave Stigler advanced warning of this criticism. Stigler had no objection. He wrote Friedman in spring 1951, "I had already grown to dislike that excise-income tax [application], and I admit its error" (rec 4, 3/27/51). He subsequently deleted the application in the 1952 edition of his textbook.

### **Friedman on Stigler's "History of the Giffen Paradox"**

Stigler drew Friedman's attention to his next research project by criticizing Marshall, whom he thought Friedman was prone to "overpraise" (rec 131, Summer 1946). "I am investigating the Giffen paradox, historically, and when I'm done I'll send you a note on it. Meanwhile it is clear (1) Marshall doesn't shine, (2) the evidence for the paradox is deeply hidden" (rec 10, 11/46).

After he received the draft ("Notes on the History of the Giffen Paradox," 11/46, mimeo) Friedman wrote that he thought the piece very nice and that he had almost no comments on it. He admitted that Marshall did not come out so well. He suggested that Stigler might send it to the JPE, which "is desperate for material & would be overjoyed at getting your note. It (the note) may be too good for the JPE – but why not bring the JPE up instead of the other way" (rec 162, 11/27/46).

But "almost no comments" did not mean no comments, and as we shall see, the comments had an effect on Stigler's drafting of the paper for the JPE (April 1947, pp. 152-6). Friedman questioned Stigler's inferences from some of the paper's statistical evidence. Stigler subsequently replaced part of the disputed evidence and modified the interpretation in line with Friedman's comments.

Stigler's thesis was that the Giffen paradox had received more attention from economists than it should, for there had never been a systematic empirical demonstration of its validity and the evidence that had been used to look for it suggested that it was unlikely to be demonstrated. Stigler singled out Marshall for overemphasizing the likelihood of the Giffen paradox, and credited Edgeworth for taking the role of skeptic.

Stigler's draft quoted Marshall's introduction of the paradox from the third edition of his *Principles* (1895).

There are however some exceptions. For instance, as Mr. Giffen has pointed out, a rise in the price of bread makes so large a drain on the resources of the poorer labouring families and raises so much the marginal utility of money to them, that they are forced to curtail their consumption of meat and the more expensive farinaceous foods: and, bread being still the cheapest food which

---

<sup>13</sup> *Journal of Political Economy*, LX (February, 1952): 25-33.

they can get and will take, they consume more, and not less of it. But such cases are rare; when they are met with they must be treated separately (Marshall, p. 208; Stigler, mimeo, p. 1; Stigler, 1947, p. 152).

Friedman's first comment had to do with Stigler's interpretation of correlation coefficients of +0.213 for British per capital consumption and the price of wheat, and +0.182 for the same variables holding income constant, for the years 1884 – 1908. Stigler's interpretation was:

There is a small, statistically non-significant positive correlation between quantity and price ( $r = .213$ ); even this small correlation would diminish and perhaps disappear if we disregarded the years of very high and low prices, as Marshall indicated we should. If we take income roughly into account by using Bowley's index of the national wage bill, the data still fail to increase our confidence in the validity of the paradox (Stigler, mimeo, p. 6).

Friedman argued that though insignificant, the positive sign gave more credence to the Giffen paradox than to an inverse relationship between price and quantity. "Granted that a correlation of +.213 or of +.182 is not significantly different from zero, & hence does not contradict the hypothesis of a d.c. [demand curve] of zero elasticity (or a range of d.c.'s with neg. elasticity), it clearly contradicts even less the hypothesis of positively sloping d.c." (rec 162, 11/26/46). Friedman pointed out that the 95% confidence limits on the correlation of consumption on price, holding income constant, are approximately -.26 to +.55. "Your discussion gives the impression that the statistical data are inconsistent with Giffen; though all you can say is that the data are consistent with it -- & give it somewhat more support than the opposite – but unfortunately (or fortunately) are inconclusive" (rec 162, 11/27/46).

For the published version of the paper Stigler substituted other data on price and consumption, data that showed a negative though insignificant rank correlation. This allowed him to escape Friedman's conclusion that the positive correlation supported Marshall.

