

Measurement, Incentives, and Constraints in Stigler's Economics of Science

Forthcoming in:

The European Journal of the History of Economic Thought 12, no. 4 (Winter 2005).

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Last revised: July 29, 2005

Short biographical note: Diamond is Professor of Economics at the University of Nebraska at Omaha. He received graduate degrees in philosophy and economics from the University of Chicago. Most of his publications have been in the areas of labor economics, the history of economic thought, and the economics of science. His current research interests include the economics of science and technology, and Schumpeter's process of creative destruction.

ABSTRACT

George J. Stigler's seminal role as one of the founders of the economics of science is summarized and evaluated. His main contribution rests in his asking an array of important questions, and arguing persuasively for the application of empirical and especially statistical, techniques to the answering of those questions. He asks whether and how science progresses; whether a scientist's biography is important in understanding his science; what characteristics of a scientist are most complementary to success in science; and how the professionalization of science redirects the attention of scientists more toward internal puzzle-solving, and less toward applied relevance.

Key words: Stigler, science, citations, economists, sociology

1. Introduction

. . . sociology puts its imperialistic title on this area of study only on the ground that sciences are practiced by human beings and therefore involve social behavior. In the same sense it would be possible and equally meritorious to describe as the economics of science the economic organization and evolution of science. (George J. Stigler, "Does Economics Have a Useful Past?" 1982c: 112)

George Stigler (1911-1991) was devoted to science and to economics. He once joked that every morning he composed ". . . a stanza of my romantic epic, *Science is a Boy's Best Friend*." (1984: 34) Stigler did not bother to enter the debate of whether economics was a science, perhaps because of his general belief in the fruitlessness of methodological discussions. But the view that economics is a science permeates his work, e.g., his discussion of himself in an autobiographical essay flows naturally from comments about scientists in general, to that of economists in particular, then himself most particularly (1986: 93-95 and passim).

He noted (1982c: 112) that the scientific study of scientists has been mainly undertaken by sociologists rather than economists--there is an organized subfield of sociology called "the sociology of science," but no organized subfield of economics called "the economics of science." Stigler thought that the void was unfortunate and he spent a significant amount of his time and energy helping to fill it.¹

This is not inconsistent with his referring to, and making use of the work of sociologists, especially his former Columbia University colleague Robert K. Merton (e.g.: Stigler 1982c;

1980; 1983: 535). We would expect Merton's work to appeal to Stigler for several reasons. One is that Merton frequently discussed the importance of economic constraints in influencing the behavior of scientists and science. In Merton's dissertation on science (1957, 1970) he suggested that the choice of problems to study in science may be influenced by economic considerations, but that such influence need not extend to the substantive content of the science produced. In later work, Merton emphasized the competition for priority as motivation for scientists (1973c), an argument that many economists find congenial. A second reason why Stigler might have found Merton's work congenial is that Merton valued (and performed) the same sort of broad statistical work in science studies that Stigler advocated. Finally, Stigler seems to have found a kindred spirit in Merton's wry and biting wit (e.g. Merton 1985).

Some evidence of the relationship between Stigler and Merton can be gleaned from their correspondence, which is currently partly available, with permission, in a folder of the Stigler archives at the Regenstein Library at the University of Chicago.² In general, from the letters in the folder, one senses mutual affection, wit, and respect, although Merton's letters are generally longer and more effusive than Stigler's.³ In a brief Stigler response to a much longer Merton letter on the sociology of science, Stigler writes (6/22/72) that "in this great area I prize your opinion above all others and shall cherish your letter." A later letter from Merton (2/14/83) graciously praises Stigler's Nobel Prize lecture (1983): "Surely never before has the case for the history and sociology of science been as instructively and gracefully made as in your Nobel lecture - - and surely, never before on so august an occasion."⁴

The “august occasion” to which Merton refers was the awarding of the Nobel Prize to George Stigler in 1982, which was made on the basis of many important contributions to economics. Several articles have appeared that summarize aspects of these contributions; in addition to praising his wit, or his character as a colleague, editor or mentor. One important source for several of these articles is the October 1993 issue of the *Journal of Political Economy*, which contains articles on Stigler by Becker, Demsetz, Friedland, Friedman, Peltzman, Rosen, Rosenberg, Sowell, and Wallis, as well as a bibliography of Stigler’s publications compiled by Longawa. Moss edited and arranged another very useful collection of briefer observations on Stigler in the July 2002 issue of the *American Journal of Economics and Sociology*. The contributors include Friedland (who is listed as the “first-author” of this compilation of comments), Goodwin, Claire Hammond, Daniel Hammond, Levy, Medema, Naples, Samuels and Stephen Stigler. McCann and Perlman have also published a more detached, though mainly complementary, review of Stigler’s contributions.

Although Stigler and his work have earned the careful attention of several able scholars, one important aspect of his work has so far not been afforded the attention it deserves: his contributions to the economics of science.

2. Stigler's contribution

If we consider Stigler's contributions to the economics of science broadly, then we will include, as he would have, much of his work on the economics of economics. Traditionally, much of this work has been classified as being part of the history of economic thought. But it

differs from traditional history of economic thought in that it seeks to address general theses about how science does, or does not, advance.⁵ For example, Stigler's "Does Economics have a Useful Past?" (1969) traditionally has been interpreted as arguing that the history of economics field is not worth pursuing. But I think the deeper argument of the paper is that history of economic thought is not worth pursuing for the traditional reasons, but is worth pursuing if it is seen as providing a database for testing important hypotheses in the economics of science.

Almost all of Stigler's contributions in this area are empirical in one way or another. He often uses basic economic theory to suggest hypotheses, which he then tests, characteristically with one or more well-wrought case-studies. In addition to case studies testing important hypotheses, he also advocated, and sometimes produced, systematic statistical studies related to hypotheses in the economics of science.

Stigler asks, and provides evidence on, many of the "big questions" in the economics of science. He asks whether science progresses, answering "no" in his retrospective kinky demand curve article (1978), and "yes" in his Nobel lecture (1983). He asks (1982j) whether the biographical constraints of a person's life influence their science, answering "no" (but with a lot of qualifications). He asks whether events influence science, answering that events may influence the problems studied, but have less influence on the conclusions reached about the problems (1965b). He asks about the characteristics of the successful scientist and he concludes that the successful not only possess originality, but also possess conviction and energy in repeating and exaggerating claims (1965: 5-6).

