

Friedland, Claire et al.

Working Paper

Remembrance and Appreciation Roundtable – George J. Stigler (1911-1991): Scholar, Father, Dissertation Advisor, Referee, Textbook Writer and Policy Analyst

Working Paper, No. 179

Provided in Cooperation with:

George J. Stigler Center for the Study of the Economy and the State, The University of Chicago Booth School of Business

Suggested Citation: Friedland, Claire et al. (2002) : Remembrance and Appreciation Roundtable – George J. Stigler (1911-1991): Scholar, Father, Dissertation Advisor, Referee, Textbook Writer and Policy Analyst, Working Paper, No. 179, The University of Chicago, George J. Stigler Center for the Study of the Economy and the State, Chicago, IL

This Version is available at:

<https://hdl.handle.net/10419/262581>

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**“Remembrance and Appreciation Roundtable George
J. Stigler (1911-1991):
Scholar, Father, Dissertation Advisor, Referee,
Textbook Writer and Policy Analyst”**

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**George J. Stigler Center
for the Study of
the Economy and the State**

The University of Chicago



Remembrance and Appreciation Roundtable George J. Stigler (1911–1991):

*Scholar, Father, Dissertation Advisor, Referee,
Textbook Writer and Policy Analyst*

By CLAIRE FRIEDLAND, CRAUFURD GOODWIN, CLAIRE H. HAMMOND,
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Edited and arranged by LAURENCE S. MOSS*

I

Moss on Stigler as a Historical Subject

GEORGE JOSEPH STIGLER is an obvious subject target for historians of 20th-century economic thought. His *Theory of Competitive Price* (1942) helped shape the development of microeconomics after World War II and his remarkable topics for analysis produced seminal contributions to important and rapidly growing fields such as the economics of industrial organization, the economics of regulation, public choice economics and the economics of information. He was recognized for his contributions to economics, especially the economics of

* On June 30, 2001, at the History of Economics Society meeting at Wake Forest University, Winston-Salem, North Carolina, the current editor of this journal, in consultation with the then president-elect of the Society, Professor Dan Hammond, organized a special remembrance session in honor of George J. Stigler. Contributors to this session were: Ms. Claire Friedland, George J. Stigler Center for the Study of the Economy and the State, University of Chicago, Chicago, Ill. 60637; Professor Craufurd D. Goodwin, Duke University, Box 90097, Durham, N.C. 27708-0097; Professor Claire H. Hammond, Wake Forest University, Box 7505, Winston-Salem, N.C. 27109; Professor Daniel Hammond, Wake Forest University, Box 7505, Winston-Salem, N.C. 27109; Professor David Levy, George Mason University, Center for Public Choice, 4400 University Drive, MSN 1D3, Fairfax, Va. 22030; Professor Steven G. Medema, University of Colorado at Denver, P.O. Box 173364, Denver, Colo. 80217; Professor Michele I. Naples, The College of New Jersey, P.O. Box 7718, Ewing, N.J. 08904; Professor Warren J. Samuels, Michigan State University, Economics, East Lansing, Mich. 48824; Professor Stephen M. Stigler, Statistics, University of Chicago, 5734 S. University Avenue, Chicago, Ill. 60637.

information, by the award of the Nobel Prize in economics in 1982. Since the Nobel Prize there has been an abundance of biographical and historical material about Stigler, including his own brief autobiography, *Memoirs of an Unregulated Economist* (Stigler 1988a) and the entry in Blaug's *Who's Who in Economics* (Blaug 1986). A useful bibliography of Stigler's writings has been prepared by Ms. Vicky M. Longawa and published in the *Journal of Political Economy* (Longawa 1993).

In this article we shall not try to reproduce all of this information. Also, we shall not try to touch on all aspects of his life and intellectual contribution. We must cover just a few of the biographer's subject areas, hopefully in a novel way. Still, something "extra" does need to be introduced into any conversation about Stigler and his legacy. In 1989, Professor D. N. McCloskey recalled a conversation with Stigler that was "especially eye opening to an associate professor [who was] beginning at last in 1978 to doubt the epistemological claims of positivism" (McCloskey 2001:160). McCloskey (the "younger colleague") recalled the incident as follows:

George was holding forth on the merits of behaviorist theories of voting in which people are said to vote their pocketbooks. His younger colleague, who had just read Brian Barry's devastating attack on such models [1978] and for ten years had been teaching first-year graduate students about the small man in the large market, following George's exposition in *The Theory of Price*, noted that people would be irrational to go to the polls in any case. Since the people were nuts to begin with, it would be strange if they voted their pocketbooks when they got inside the booth. The argument struck a nerve, and Stigler became as was his custom abusively positivistic, declaring loudly that all that mattered were the observable implications. (McCloskey 2001:160).

As a result of this encounter and others with the Chicago school economists in residence circa 1980, McCloskey concluded that Chicago's version of positivism had become a negative element in economics. Stigler was the catalyst in purging McCloskey of his "Chicago school methodology" (McCloskey 2001).