Friedman also objected to Stigler's interpretation of data showing income and expenditures on bread and flour from a 1904 study of workingmen's budgets. From the data (which are given in table 2, p. 155 of the JPE article) Stigler concluded, "it is clear that Marshall was wrong in believing that the workmen's income elasticity of demand for wheat is negative" (mimeo, 1947, p.7). Friedman's objection was that with one-week cross-section observations, the lower income classes could have consumed more than they purchased. If so this could convert what appeared to be no relationship into an inverse one. In the published paper Stigler changed his inference from these data to: "Again the data are defective (income and consumption expenditure are for only one week), but later English budget studies reveal larger positive income elasticities for wheat" (JPE, 1947, p. 155).

**Stigler on Friedman's "Lerner on the Economics of Control"<sup>14</sup>**

In mid-July 1947 Stigler took Friedman's manuscript entitled "Lerner on the Economics of Control" along with him to his vacation cottage in Windemere, Canada. He wrote, "I've read it once, and while it is perfectly satisfactory, it doesn't pack the Lange wallop; perhaps I may get some ideas" (rec 35, 7/11/47).<sup>15</sup> Later that month Stigler returned the manuscript to Friedman and wrote that he had made "only a few minor comments" due to a deluge of summer visitors (rec 366, 7/47). In a postscript he added, "In a last skimming of the MS, my chief objection is your practice of reciting his [Lerner's] failings; it would be much stronger to indicate the type of thing he should take into account (rec 366, 7/47).

From close readings of Stigler's numerous handwritten comments on the draft manuscript and the published version of the paper, it is clear that Friedman took all of Stigler's comments seriously. To accommodate Stigler's suggestion to "indicate the type of thing he [Lerner] should take into account" Friedman pulled the following two paragraphs buried on pg. 8 of the manuscript almost word for word into his introduction.

What are the institutional arrangements proposed by Lerner to attain the economic optimum? It turns out to be much more difficult to answer this question than would be expected from the title of the book, the introduction, or even a first reading. These all somehow generate the expectation and the illusion of a detailed program. "In this way we shall be able to concentrate on what would be the best thing that the government can do in the social interest--what institutions would most effectively induce the individual members of society, while seeking to accomplish their own ends, to act in the way which is most beneficial for society as a whole" (p. 6). An attempt to set down the explicit details of the program dispels the illusion. Much of what at first reading sounds like detailed proposals, particularly about the general structure of the society, turns out to be simply an admonition to the state that it behave correctly and intelligently.

The hortatory character of the proposals is foreshadowed in Lerner's initial discussion of "the rationally organized democratic state", which he names "the controlled economy". "The fundamental point of the controlled economy is that it denies both collectivism and private enterprise as principles for the organization of society, but recognizes both of them as perfectly legitimate means. Its fundamental principle of organization is that in any particular instance the means that serves

---

<sup>14</sup> *Journal of Political Economy* 55 (October 1947): 405-416. Reprinted in *Essays in Positive Economics*. Chicago: University of Chicago Press, 1953. Pp. 301-319.

<sup>15</sup> The reference is to Friedman, "Lange on Price Flexibility and Employment." *American Economic Review* 36 (September 1946): 613-31. Reprinted in *Essays in Positive Economics*. Chicago: University of Chicago Press, 1953. Pp. 277-300.

society best should be the one that prevails" (p. 5, italics in original). Now surely it is no principle of organization that society do what is best for society. At most, it is an objective of society, though even as an objective it is obviously question-begging (p. 8, "Lerner on the Economics of Control," draft ms., rec 366. Compare to pp. 405-406 of published article.)

The majority of Stigler's comments were minor word changes that Friedman usually adopted. But Stigler also criticized Friedman's clarity in a few places. Friedman always changed his text in response. For example, Friedman wrote in the manuscript,

The analysis as given is not rigorous even on its own level and contains several logical loopholes; by rather slight modifications, however, these loopholes can be closed and the conclusion made to follow rigorously from five assumptions: (1) interpersonal comparisons of satisfaction are "not meaningless"; (2) the utility derived by an individual from any given income is independent of the income of other individuals... ("Lerner on the Economics of Control," draft ms., p. 4).