Listing Stigler's contributions to the economics of science would seem a straightforward, if time-consuming, task of culling his writings for relevant theses, hypotheses and empirical evidence. The task is harder than it first appears, however, because Stigler frequently makes inconsistent claims on important issues. Partly this is because, over a long and productive life, he sometimes learned something, and learning something implies occasionally changing one's mind. The inconsistencies may also be partly due to his sharing the economist's well-noted tendency to see the arguments both on the one hand and then on the other hand. Finally, some of his inconsistencies may be due to his occasionally writing as devil's advocate---for the fun of it and as a slap at sleepy complacency. What Stigler said of Viner⁶ can also be said of him:

It is not quite so easy to handle Vinerian mischief, but when one recognizes that it is fundamentally a serious attempt to cope with intellectual complacency, one can at least try to keep out of its way. (1963: 23-25)

Stigler would probably not be too bothered that we find it hard to position him on several key issues. In discussing his relationship with his own mentor, Frank Knight⁷, he suggests that Knight's legacy to his friends and students was one of fundamental intellectual values, rather than specific propositions, or even methods (1986: 95-98 & 110). (E.g., Knight was highly skeptical of the statistical techniques that Stigler championed.) Stigler tells us that ". . . the prospects for scientific progress would be bleak if we could train our students to become truly faithful disciples." (1986: 110)

3. Stigler on the aims of scientists and the efficiency of scientific institutions

One key issue of the economics of science is the question of what scientists are after. Stigler's official position, inferred from his controversial paper with Becker "De Gustibus Non Est Disputandum" (1977), would be that everyone has the same utility function, scientists included. The controversial proposition is that all human values are universal.

But elsewhere, as in his tribute to Knight, Stigler writes as if scientists, or at least some scientists, are different. The tribute to Knight reveals what, in Knight, Stigler found most admirable and most worth emulating:

One great source of his influence was the purity of his devotion to the pursuit of knowledge. Frank Knight transmitted, to a degree I have never seen equaled, a sense of unreserved commitment to the truth. This harsh mistress must be served even when the service was dangerous or painful. No authority was too august to challenge: Knight would not hesitate to tell Gabriel if his horn needed tuning. No contemporary passion was so powerful that it could escape critical scrutiny and usually denunciation. One must quarrel even with an esteemed colleague who was entertaining mistaken views. The compromises of expediency were simply alien to the world of this scholar: thus, it would be an absurd question to ask to which political party he belonged, for neither possessed a scrap of him. (1973a: 518-519)

If Stigler had as eloquent a student as Knight had, he would say much the same of Stigler as Stigler said of Knight.

Another issue on which Stigler expressed differing views over his career concerned the extent to which economists ought to advocate policy positions and institutional reforms. Much of

the time, again emulating Knight, he advocated the scholar's detachment from practical affairs (see Friedman 1993). In "Do Economists Matter?" he even argued that economics had not had an effect on government policy (1982b). But that did not stop him from commenting on policies he viewed as misguided. One famous example was his early pamphlet with Milton Friedman arguing against the rent controls in post-World War 2 cities in the United States (Friedman and Stigler 1946). In another well-known example, he argued against minimum wages,⁸ both early and late in his career (1946; Raisian and Stigler 1988).

More specifically related to science, Stigler believed that scientific institutions were competitive (especially in the United States) and that competition would identify the best theories and the best scientists, again, especially in the United States (1988b: 85-86). In his *Memoirs* (1988) he makes general reference to the work of Chicago sociologist Joseph Ben-David as providing important evidence on the competitiveness of science, and in his earlier "Does Economics Have a Useful Past?" (1982c: 117) he specifically cites Ben-David's study (1960) of American medical research. There Stigler admits in a footnote: "I find the conclusion more congenial than the evidence---the international differences in number of medical discoveries (Ben-David's key dependent variable) can perhaps be better explained by number of holders of medical and related degrees." So perhaps Stigler's belief in the efficiency of scientific institutions was based more on casual empiricism than on the sort of systematic evidence that he valued so highly.

He believed that elites in academics had earned their status through a competitive process where merit mattered:

I don't think that the successes achieved by the graduates of the major schools are due simply to an old-boy network. Anyone who does outstanding work is in strong demand even if, . . . , he has few other redeeming traits. (1988b: 36)

He was clever at dealing with apparently contradictory evidence. Historians of economic thought can identify many economists who "got it right" before those who are given credit for the discovery. Stigler's son, Stephen, has even elevated this observation to a law: stating that no law in science is ever named after the scientist who discovers it (1980, 1982). Stigler himself, in his paper on Merton's multiples, gives seven examples of discoveries in economics for which the original discoverer was not recognized by contemporary economists. Commenting on these cases, and the famous case of Gossen's anticipation of marginal utility theory in particular, Stigler says:

If an earlier, valid statement of a theory falls on deaf ears, and a later restatement is accepted by the science, this is surely proof that the science accepts ideas only when they fit into the then-current state of the science. Gossen, writing in the high tide of German Historical Economics, was simply inappropriate to his scientific environment. Longfield in Ireland, and von Thunen in Germany, were presenting a marginal productivity theory for which neither German nor British economic science was ready. And similarly for Slutsky, Cournot and other unsuccessful discoverers. (1982h: 102)

Such evidence seems inconsistent with Stigler's claim that merit is rewarded in science. George Stigler's response is that to be deserving of scientific merit, one needs not only to discover truth, but to exaggerate it and sell it with great energy. If a scientist fails to "sell" his contribution, then

it is the scientist's fault, not the fault of the scientific institutions. Notice that Stigler's proposition that the elite always chooses merit (as contrasted with old-boy connections) is strictly circular, because (when push comes to shove) merit for Stigler is defined as acceptance by the elite. (If you fail to convince the elite, well that's because you lacked energy or salesmanship, not because the elite failed to recognize and reward a scientific advance.)

It is well known (or at least widely believed) that distinguished scientists will be successful at publishing lower quality work than lesser-known scientists.⁹ Merton has called this, and related phenomena, the Matthew Effect (1973a). This would seem to be a failure of scientific institutions. Yet I once heard Stigler say that the journal editors were behaving optimally because even the mistakes of a great mind are worth reading and understanding, because they provide evidence of how a great mind thinks.