Stigler wrote in the margin: "this (1) is surely crazy; do you mean that a uniquely measurable utility function exists? Also (2) is surely not necessary, and is in fact highly unrealistic. This section + appendix is too much for a trivial point" (holographic note on "Lerner on the Economics of Control, draft ms. p. 4, emphasis Stigler). Friedman was evidently persuaded. In the published version the appendix is dropped and the text is changed so that Friedman uses direct quotations from Lerner, thereby eliminating any confusion regarding his paraphrasing. Friedman substituted,

It requires only a slight modification of the argument, however, to eliminate this appeal and to make Lerner's conclusion a rigorous implication of his assumptions, of which the following five are essential: (1) "It is not meaningless to say that a satisfaction one individual gets is greater or less than a satisfaction enjoyed by somebody else" (p. 25). This is taken to mean that numerical utilities can be assigned to the satisfactions enjoyed by individuals; and the value assigned to different individuals can appropriately be added. (2) "Each individual's satisfaction is derived only from his own income and not from the income of others" (p. 36). This means that the utility to an individual of any given income is not a function of the income of other individuals. (JPE, 1947, p. 308)

Other of Stigler's suggestions involved giving due credit for the sources of ideas. On page 4 of the manuscript Friedman wrote, "Lerner does not consider directly the distribution of resources among individuals, but rather the associated problem of the distribution of income. The brief chapter dealing with this problem is highly original and extremely interesting" (emphasis Friedman). But Stigler noted, "Edgeworth got same result by same argument and Marshall's utility of groups is similar" (p. 9). In the published version Friedman responded by omitting the words "highly original" (see p. 308). Regarding Friedman's discussion of income inequality and differences across individuals in capacity to

enjoy satisfaction, Stigler noted "you know Simons made this argument in *Pers. Inc. Taxation*" (ms., p. 6). In the published version Friedman included the reference.<sup>16</sup> Later in the manuscript (p. 9) Friedman referred to the four functions of prices. Stigler noted in the margin "They also ration consumers when S is fixed." In the published version (p. 304) Friedman included Stigler's suggested addition, "And, finally, prices serve (5) to ration fixed supplies of goods among consumers." It is interesting that Friedman cited Knight's *The Economic Organization* (1933) as the source for this fifth function of prices, not Stigler.

From reading the Stigler-annotated manuscript and Friedman's published article, it is clear Friedman took Stigler's comments seriously. However, Friedman significantly reorganized the paper before its publication, rearranging whole sections. This reorganization was not prompted by Stigler's handwritten comments. Similarly, while Friedman usually replaced words or phrases according to Stigler's suggestions, he did not always do so.

### **Stigler and Friedman on Methodology and Monopolistic Competition**

Edward Chamberlin reviewed Stigler's *The Theory of Price* in the June 1947 issue of the AER (Vol. 37, June 47, 414-418). He sent Stigler a proof of the review, which was highly critical of Stigler's organization and his treatment of imperfect competition including monopolistic competition. Stigler replied directly to his critic (rec 18), concluding his letter to Chamberlin with the statement that "I am prepared to argue (1) that your theory is indeterminate, and (2) that it is 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 (Stigler to Chamberlin, rec 18, 8/47).

Stigler sent a copy of this letter to Paul Homan (editor of the AER). Homan suggested that Stigler write a short reply to Chamberlin for publication in the AER. In early August 1947 Stigler sent a copy of his Chamberlin letter to Friedman and asked him for his advice. He wrote,

I am not inclined to do this [write a reply for publication] because (1) of a general feeling against replies to reviews, and (2) the inappropriateness of a short note in dealing with this matter (and the disinterest in a long one). All I gain by a reply is creation of doubts in the minds of those economists (numerous, alas) who think Chamberlin is a great man. What say? (rec 17, 8/47)

We don't have Friedman's answer to this letter. The next correspondence concerning the Chamberlin issue is dated November 19, 1947 (rec 13). It

---

<sup>16</sup> See footnote 7, p. 311. It reads, "This argument is essentially taken from Henry C. Simons, *Personal Income Taxation*."

includes Friedman's reaction to "your piece on Chamberlain."<sup>17</sup> Friedman wrote, "As you know, of course, I thoroughly agree with you.... I am enclosing a reprint of a review of mine that you might find of some interesting relevance<sup>18</sup> (rec 13, 11/19/47).

The remainder of Friedman's letter deals with an important methodological issue that was raised in Friedman's mind after reading Stigler's draft lecture. With this letter Friedman opened a conversation on the appropriate methodology for economics. The letters that followed, dating from fall 1947 until fall 1948, directly influenced both the final version of Stigler's lecture on monopolistic competition and Friedman's article, "The Methodology of Positive Economics" (1953). Friedman began the methodological conversation,

The main ...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. In the course of these I have been led to go farther than I had before in distinguishing between description and analysis and in discarding comparisons between assumptions in reality as a test of the validity of a hypothesis. I should like to offer the general proposition that every important scientific hypothesis almost inevitably must use assumptions that are descriptively erroneous.... In a way, the better the hypothesis the greater the extent to which it simplifies, the more sharply will its assumptions depart from reality....