Stigler admits the cliquishness of many academic institutions. He quotes a wonderful passage of Babbage's, the conclusion of which is that anyone of merit "has a fine chance of being black-balled" for membership in the Royal Society. Stigler's response to the passage is:

As I understand Babbage's main (unoriginal) contribution to the subject, it is the assertion that learned bodies are each run by a self-perpetuating clique. I believe that this is true, and necessary to their survival. Private property not only turns sand into gold but also turns committee meetings and journal editing into careers. Babbage's violent dissatisfaction with this state of affairs is reminiscent of Ambrose Bierce's definition of the word *incumbent*: "A person of the liveliest interest to the outcumbents." (1969: 118)

To this "outcumbent," the final sentence has a somewhat smug flavor to it, implying that envy is the only motive for criticism of self-perpetuating cliques. And what does the "private property" sentence mean? Do incumbents hold property rights in professional organizations and journals? And in academic institutions, where entry is limited by massive government subsidy of the incumbents, do property rights in the incumbents' institutions necessarily turn academic sand into gold (at least if gold is measured socially in terms of the advance of truth, rather than personally in terms of achieving tenure and salary increases)?

Stigler argues that the reason scientists examine new theories much more carefully than old ones is that the longevity of the old theories implies that if they were easily refuted, someone would have received great economic rewards from having refuted them. But elsewhere Stigler points out that the profession for long periods of time persisted in believing stylized facts that were false. Sometimes this even occurred in cases where the alleged fact had been decisively disproven by a well-known and articulate economist (as in the case of the kinky demand curve).¹⁰ Were the institutions performing optimally then? Did the survivorship principle that underlies competition in the case of firms, apply here as well? (Or does the detachment of academic funding from results, produce significant slack, as has been believed by Adam Smith and others?)

And is Stigler's witty, usually gentle¹¹, criticism of the over-mathematization of economics consistent with a view that scientific institutions are competitive, and efficient, and optimal? In one of my favorite passages, Stigler complains:

. . . as professional economics becomes more complicated and its practitioners use an increasingly more formidable apparatus, there seems to have been retrogression in the

ability of economists to communicate with other intellectuals. Less than a century ago a treatise on economics began with a sentence such as, "Economics is a study of mankind in the ordinary business of life." Today it will often begin, "This unavoidably lengthy treatise is devoted to an examination of an economy in which the second derivatives of the utility function possess a finite number of discontinuities. To keep the problem manageable, I assume that each individual consumes only two goods, and dies after one Robertsonian week. Only elementary mathematical tools such as topology will be employed, incessantly." (1984: 153)

Of course there are counter-examples to Stigler's aversion to institutional reform: for instance, he implemented a modest reform in the management of the *Journal of Political Economy* by initiating a scale of payments for referee's reports based on speed of submission of the reports.¹² (A deeper analysis of the incentives of the scheme might have revealed that rewarding one dimension of output (speed) but ignoring another (quality) would result in over-production of the rewarded dimension relative to the unrewarded one.)

Stigler also provided tools that might be used for institutional reform, even though he did not himself use them for that purpose. For example, he developed a clever method for estimating economies of scale (and hence the optimal size) for firms (e.g., 1958). A potentially fruitful direction for future research would be to apply this technique to the question about the optimal size of departments. The debate on this issue was especially active in Great Britain where there was a movement to consolidate departments on the grounds that in the past, significant economies of scale had gone unexploited (see, e.g., Dickson 1989). The consolidation of departments did

not occur, perhaps due to the persuasiveness of arguments that small departments were productive.¹³ Just as Stigler found a range of firm sizes that survive over time (and hence are judged equally efficient), so there may be a range of academic apartment sizes that are equally efficient.

Stigler's brief and suggestive study of the impact of private foundations on economics (1967) also might be applied to institutional reform. In that admittedly "impressionistic" study, Stigler concludes that the large foundations have advanced the research of non-mathematical research innovators. If such a conclusion was confirmed by the sort of systematic research that Stigler advocated, then Stigler's study might serve as the partial inspiration for an argument to reduce government expenditures on science that may be crowding out more efficiently spent private foundation expenditures on science.

4. **Statistical studies**

Stigler's systematic statistical studies in the economics of science include his early National Bureau of Economic Research (NBER) monograph with Blank on *The Demand and Supply of Scientific Personnel*, as well as the better-known applications of statistics to the development and practices of economists (both on his own and in work with Claire Friedland).

In their 1957 NBER monograph, Blank and Stigler state the policy conclusion of their data-rich study in the following terms:

In a broad way it may be stated that there cannot be a serious problem of supply of faculty in institutions of higher education. For the very presence of a much increased demand

(that is, a much increased student body) carries with it a much increased supply of trained individuals. Indeed there has been much more concern with the problem of finding appropriate employment for a rapidly increasing population of highly trained persons than in finding teachers to train them. (1957: 103)

In his *Memoirs* Stigler said of the early NBER work that: "It was not exciting." (1988b: 71) (It is not clear whether his restrained evaluation refers to the labor he put into the research or to the output of the labor.)

Stigler's "Statistical Studies in the History of Economic Thought" (1965d) was one of the pioneering studies in the systematic statistical analysis of scientific institutions. His paper addressed several issues, including how the economics discipline changed when professional economists replaced amateur economists in the discipline. He argued that when economics was done by part-time gentlemen scholars, the work was usually applied and policy-oriented. As economics came to be done by professional academics, holding endowed chairs, it became less responsive to policy demands from the outside world and more responsive to internal theoretical puzzles.

In his first citation paper with Claire Friedland, "The Citation Practices of Doctorates in Economics," (1982a) the authors use citations to examine whether there is evidence for distinct schools of thought in economics. They conclude that: "Aside from the penchant of doctorates in citing their own faculty, . . . , it is difficult to find systematic differences among the schools in citation practices." (1982a: 207) The article also includes some other interesting and useful

findings, such as the positive and significant relationship between the quantity of articles published and the number of citations received.

The second citation paper with Friedland, "The Pattern of Citation Practices in Economics," (1982b) addresses a more eclectic set of issues. They again regress citations on number of articles, but this time their estimated coefficients are insignificant. They conclude that, due to the low R-squared, the quality of publications plays a much greater role in determining the number of citations received than does the quantity of publications.

Stigler (1982g) also used citation analysis to see if his refutation (1947) of the kinked oligopoly demand curve reduced later use of the curve (he found that it did not). Another finding is that most economics articles do not make any substantive contribution to new knowledge. He suggests that the research leading to these articles has a different purpose: to make the researcher a better teacher by keeping him up-to-date in the latest methods and substantive developments in the field.

5. The place of economics of science in Stigler's life work

Chicago economists of Stigler's era believed that to understand human behavior we should look at what people did, not at what they said about what they did. So in understanding Stigler's views about science, it is instructive to look not just at what he said about doing economics, but at how he did economics.

Stigler believed that what is important in a scientist's work is what his peers viewed as important, so we focus on the 20 most highly rated articles in terms of citations. In the spirit of

Stigler and Friedland's pioneering citation studies, I present in Table 1 Stigler's 20 most highly cited articles that were published after 1955. The source of citation counts are the total citation counts given in the Web of Science source published by the Institute for Scientific Information (ISI). The publications covered in the Web of Science extend back through 1956, several years earlier than ISI's print volumes of the *Social Sciences Citation Index*.¹⁴ Citations to Stigler's books are not included. Thus the counts in Table 1 may be biased downwards for articles that were reprinted in collected volumes, since citations to the volumes would not be counted in the article's citation count. Other limitations of the citation counts provided in the Web of Science, are discussed in Diamond (2004).

Are there broader lessons for the doing of economics of science that can be extracted from what Stigler did in his opus, as represented by the 20 important works listed in the table? On a few issues, Stigler's views dramatically changed over time---most notably in his shift from defense of antitrust to suspicion of it. But in many respects there is a consistency in the intellectual values embodied in Stigler's work. Stigler did not generally argue systematically for a methodology of economics. But he did make his bets and his preferences known, and these were generally exemplified in the work that Stigler did.

I will argue that all of the important 20 articles exemplify one of two broad themes in Stigler's work. The first theme is that measurement is of central importance in advancing our knowledge of human behavior.¹⁵ The second is that a wide range of important actions can be understood as responses to incentives and constraints.

More specifically, I suggest that each of the important 20 articles can be grouped under one or more of the following four sub-themes:

1. attempts to measure what had not been measured, (or to measure better).
2. calls for more measurement
3. attempts to theorize based on incentives and constraints
4. calls for theorizing based on incentives and constraints

In the first column of Tale 1, I categorize each article by sub-theme, with ‘m’ standing for ‘measurement’ and ‘i’ standing for ‘incentives.’ A ‘1’ following a letter indicates ‘doing’ while a ‘2’ indicates ‘advocating.’ So the ‘m-2’ for “The Statistics of Monopoly and Merger” indicates my judgment that Stigler in that article was primarily advocating better measurement of the effects of monopolies on consumer well-being.

For the reader who would like more information on what is important in each publication, I have provided in an appendix to this paper, a “Reader’s Guide” to the 20 papers, usually indicating what it was about each paper that led me to classify it as I have in the table. The chronological order of Table 1 allows us to see if there are any obvious patterns of change of emphasis on his two main themes. What is clear is that throughout the nearly 30 years of publications represented in the table, both main themes are always present.

On the dust jacket of the first edition of Stigler’s *The Economist as Preacher* (1982a) is a photo of Stigler dressed in academic garb, with a mischievous grin on his face, peering out from the pulpit of Rockefeller Chapel, as if to begin a sermon. The grin is undoubtedly because Stigler in the book argued against most preaching. He believed the primary goal of the economic

scientist was not to advocate, but to understand. Stigler believed that generally, doing was more useful, though harder, than preaching. So in examining the 20 top articles in Table 1, it is edifying that of the 11 articles mainly on measurement, nine primarily do measurement, while only two primarily advocate that somebody do measurement. Similarly of the nine articles on incentives, it is edifying that six primarily theorize in terms of incentives, while only three primarily advocate that somebody propose a theory of some phenomenon in terms of incentives.

If Stigler's two themes are applied to the study of science, they would imply that first and foremost, we should collect statistical evidence on the behavior of scientists and scientific institutions, and secondarily, we should explore how much of the behavior of scientists can be understood as responding to incentives and constraints.

It may be noteworthy that among Stigler's 20 most highly-cited papers, only one, "Citation Practices" (1975) is directly a major part of Stigler's contribution to the economics of science. One may infer that, at present, Stigler's work on science does not weigh heavily in the esteem of most economists.

Many other Nobel-prize winning economists have written papers on the history of economic thought, and on economic method, if not directly on what may be called the economics of science. Some of them may be motivated by the sentiment often attributed to Churchill that "history will be kind to me for I intend to write it." But Stigler's interest was longer and deeper than that, as evidenced by his having written his dissertation on a topic in the history of economic thought. I speculate that another difference would be that Stigler's work in history of economic

thought is taken much more seriously by specialists in the history of economic thought, than the more casual history written by most other Nobel Prize winners.

5. Stigler's students

In his *Memoirs* Stigler says that: "An important way, if not the most important way, in which one influences a field is through one's students." (1988b: 35) In the literal sense of "student," Stigler has certainly affected the work on economics of science of Chicago students such as David Levy (e.g., 1988), James Adams (e.g., 1990) and myself (e.g., 1986, 1988). In the figurative sense, a wide group of those working in the area have found his work stimulating and useful (e.g., see Bronfenbrenner 1976; Rosenberg 1993; and Coase 1994).

Sometimes his support was financial, as during the summer when he provided me a small grant from his Walgreen funds in order to hire (and closely supervise) students to count citations for my study on the economic worth of a citation (Diamond 1986). More commonly his contribution was to ask stimulating and important questions, providing only limited evidence himself, and inviting others to fill in the gaps.

His caustic comments on drafts could be confidence destroyers, but on the other side of the ledger, his presence in the field provided confidence that we are doing something worthwhile. Consider the literature in economics that discusses why people follow herds and pursue fads (Banerjee 1992; Bikhchandani et al 1992; Scharfstein and Stein 1990). Just as investors often follow the herd in their stock picks, so economists often follow the herd in their choice of field of

specialization. In abandoning the herd to choose the 'road less traveled' it was reassuring to see Stigler up ahead urging us on for the sake of important truth and good intellectual fun.