The only thing that really matters, therefore, is a conformity between implications and reality, since only after this has been established can one say what discrepancy between reality and assumption is significant (rec 13, 11/19/47).

Stigler's immediate response was to write that "I haven't studied your methodology [yet], but it seems so true as to be accepted (rec 15, after 11/24/47). Later Stigler wrote, "I've skimmed your comments on the Chamberlin thing enough to see that they are going to be bothersome, a synonym for useful, and I thank you" (rec 20, after 12/6/47).

The published version of the lecture shows that Stigler largely adopted Friedman's perspective. Stigler wrote, "it is necessary to set forth certain methodological principles" and credited "the present interpretation of these principles ...to Professor Milton Friedman" ("Monopolistic Competition in Retrospect," p. 23 and n.1). He continued,

---

<sup>17</sup> This is a reference to a draft of the lecture Stigler presented in London in early 1948. It was ultimately published as "Monopolistic Competition in Retrospect," *Five Lectures on Economic Problems* (London: Longmans, Green; New York: Macmillan, 1950). Reprinted by Books for Libraries Press, 1969, pp. 12-24. We have not found the draft version of the lecture.

<sup>18</sup> Evidently Friedman's review of Robert Triffin's *Monopolistic Competition and General Equilibrium Theory*, *Journal of Farm Economics* 23 (February 1941): 389-90.

The sole test of the usefulness of an economic theory is the concordance between its predictions and the observable course of events. Often a theory is criticized or rejected because its assumptions are 'unrealistic.'...

But this line of argument grants the ungrantable: it is often impossible to determine whether assumption A is more or less realistic than assumption B, except by comparing the agreement between their implications and the observable course of events. One can but show that a theory is unrealistic in essentials by demonstration that its predictions are wrong ("Monopolistic Competition in Retrospect," p. 23).

And in later correspondence, after he had read a draft of Friedman's "The Methodology of Positive Economics" <sup>19</sup>, Stigler wrote,

Personally I would like it published (in part because I've paraphrased the argument in two paragraphs of my Chamberlin essay, and would like to give a more specific reference). But I keep feeling that you arouse skepticism and opposition by stopping where you do. Because surely in some sense an assumption can be more promising than another. If I predict basing points in industries where the geographical pattern of consumption is unstable, you (I hope) will find these worth looking into. If I predict basing points in industries where Yale men are over Princeton men, and love to rib Fetter's disciples, you sneer, although you haven't a shred of evidence that the latter is inferior in predictive value to the former. It is surely possible to say something about some assumptions being more promising than others, and yet not to take back any of the things you are saying at present. If you can pierce this muddy frontier of your article, it would be a great improvement. (An alternative way of arguing this is: if we really could devise good theories only by theorizing and then testing against prediction, good theories would be chance events as likely to come from Seymour Harris -- no more likely, -- than from Smith or Marshall. Only an infinitesimal portion of scientific work would be rewarding. And I don't believe this.) (rec 23, 9/48).

Friedman replied at length.

I think part of the difficulty you have on the methodology problem arises out of the fact that the issues it deals with pertain only to one small part of all work in economics. One might, I suppose, separate out four kinds of things that economists and other scientist do: first, the collection of data to provide something to generalize from; second, the derivation of hypotheses to generalize the empirical uniformities discovered in the data; third, the testing of these hypotheses; and fourth, the utilization of them. My strictures apply only to the third of these steps. Clearly in the process of collecting data to be generalized, realism and respect for detail, and so on, are all to the good. The real problem that you raise arises, I take it, when somebody has proposed a theory which we haven't

---

<sup>19</sup> Probably "Descriptive Validity vs. Analytical Relevance in Economic Theory", undated.

as yet been able to test, and the question arises, shall we use it instead of some alternative. It's at this point that one is most likely to say that he is judging the theory by its assumption and to say that he will have some confidence in it if the assumptions are reasonable, and he will not if they are not. This is the kind of point Arthur [Burns] was raising most strenuously this summer against it. I'm inclined to argue that the logical counterpart of the intuitive process whereby we reach such judgments is a process of indirect testing, that our so-called theories are not separate, concrete, disparate things, but fit together into some kind of a whole. And what is involved is that we have certain phases of our theory in which we have a good deal of confidence because they have stood the test of experience, that certain kinds of assumptions or kinds of theories have in those fields turned out better than others, and that that's the real basis for our confidence in one theory or another....