6. Coda

It would be simpler if all of Stigler's contributions to the economics of science fell under one unified heading. The evidence is otherwise. I have argued here that he made three distinct contributions to the economics of science. The first, was through his collection of data on the labor market, publications and citations of scientists; and through his emphasis on measurement in all aspects of his work; both of which point toward the empiricism by which we are most likely to make progress in science. The second, was through his emphasis on incentives and constraints, which points toward the sort of theory that is likely to be fruitful in understanding science and scientists. The third, and arguably most important, was by witty and sometimes provocative essays, to shake us out of our complacency on fundamental questions.

McCloskey suggests (1985) that reading Stigler helps us write clearly. Clarity matters. But what matters more is that reading Stigler helps us to stay focused on important issues, and to remember that the final arbiter of truth in science is evidence.

TABLE 1: 20 Most-Highly-Cited Articles Published after 1955 (Based on Citation Counts Reported in the “Web of Science” as of 7/19/2002)*

Theme ⁺	Year	Post-1955 Articles by Stigler that Were "Most-Cited" as of 2002	Cites	Rank
m-2	1956	“The Statistics of Monopoly and Merger.”	30	20
i-2	1957	“Perfect Competition, Historically Contemplated.”	46	16
m-1	1958	“The Economies of Scale.”	119	10
i-1	1961	“The Economics of Information.”	957	2
m-1	1962	“Information in the Labor-Market.”	246	6
m-1	1962	“What Can Regulators . . . Case of Electricity.” [with Friedland]	124	9
i-1	1964b	“A Theory of Oligopoly.”	314	4
m-1	1964a	“Public Regulation of the Securities Markets.”	94	13
m-2	1967b	“Imperfections in Capital Market.”	44	17
i-1	1970c	“The Optimum Enforcement of Laws.”	205	7
i-1	1970b	“Director’s Law of Public Income Redistribution.”	81	14
i-1	1971	“The Theory of Economic Regulation.”	1152	1
m-1	1973b	“General Economic Conditions and National Elections.”	159	8
i-1	1974	“Law Enforcement, Mafeasance, and Compens. . . .” [with <u>Becker</u>]	252	5
m-1	1974	“Free Riders and Collective Action – Appendix to Theories . . .”	98	12
m-1	1975	“Citation Practices of Doctorates in Economics.” [with Friedland]	38	18
i-2	1976b	“The Xistence of X-Efficiency.”	106	11
m-1	1976a	“The Sizes of Legislatures.”	34	19
i-2	1977	“De Gustibus Non Est Disputandum.” [with Becker]	552	3
m-1	1985	“The Extent of the Market.” [with Sherwin]	65	15

* The letters next to some of the years refer to the ordering of the publications in the bibliography. Co-authors in brackets; underlined when co-author was first-author. When two publications have the same publication year, the publication with the highest citations is listed first.

⁺ Under “Theme” the letter “m” means “measurement” and “i” means “incentives”, while “1” means “doing” while “2” means “advocating.” So “m-1” would be an article that primarily does measurement on the topic of the article, while “i-1” would be an article that primarily gives a theoretical account explaining some behavior in terms of incentives and constraints.

APPENDIX: READER'S GUIDE TO STIGLER'S TOP 20 PAPERS

The Reader's Guide provides my brief assessment of what is most important, or of most current interest, in Stigler's top 20 most-highly-cited papers.

Stigler's earliest (1956) paper on the list "The Statistics of Monopoly and Merger" is a meticulous statistical critique of three papers that had argued that the welfare loss from monopoly was less than commonly thought. The paper is dry, some might even say nit-picking---but illustrates Stigler's high regard for statistical evidence. This is one of the few papers in the top-20 that Stigler never reprinted in any of his collections. Stigler defends the dominant view that monopoly is a significant economic problem, by giving detailed criticism of three papers that presented a revisionist view that monopoly was a less serious problem than had usually been thought. In later years, Stigler changed his view of monopoly, for which he gives some credit to the "heresy" (1988b: 101) of Schumpeter (1942), and more credit to the arguments of Aaron Director on Standard Oil, and the confirmation of Director's arguments in the careful research of Director's student John McGee (1958).

A year later (1957) he published "Perfect Competition, Historically Contemplated" which is part history of economic thought, and part theorizing. Stigler sketches how the concept of "competition" has evolved over the history of economics, and suggests that the concept will continue to be fruitful into the future. Most of the paper is devoted to an historical account of some of the main ways in which competition had been defined and discussed in economics through the mid-1950's. A secondary aim of this sixteenth-ranked article is to argue for the usefulness and robustness of the competition concept, both in economic theory, and as a policy

tool. Stigler does, however, grant that the concept will need to continue to evolve with the advance of economic theory. In particular, he suggests (p. 282) that the concept of competition's "natural affinity to the static economy" will require modification in order to apply competition to a dynamic economy. This discussion calls Schumpeter to mind, but Stigler does not mention him.

"The Economies of Scale" (1958) is arguably the most important of the three papers from the 1950s, putting forward a whole new way to measure a key concept in industrial organization. Stigler suggests that we can learn what scale is most efficient in an industry by looking at what percent of firms of varying sizes survive over time. Stigler's tenth-ranked paper introduced this "survivor" technique for learning the optimal size of firms in an industry. Empirically Stigler found that firms of a wide range of sizes, survived for substantial periods of time. The finding led Stigler to re-draw the usual long-run-average-cost curve. Where previously, the curve would have been drawn with one quantity (scale) at which average costs were at a minimum, Stigler re-drew it so that there was a range of quantities (scales) at which average costs were at equal minimal values. In this range, which could be large, returns to scale were constant.

"The Economics of Information" (1961), a candidate for Stigler's most influential paper, extends standard theory in a simple way to suggest that incentives and costs will determine the optimal level of information that consumers will invest in. This second ranked article contains the contribution that Stigler himself most emphasized in his Nobel Prize lecture (1983): the founding of the field of the economics of information. Prior to Stigler, it was common to assume that information was free and perfect. Stigler pointed out that information was scarce, costly and

imperfect; but that in spite of that, through optimal investment in search, something close to the results of perfect competition will occur.

“Information in the Labor-Market” (1962) is a mainly statistical, empirical extension of the theory presented in the 1961 paper. Number six in rank, the paper is an elaboration and companion-piece to Stigler’s second-ranked (1961) “The Economics of Information” paper. The labor market paper discusses how lower information costs in the labor market result in more efficient matching of employees to jobs, and hence to a more productive economy.