Look at the question from another angle. You say if we really could devise good theories only by theorizing and then testing against predictions, good theories would be chance events. I don't believe that follows at all. We want theories about something and certainly whether we will have a good theory or not depends on what it is that the theory is designed to generalize [emphasis Friedman's]. The discovery of empirical regularities is not theorizing, and yet is there any doubt but that it provides a basis for theorizing and that it will greatly affect the validity of the theorizing that's done? A theory constructed to generalize alleged facts that are incorrect is so much wasted effort (rec 24, 10/4/48).

Friedman's lengthy response suggested to Stigler that he had been misunderstood. He replied,

I may be misinterpreted a little. I like your general position but want you to enlarge it, -- precisely as you are enlarging it in your letter to me [rec 24, 10/48]. While some elaboration along these lines will take some of the paradox out of your thesis (and in a certain sense weaken its message unless you write very carefully), it will create sympathy for and receptiveness to your thesis and make the paper much more influential (rec 26, 10/48).

Friedman incorporated Stigler's suggestions in the next draft of his paper, "The Relevance of Economic Analysis to Prediction and Policy."<sup>20</sup>

Other methodological issues also engaged the two correspondents, sometimes with their tongues in their cheeks. In December 1947 Stigler proposed three hypotheses to Friedman. The first he named "Stigler's Law: The gorgeousness of a theory varies with the range of phenomena it embraces and inversely with

---

<sup>20</sup> Hammond ("Early Drafts of Friedman's Methodology Essay," paper presented to the History of Economics Society, June 1991) shows how Stigler's and others comments were woven into what became the final version of "The Methodology of Positive Economics."

the number of its constants." Stigler II was "If businessmen deliberately adopt and persistently retain a practice, that practice is explicable in terms of maximum profits." The last was "Stigler's Razor: In dealing with economic theory, always use the most advanced branch of mathematics you can apply." (rec 21, 12/47). In reply to Friedman's reply (which we don't have), Stigler wrote

On methodology, there's a good deal to say for its essential elusiveness. It seems to me, as it appears to you, that most of the problems are psychological, and that one loses most of the real problems by talking in retrospect rather than in prospect. Ex post it is all neat equations. Ex ante its a baffling jungle. You don't know even IO theories that will explain monopoly prices, although in principle there are billions. Stigler II is not a tautology, however: it is a working hypothesis, and can be refuted (rec 22, 12/47).

### **Friedman and Stigler on Economies of Scale**

In the mid-1950s both Friedman and Stigler published pieces on economies of scale.<sup>21</sup> However, their discussions of the topic had begun several years earlier. While we don't have their full correspondence on economies of scale there is evidence from the letters that we do have that suggests there was a lot of cross-fertilization of ideas going on. Both were interested in how one could appropriately measure economies of scale. Stigler's interest stemmed from his work on monopoly and the optimum size of the firm, while Friedman's interest, it appears, was piqued by Stigler. Both eventually made contributions to the topic, although Stigler's is better known. Friedman demonstrated the fundamental reasons preventing measurement of optimum firm size using cross-section accounting data. Stigler, apparently convinced by his exchanges with Friedman, championed the survivor technique as a better method for determining optimum size.

In November 1947 Friedman wrote to Stigler "I shall try in the next few days to ...think about your proposed procedure for studying the economies of scale." (rec 13, 11/47). In a follow-up letter (not found) Friedman wrote of concerns about economies of scale in general. Stigler replied, "I have also continuously had your feeling that all is not right" (rec 20, 12/47). He proceeded to answer some "points" that Friedman had made:

1. If the firm expands from  $x$  to  $2x$ , average profits fall from  $.01x$  to  $.0075x$ ; total profits rise from 1.5 to 1. ...
  - a. Ought the firm to expand? Sure, on these facts.