“What Can Regulators Regulate? The Case of Electricity” (1962) with Friedland, is a paper that Stigler continued to like in later years. The ninth-ranked paper, was the early empirical paper that arguably led to the theoretical discussion in Stigler’s (1971) most-highly cited paper, “The Theory of Economic Regulation.”¹⁶ The 1962 paper illustrates that of the two broad themes exemplified in Stigler’s work, the main one is that the empirical evidence matters most. The authors use data on electricity prices during an early period of time when some of the states regulated the industry, and some did not. Specifically, they estimate a regression with the price of electricity as the dependent variable, and including as one of the independent variables, a dummy variable equal to one if the state regulated the industry. The strong expectation was that regulation would lower price, but the regression showed no relationship between regulation and price. Originally Stigler interpreted the lack of effect as evidence that the industry was already acting competitively, so the regulators had nothing to do. After the publication of the paper, Stigler re-interpreted the results as evidence for the “capture theory” of regulation. Stigler stuck with the regression, but changed his interpretation.

“A Theory of Oligopoly” (1964b) can be viewed as an extension of “The Economics of Information” paper. In this fourth-ranked paper, Stigler explains why some oligopolistic industries are more successful than others at price collusion: those that succeed have lower information costs in policing the collusive agreement. Oligopolies will organize to control prices when the costs are low and the benefits high.

“Public Regulation of the Securities Markets” (1964a) is a mainly empirical/statistical paper shedding doubt on the claim that public regulation has benefited consumers in the securities market. The paper examines the effects on investors of U.S. Securities and Exchange Commission (SEC) regulations and is in some ways a companion piece, in time, method, and aim to the slightly earlier (1962) paper on regulation of electric utilities. The thirteenth-ranked paper uses several different empirical measures to find very little difference between the investor-outcomes before SEC regulations, and the investor-outcomes after SEC regulations.

“Imperfections in the Capital Market” (1967b) is a gentle jab at Stigler’s friends and others for glibly assuming, without evidence, that certain capital markets (usually those with high interest rates) are not efficient. Stigler suggests that maybe the markets are imperfect, and maybe not, but that we’ll only know when we find ways to measure the lenders’ risks. Thus Stigler’s seventeenth most highly-cited paper is a modest call for empirical evidence to support the common claim that capital markets are imperfect. One interesting aspect of this paper is that many of the economists skewered in the examples, generally share Stigler’s friendliness to the market (e.g., Friedman, Becker, Machlup, and T.W. Schultz).¹⁷

“The Optimum Enforcement of Laws” (1970c) exemplifies another application of the proposition that basic theory can be usefully extended to understand an important aspect of the world. Stigler’s first contribution to the economics of crime, this seventh-ranked paper makes the sensible recommendation that marginal penalties should increase with the severity (costs) of the crime, in order to discourage miscreants from choosing the more severe crimes. Although seemingly obvious, Stigler includes evidence that in the arena of economic penalties, his recommendation was not being implemented.

“Director’s Law” (1970b) provides some evidence for Aaron Director’s assertion that the members of the middle class are the main beneficiaries of public transfers, but mainly sketches a theoretical explanation for the assertion, in terms of the middle class’s ability and incentives to form coalitions.

“The Theory of Economic Regulation” (1971) applies to another aspect of government behavior, the same cost/benefit motivation economists assume in the marketplace. This most highly cited of Stigler’s papers, was one of the first to analyze government regulation of industry as sometimes beneficial to the industry being regulated. As such, it is a key contribution to what has been called the “capture theory” of regulatory agencies. The paper is certainly one of Stigler’s most important. Stigler’s role as a founder of the economics of regulation, as well as a founder of the economics of information, is emphasized by the Nobel Foundation’s press release for his Nobel Prize.

“General Economic Conditions and National Elections” (1973b) expresses statistical doubts for the assertion, commonly believed without evidence, that short-term declines in the

performance of the economy will hurt the party in power. The brief eighth-ranked paper presents evidence that the change in the general level of national income is not a statistically significant predictor of the share of votes that the dominant party will receive in presidential elections.

“Law Enforcement, Malfeasance, and Compensation,” with Becker as first-author (1974) continues the program of Stigler’s 1970c paper, to use standard cost-benefit considerations to understand which employees will cheat their employer, and what the optimal response of the employer is. In this fifth-ranked paper, the authors argue that an enforcer is less likely to take bribes (i.e., “malfeasance”), the higher the enforcer’s salary, and the higher the probability of his being caught. One implication of the analysis is that an enforcer’s salary should be higher, the more costly it is to obtain good information on the enforcer’s acceptance or rejection of bribes.

“Free Riders and Collective Action” (1974) sketches three alternative accounts of how firms might sufficiently overcome the free rider problem so as to be able to support a trade association. Stigler then collects a new data set of companies and trade associations to test between the alternative accounts. In this twelfth-ranked paper, Stigler argues that the free-rider problem may not be as damaging to voluntary industry joint-activities as was commonly supposed. He indicates that firms may bear significant costs if they fail to participate, especially if the firms are heterogeneous in their outputs, or inputs, or desired research agendas.

“Citation Practices” (1975), with Friedland, is an effort to measure influence in economics by collecting the first major citation database of economics. In eighteenth place we find the only paper of Stigler’s top 20 that falls squarely under the heading of Stigler’s work on the economics of science, and hence the first paper that we have discussed in the main body of the current paper.

“The Xistence of X-Efficiency” (1976b) is a brief methodological piece aimed at convincing the reader of the continued fruitfulness of the standard optimization-under-constraints model. The eleventh-ranked paper is a critique of the Simon/Leibenstein advocacy of satisficing or x-efficiency as alternatives to the standard optimization approach. Stigler argues that the effect of laziness sometimes can be incorporated into optimization models by remembering that there are multiple outputs, often including, for instance, leisure and health. In other cases, the differences in firm behavior that are attributed to x-efficiency are better seen as differences in technology. Stigler does grant that Leibenstein’s analysis may draw our attention to an actual deficiency of standard microeconomic theory (p. 215): “No attention has been paid by economists to the analysis of the optimal amount of technological knowledge that a firm should possess.”

“The Size of Legislatures” (1976a) is an ambitious, if not entirely successful, attempt to develop and statistically test empirical generalizations to help explain the variety of sizes of legislatures in different jurisdictions. In his nineteenth-ranked article, Stigler examines several plausible explanatory variables, and finds a few that sometimes matter in the expected direction.

“De Gustibus,” with Becker (1977) is a bold methodological call to arms, with ample illustrations, but few statistics, of the fruitfulness of the neoclassical optimization-under-constraints model. Third ranked, it is a bold paper, not only for having a title in Latin that is a double entendre, but for sticking its neck out to make the controversial claim that economics can explain all human behavior. The paper argues for its thesis in two versions. The strong version contends that in the end, all behavior will actually be explainable in economic terms. The weak version contends that it is a tough-minded, and fruitful, methodological assumption for economists

to proceed *as if* all human behavior were actually explainable in economic terms. The paper is another of Stigler's challenges to intellectual complacency; and enough economists and philosophers have taken up that challenge to make the paper very highly cited.

"The Extent of the Market" with Sherwin (1985) is another attempt to boldly measure what had not been much measured before---in this case to determine how broad a geographical area in product and labor markets can be said to share "one price" and hence be part of the same market. The authors attempt to measure the correlation of prices in different locations, as an empirical method for judging whether the different locations are part of the same market. In this fourteenth-ranked paper, the authors find correlations around .9 for commodities in major cities across the U.S., and more variable, and generally lower, correlations (in the .3-.9 range) for wages of industrial nurses in major cities across the U.S.

FOOTNOTES

*Some of Stigler's contributions to the economics of science were more briefly discussed in Diamond 1996. An early version of the current paper was originally presented to the Liberty Fund symposium "The Legacy of George Stigler." The author is grateful for support from Liberty Fund, and is also grateful to David Levy for organizing the symposium. A slightly revised version of the paper was also presented at the annual meetings of the History of Economics Society. I thank Stephen Stigler for allowing me access to George Stigler's correspondence with Robert Merton at the Special Collections Research Center of the University of Chicago. Jay Satterfield expedited my visit to the Center. The author is grateful for comments from Claire Friedland, Vicki Longawa, Steve Levitt, Bryan Clarke, Gary Becker, and Robert K. Merton. Angela Kuhlmann and John Mulvey provided research assistance. Cathey Webb sent me a copy of the Southwood Report. Finally, an anonymous referee provided thoughtful comments that substantially improved the paper. If a page reference is given to one of Stigler's papers that has been reprinted in one of his major collections, the page reference generally refers to the reprinted version.

1. A premise of Stigler's "economics of science" was that scientific knowledge was of special interest, in part because it was useful in practical applications. Stigler would sometimes say 'whether you're a fireman, or an incendiary, you need to know how fire works.' Now some who do not share Stigler's premise (e.g, Mirowski and Sent 2002), are co-opting the label 'economics of science,' and applying it to their own work. Their intent is not so much to understand

scientific institutions in order to improve them, as it is to argue that knowledge produced subject to constraints, and in response to incentives, is thereby deprived of any special epistemic status, or practical usefulness. Near the end of his life, Stigler wrote a brief review (1989) of a collection of essays on the “rhetoric of economics,” which is an approach to criticizing economics that is sometimes viewed as complementary to Mirowski’s critiques of economics. Stigler acknowledged that McCloskey’s original *The Rhetoric of Economics* was written “with vigor and wit” (p. 839), but in the reviewing the collection of essays concluded that “to date, the only clear consequence of the study of rhetoric for economics appears to be conferences and volumes such as these.” (p. 840) If Stigler were alive today, he might say the same about Mirowski’s “economics of science,” only omitting the comment on vigor and wit.

2. At present, access to the Stigler archive requires permission from Stigler’s son, Stephen Stigler, who is a professor of statistics at the University of Chicago. The correspondence is in a folder that that contains letters from Stigler to Merton, and copies of some of Stigler’s letters to Merton. A more complete view of the correspondence may be available when the archives of Robert Merton are available at Columbia University. Since only a short time has passed since Merton’s departure from the scene (2/23/03), it is not unexpected that his documentation of the correspondence is not yet available. In an email to me dated 1/22/04, Bernard R. Crystal, Curator of Manuscripts at the Rare Book and Manuscript Library of Columbia University, suggested that timing of the availability of the Merton archive would probably not be known until

at least 3/04.

3. One of Stigler's longer letters in the folder (10/29/79) comments on a intriguing paper that Merton drafted, but never published, with his finance professor (and Nobel-Prize-winning) son, aiming at an economic model of problem choice in science. In early publisher's advertisements for the interdisciplinary journal *Rationality and Society*, founded by the late Chicago sociologist James Coleman in 1989, the paper by Merton and Merton was listed as forthcoming. I subscribed to the journal based partly on that information, and was disappointed when the paper never came forth. I wrote to the senior Merton and he kindly sent me a copy of a draft dated 1982. In response to an inquiry about the long delay in publication of the article, Robert K. Merton assured me (1/29/94) that ". . . neither my father nor I had a change of heart about the piece. Probably as you suggest, high competing demand on our joint time is the prime, if irrational, reason." He goes on to point out that his father was a perfectionist, as illustrated by his last book (Merton and Barber 2004) having been in process for over 40 years before it was published. He concludes: "I harbor the hope that I will complete that 1982 and beyond draft so that it does get published." Stigler's 1/29/79 letter was enthusiastic about the paper's intent, but did not refrain from noting its limitations. His enthusiastic opening comment is that the ". . . model addresses a fascinating problem, -- was it chosen to maximize the product of expected priority times scientific significance?" And at the end of his letter, Stigler says "I am eager to see the continuation of this work." In terms of limitations, Stigler suggests that the Mertons' ". . . formulation deals with

what might be called explicit problems, or problems that are at least moderately well-defined by the literature of the field.” Stigler seems to believe that the Merton’s model is less applicable to “. . . implicit problems – problems that are important but of which the scientists of the field were unaware.” He concludes that “in this class of problems your model still holds although the crucial question is now whether the sought after problem can be articulated in a coherent way and whether it is solvable, rather than whether someone else will get there first.” These comments are complementary to the view Stigler expressed in his Nobel Prize lecture (1983) that most progress in economics occurs by identifying a new (or previously neglected) area of fruitful inquiry, rather than through a competition to solve a well-defined problem.