---

<sup>21</sup> See Milton Friedman, "Comment on 'Survey of the Empirical Evidence on Economies of Scale,' by Caleb Smith." In *Business Concentration and Price Policy*, pp. 230-38. A Report of the National Bureau of Economic Research. Princeton: Princeton University Press, 1955 and George Stigler, "The Economies of Scale." *Journal of Law and Economics* 1 (October 1958): 54-71.

- b. Is the decision to expand to be interpreted as an increase in size despite private (dis?)economies of scale? Yes, despite diseconomies.

This industry is making a non-competitive return. Let it be so. Then either (1) small firms enter and produce  $x$ , and wreck the big firm, or (2) small firms expand to  $x$  and do the same, or (3) nobody but the firms at  $x$  can expand, and of course do. But my ratio still traces out economies, and answers the question: will trust-busting raise prices or costs? - and in the negative.

2. You ask whether economies of scale accrue to stockholders or managers or promoters? They may, of course, accrue to consumers or suppliers of resources also, but this isn't important. If they accrue to firms in the industry, I take it that your problem is that they may show up as costs - excessive salaries, or excessive investment on which interest is reckoned. Re salaries: they are not important enough to be of any significance....
3. Small firms have non-competitive wage rates for tax purposes. McConnell (S.C.B.) meets this by calculating the same salary for officer-owned corporations as found in a sample of non-officer owner corporations. This strikes me as crazy; it would be preferable to calculate interest this way and attribute the residual to differences in entrepreneurial ability -- which, however, would be a little question-begging on economies of scale. At the moment I'm stumped, but say that it is not a problem with firms selling 2-5 millions a year (rec 20, after 12/6/47).<sup>22</sup>

Stigler continued:

I grant that my economies border on profitability. But only in a stationary analysis could this be avoided. A firm is more efficient if it rides a cycle better, if it introduces new techniques sooner, if it gets its labor cheaper, if it operates at a steadier output, if it sells at higher prices (at least for the same goods), etc. Even if I could get a stationary, text-book long run average cost curve I wouldn't want it for policy purposes. If my firms differ in product structure, and the big firms also have monopoly power, then I concede that I underestimate their relative costs - a bias I can stand if it doesn't produce falling average costs (rec 20, after 12/6/47).

The topic was discussed in at least two other letters in late 1947. Stigler wrote:

On the economies of scale, I shall want to think about your argument, which carries a certain plausibility. But I find it hard to fit into your scheme, (1) the fact that US Steel was formed to extract monopoly

---

<sup>22</sup> The reference is to J.L. McConnell, "Corporate Earnings by Size of Firm." *Survey of Current Business*, May 1945.

profits, (2) your theory emphasizes the hereogeneity of resources, the jigsaw in contrast to the Marshallian view of economic life, and I am not prepared to concede the importance of these phenomena. Suppose I take the position that the urge for monopoly profits outweighs other factors in bringing forth large firms? This may, in fact, would get me into further trouble; anyway I'll think about it (rec 22, before 12/21/47).<sup>23</sup>

A few years later, in early 1951, Stigler asked Friedman to give a "paper on conceptual problems in the measurement of private and social economies of scale" at the spring 1952 NBER conference on Business Concentration and Price Policy for which Stigler was chair of the program committee. According to Stigler, "This [measurement of economies of scale] is a bewildered area of economic research, and I know you have done some thinking about it" (rec 31, 1/15/51). Friedman replied, "The answer you expect [to your letter of January 15<sup>th</sup>], of course, is the right one, 'no.' I have been wasting too much time doing nothing" (rec 32, 3/2/51). However, Stigler prevailed on Friedman to be a discussant at the conference and Friedman provided lengthy remarks to Caleb Smith's "Survey of the Empirical Evidence on Economies of Scale."

In his published remarks Friedman explained the conceptual difficulties for drawing conclusions about optimum firm size from cross-section data.<sup>24</sup> Even when there are no specialized factors of production measured costs are difficult to interpret because of differences between historical costs and reproduction costs and because of differences in how the capital market values assets as mistakes are made or ownership changes. When specialized factors exist there is the added problem of determining their opportunity costs. He concluded,

It may well be that a more promising course of information than cross-section accounting data would be the temporal behavior of the distribution of firms by size. If, over time, the distribution tends to be relatively stable, one might conclude that this is the "equilibrium" distribution and defines not the optimum scale of firm but the optimum distribution. If the distribution tends to become increasingly concentrated, one might conclude that the extremes represented mistakes, the point of concentration the 'optimum' scale;... None of this can be taken for granted; it would have to be established by study of the empirical circumstances of the particular industry (Friedman, 1955, p. 237).

---

<sup>23</sup> The Marshallian view to which Stigler refers may be Marshall's statement quoted by Stigler in "The Economies of Scale," (1958), p. 56, in which Marshall likens economies of scale to survival of the fittest or this could refer to the contrast between Marshallian industries and the heterogeneous products of monopolistic competition.

<sup>24</sup> Friedman "Comment: Survey of the Empirical Evidence on Economies of Scale by Caleb A. Smith," *Business Concentration and Price Policy: A Conference*. (Princeton: Princeton University Press (for the NBER), 1955). Pp. 230-238.

It is clear that Stigler had come to the same opinion. In his introduction to the published version of the conference papers<sup>25</sup>, regarding Caleb Smith's paper Stigler wrote,

Smith shows how difficult the problems of measurement are, even though he does not emphasize the point (the valuation of inputs) that I find most troublesome. Difficulty is not an adequate reason for abandoning a problem, but I think there are some positive reasons for determining economies of scale from changes in concentration over time rather than using economies to explain concentration. That is, those firm-sizes whose outputs are growing relative to the industry may be interpreted as having the lowest (private) costs. All comprehensive definitions of economies of scale ultimately imply that firms with the lowest costs prosper relative to other sizes of firms, and it is desirable to recognize this explicitly by defining the most efficient size as that which grows relative to other sizes (Stigler, 1955, p. 8).

Stigler's discussions with Friedman also influenced the revision of his textbook on this topic. In his 1946 edition Stigler noted a few problems with using interfirm comparisons (for example, heterogeneous outputs and differences in transportation costs when firms are separated geographically) (p. 207). However, he was confident enough in cross-section data to refer the reader to studies that used cross-section data and to conclude that the available evidence suggested that L-shaped long-run average-cost curves were typical.

The comparable section in the revised edition of his text (1952, pp. 142-145) is very different. While Stigler again explained some of the problems of measuring long-run average costs, the problems mentioned are mostly those that either appeared in his letters to Friedman or in Friedman's remarks to Smith. For example, Stigler mentions the problems with historical versus current market costs, issues with the return to investment, and even McConnell's treatment of salaries. He concluded, "These few remarks are intended only to show that direct measures of the comparative efficiency of different sizes of firms are difficult to make and that considerable ambiguity attaches to almost all existing direct measures.... The only all-embracing test of its efficiency, then, is its survival (Stigler, 1952, p. 144).

By 1954 Stigler had written a draft of a paper, "The Economies of Scale (rec 40)", which he forwarded to Friedman in late 1954 (rec 39, 11 or 12/54).<sup>26</sup> In the draft and in the revised published version of the paper he described more fully "the survivor technique" for determining optimum firm size and credited Mill and Marshall as the original proposers of the technique. In explaining why direct measurement of economies of scale won't work, he referred the reader to Friedman's remarks on Caleb Smith's paper. Stigler wrote:

---

<sup>25</sup> Stigler, "Introduction," *Business Concentration and Price Policy: A Conference*. (Princeton: Princeton University Press (for the NBER), 1955). Pp. 3-14.

<sup>26</sup> We have not found Friedman's comments on the draft.

The comparisons of both actual costs and rates of return are strongly influenced by the valuations which are put on productive services, so that an enterprise which over- or undervalues important productive services, will under- or overstate its efficiency.... The host of valuation problems are accentuated by the variable role of the capital markets in effecting revaluations and the variable attitudes of the accountants toward the revaluations (Stigler, "The Economies of Scale," JLE, 1958, p. 55).

The footnote at the end of this paragraph reads, "These problems are discussed by Milton Friedman in *Business Concentration and Price Policy*, pp. 230 ff. (1955)."

### **Stigler and Friedman on Utility and Demand Theory**

Friedman wrote a personal remembrance of Stigler following his death in 1993.<sup>27</sup> There he quoted Stigler as being fond of saying, "Milton wants to change the world; I only want to understand it." This difference in approach is evident in the lively exchange of letters on Friedman's interpretation of Marshall's demand theory and Stigler's interpretation of the history of utility theory. In June 1948 Friedman sent to Stigler the first draft of "The Marshallian Demand Curve." At the same time Stigler was completing *Employment and Compensation in Education* and beginning to read Gossen and Dupuit to prepare for writing "A History of Utility Theory."<sup>28</sup> Stigler was skeptical upon his first reading of Friedman's reinterpretation of Marshall, both historically and practically. He wrote:

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. ...