4. Merton’s letter to Stigler may be read as both gracious and poignant, since one may suppose that if there had been a Nobel Prize in sociology, Robert K. Merton would have been a recipient. Merton had been President of the American Sociological Association in 1957 and in 1994 became the first sociologist to win the National Medal of Science. On a web page of the American Sociological Association following his death, he is called “. . . one of the most influential sociologists of the 20th century.” (Source: Robert 2004)

5. Although Stigler also did spend some time in the more traditional history of economic thought pursuits: explicating what great economists really meant and finding obscure predecessors in the discovery of some truth usually attributed to somebody else.

6. Jacob Viner, along with Frank Knight, was one of the leading faculty members at the University of Chicago while Stigler, and his classmate Milton Friedman, were graduate students. He is perhaps best known in the history of economic thought for his paper discussing the relationship between long-run and short-run average cost curves. Although Frank Knight is often viewed as Stigler's primary 'mentor', Gary Becker suggests (1993: 761) that "Viner may have had the greater long-run impact through his emphasis on the empirical relevance of microeconomic theory, and on the necessity of testing a theory with historical and other empirical evidence."

7. Frank Knight, along with Jacob Viner, was one of the leading faculty members at the University of Chicago while Stigler, and his classmate Friedman, were graduate students. Knight's doctoral dissertation, later published as *Risk, Uncertainty, and Profit* (1921), is still referred to for its distinction between "risk," where probabilities of outcomes are known, and "uncertainty," where such probabilities are not known. More broadly, the work is consulted for its discussion of entrepreneurship and competition. A distinguished group of students found Knight to be a stimulating and inspiring, though difficult, teacher. They remember him less for a particular economic doctrine, than for his devotion to intellectual inquiry. Stigler was one of only a few students who completed their dissertation under Knight's guidance.

8. Minimum wage laws in the United States mandate that covered workers be paid no less than

the government-specified minimum. The first federally-mandated minimum wage in the United States took effect on October 24, 1938 (History 2004). Economists have frequently been opposed to minimum wages because the policy increases unemployment, especially among the poor.

9. A story circulated at Chicago while I was a graduate student that then-editor of the *American Economic Review*, George Borts, once bragged to Harry Johnson that he was amazed at the quantity of great research that passed through his hands. Johnson's reputed response was that if that was so, maybe Borts should occasionally publish some of it.

10. In his paper with Friedland on the assertions of Berle and Means (1983), the authors provide another example that in many ways is complementary to the kinky demand curve case. In a tour de force of empirical analysis, Stigler and Friedland, using only data readily available at the time of Berle and Means, show that there is little evidence for Berle and Means' central message that broadly held corporations did not profit-maximize. Looking at the actual response of economists at the time, Stigler and Friedland find indifference at best, uncritical acceptance at worst.

Although Stigler chastised other economists for not being sufficiently influenced by empirical evidence, he admitted that he himself had sometimes been influenced on important issues by factors besides empirical evidence. For instance, in Stigler's "The Case against Big Business" in *Fortune* (1952) he advocates an activist antitrust policy against big business on the grounds that big business is generally not competitive. Later in his career, he had adopted a very different position. Was the change based exclusively on empirical evidence (as I had heard asserted by a

fellow graduate student in my years at Chicago)? Stigler in his *Memoirs* (1988: 100-103) recounts that in addition to empirical evidence (such as McGee's 1958 case study on Standard Oil), he was influenced by other factors, such as the arguments of Schumpeter's *Capitalism, Socialism and Democracy* (1942), conversations with Aaron Director, and economic theory.

11. "If I have a prejudice, it is that we commonly exaggerate the merits of originality in economics--that we are unjust in conferring immortality upon the authors of absurd theories while we forget the fine, if not particularly original, work of others. But I do not propose that we do something about it." (1965c: 15) Concerning Frank Knight, Stigler said: "There was no element of gadgetry in his work, although gadgetry has a powerful fascination for clever and subtle minds." (1973: 519)

12. Vicky Longawa, managing editor of the *Journal of Political Economy (JPE)*, recalls that the practice started sometime after she began working at the *JPE* in 1979 (Longawa 2003). She also reports that the practice continues, with a referee receiving \$75 if the report is returned within a month, \$40 if the report is returned from one month through three months, and no compensation after that. Becker (2004) recalls that Stigler believed the incentives had an effect, as evidenced by many more referee reports coming in just before the deadline. Becker believes that Stigler shared Becker's view that the "the amounts were too small to expect a large effect. But I also seem to remember that he argued that it would shift attention to refereeing *JPE* papers rather than those from other journals that people had." Hamermesh reports (1994) that as of 1994, similar

incentives were offered only by “a scattering of economics journals.” In his anonymous sample of four general economics journals, and three specialized journals, only one of the seven provided monetary incentives for prompt refereeing. In his empirical analysis Hamermesh finds a modest effect of the incentives on the speed of refereeing.

13. The Southwood Report argued for consolidation. I emailed several of the participants in the controversy (including Southwood), requesting retrospective comments. Only Bryan Clarke provided me a substantive response. Clarke reports (Clarke 2003) that he co-authored a rebuttal (Clarke, Fincham and Cove 1989) to the Southwood report (Southwood et al 1989), and that afterwards the Southwood Report was shelved. In his rebuttal, he “. . . pointed out that if Southwood’s recommendations were accepted, it would mean the demise of the best Genetics Departments in Britain (in those days, in Cambridge, Leeds, Leicester and Nottingham).” (Clarke 2003)

14. Some of Stigler’s important articles appeared before 1956, and hence were not included in the Web of Science counts. Among these important early articles would be "Production and Distribution in the Short Run" (1939) and "The Kinky Oligopoly Demand Curve and Rigid Prices" (1947).

15. Stigler’s friend, and sometime colleague, George Shultz (Another . . . 1973: 80), once stunned a banquet audience by singing the following brief poem that is a good, brief summary of

Stigler's priorities in scientific method:

A fact without a theory

Is like a ship without a sail,

Is like a boat without a rudder,

Is like a kite without a tail.

A fact without a figure

Is a tragic final act.

But one thing worse

In this universe

Is a theory without a fact.

(The poem, with a couple of differences, has been attributed to the pen of Edward Teller

(Johnston 2003: 5; Memorial . . . 2003: 3). Shultz delivered the keynote eulogy at a memorial service for Teller.)

16. Peltzman (1993) argues for the importance of these papers, and discusses their relationship.

17. Here Stigler is playing Bentham to Friedman's Smith? (See: Bentham)

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