In sum: If on further reading and reflection you agree with me that Marshall didn't mean this, I still think that this note – which is ingenious and beautifully lucid – should be published, although then as what Marshall should have done. But even here more work is called for: do you really think this is a good type of demand curve? (rec. 135, June 21, 1948)

Friedman was unable to clear away Stigler's skepticism, so that when Stigler sent a draft of the portion of his history of utility theory dealing with Marshall

---

<sup>27</sup> Friedman, "George Stigler: A Personal Reminiscence," JPE, Vol. 101 (October 1993): 768-773.

<sup>28</sup> *Employment and Compensation in Education*, New York: NBER, 1950, and "The Development of Utility Theory," JPE 58 (August and October 1950).

he followed the “anti-Friedman” (rec. 139, December 5, 1949) orthodox interpretation. ...**To be continued...**

**[Their arguments over Marshall continued in at least seventeen letters over three years. These letters require detailed editing, which we have not yet completed.]**

### **Conclusion**

What have we learned from the correspondence covered thus far about the development and practice of price theory by Friedman and Stigler? The letters reveal that for these two price theorists history of economic doctrine had practical value. Stigler coupled the two from the beginning of his career. His Ph.D. thesis on the history of economic doctrine and his price theory text were published within two years of each other.<sup>29</sup> His quip to Friedman in the midst of their argument over point and money rationing schemes that “now I must leave the formal playthings of the Chicago school and turn to the hard realism of reading some history of doctrine” (rec 11, 1/31/47), makes a distinction in jest that was hardly there in reality. Friedman did not have a reputation as a historian of doctrine when he wrote “The Marshallian Demand Curve,” the first of his publications to have significant historical content. Friedman had not read Marshall to learn about the history of economic theory but to learn how to *do* economic theory. For him there was no real distinction between theory and its history. One simply used the best theory available to tackle problems of economic analysis. This might be theory recently developed; it might be theory developed a half century ago; or it might be theory that one has to develop as one goes along. It was all the same for him.

We also observe that their scholarship and teaching were bound together. Stigler had just expanded his textbook, *The Theory of Competitive Price* (1942) into *The Theory of Price* (1946), when Friedman began preparing to teach Economics 300, Price Theory, at the University of Chicago. Friedman opened their argument about proofs of the Law of Diminishing Returns by reporting, “As you know, I have been reading Stigler to prepare for teaching; I have been also reading Marshall” (rec 128, 8/12/46). Homework problems for their classes, which they passed back and forth, took on a life of their own, crowding out time that could have been devoted to scholarship, if they were inclined to make such a distinction. But they were not.

So a willingness to carefully read and draw from the knowledge of earlier economists was paramount for both of them as they developed price theory, either for articles or for classes. In their reading they questioned even the smallest detail. If they couldn't understand something, they worked on the problem until they could make sense of it or improve it. Then they wrote down the improvement or clarification for each other's deliberation and eventually for publication. Thus, it appears that many of the contributions for which Friedman and Stigler are known did not arise from their quest to discover

---

<sup>29</sup> *Production and Distribution Theories: 1870-1895*. New York: Macmillan, 1941, and *The Theory of Competitive Price*. New York: Macmillan, 1942.

something new so much as from a deep passion for understanding theory as it was passed on to them.

Why did both economists eventually win Nobel prizes? One factor may be that each had the other as a staunch friend and supporter. Over the years they were apart, they worked to keep their friendship strong and were generous to each other with their time. Despite the burdens of teaching, writing, and raising a family, each read each other's papers with a keen eye. Their letters are filled with hard, challenging criticism, but complements abound as well. It is clear from the correspondence that each wanted the other to succeed; neither believed that success in the field of economics was a zero-sum game. As Stigler once wrote to Friedman in the midst of their long debate over the correct interpretation of the Marshallian demand curve: "I hope you understand that I want you to be right. If you are right, it is a very pretty triumph over the field. Since I will be wrong with everyone else, it will be no personal reflection on me. So our vested interests are identical" (rec 8, Spring 1951).