The McCloskey anecdote is a disturbing one and the image it portrays of a stern, humorless taskmaster is not the memory many others have of Stigler. I suppose some of the material in this article can be said to "set the record straight" and offer a more balanced and

nuanced portrait of Stigler. Still, Warren Samuels recalls a Stigler who was “the principal, but by no means the sole, author of absolutist formulations of neoclassical doctrine” (see below, section VIII). The collaborators on this piece include a family member, a Ph.D. candidate who wrote under Stigler, colleagues, co-authors and several of the younger generation of historians of economic thought who offer appraisals of Stigler’s influence both on the profession and the professional literature. But the purpose of this article is not hagiography but to offer evidence for deeper interpretation and understanding.

What emerges here is a much more interesting portrait of a justly famous economist. This is not to deny McCloskey’s perception of a dogmatic Chicago school headed by a closed-minded Stigler, but against this perception we have Claire Friedland’s recollection that in his “later years” Stigler began to consider the possibility of the legitimacy of the gains obtained by special interest group legislation. Stigler asked by what independent standard could an economist pass judgment on the results. Similar problems involving the distinction between evaluating policies and explaining them are touched on by Levy in his contribution explaining the ways in which Stigler influenced his research. Let us begin the journey.

II

Stigler on Stigler as Father and Mentor

MY FATHER, AS YOU MAY KNOW, had more than a small amount of skepticism about the scientific uses to which historians put biography and autobiography (Stigler 1982a). He had many examples of dubious instances of the use of biography, such as one I would add from statistics: some people have attributed Francis Galton’s interest in eugenics to his childless marriage and Ronald Fisher’s interest in eugenics to his having had seven children. Try using those data in a regression analysis to predict scientific interests! But George was not entirely consistent on this. [I should pause to explain this familiar form of address: as long as I can remember, I have called my father “George,” and as long as I can remember my friends have thought this odd. He encouraged it, on the principle that all his friends called him by his first name, and we *were* his friends.] Despite these misgivings George read biographies

with great apparent interest, and wrote his own autobiography, *Memoirs of an Unregulated Economist* (Stigler 1988a). Still, his own was about as impersonal an autobiography as you will find. My wife Virginia typed the manuscript for that book and we were constantly urging him—with no evident success—to put more of himself in the book. I attribute this to the fact that he was essentially a very private person. He practically never spoke about his own feelings. And yet he was reasonably transparent. I think that was a trait of his that made people comfortable being around him; they knew where they stood, there was no misrepresentation. At least it made some people comfortable—there were exceptions. I understand that once Gardner Means came up to him at a meeting and exclaimed, “George, I’m not as dumb as you think I am!” For once, George was left without a reply.

He loved teaching, and even practiced some of it at home. For example, we were given unusual early training in science. I remember a lie detector he constructed in the basement out of scraps. It consisted of a box with two lights on top, a red light and a green light, and two wires leading from the box, each with a penny soldered at the end. The subject would firmly clasp a penny in each hand while answering the crucial question, and the red light would signal a lie, green the truth. We marveled at the accuracy of the device, even after we discovered the secret switch he was using to operate the lights. As a physical scientist, George was more of the school of Rube Goldberg than that of Isaac Newton.

As if to confirm his thesis on the uselessness of biography, it would have been hard to recognize him as an economist of the Chicago School by observing only his private life. He overindulged his children, yet when we showed signs of inheriting this trait he was indignant. He hired his grandsons by the hour to dispose of brush, and was surprised and outraged at the studied, glacial pace of their movements. He hired his granddaughters to dispose of brush, paying them by the load, and he was surprised and outraged at the microscopic size of the loads they pulled.

He was a practicing economist, in that he invested with some success—especially in rising markets. He once at our request gave us a list of 13 stock purchase suggestions, with the comment: “I guarantee that at least one of these will do well.” His forays into silver futures with Milton Friedman were correct in all respects except timing.

My father played golf fervently; so much that someone unfamiliar with him might wonder how he could find time for scholarship. George had a lifetime total of five holes-in-one, a fact that impressed us much more than it did him—he attributed it to the law of large numbers, but that law never seemed to work the same way for us.

His own academic career at the University of Washington did not get off to a flying start—his first quarter grades included a C in English and a D in Military Science, and he was carried forward principally on the strength of an A in German, which was in fact the first language he had learned as a small child. By his junior year, however, all was well, and he was the only student in the junior class with all As—including Military Science! I think there was an early indication of his academic personality in those marks, both as a freshman, when rebellion against what was presumably a rigid and uninspired course almost led to disaster, and his later overcoming of the same obstacle; from his weak initial performance in English to his later mastery of that language in all its myriad of subtle shades. Yet despite the breadth of his later knowledge, his own college training was not broad at all. As an undergraduate he took 25 of the 36 courses required in the business college in Business Administration, and five of the remainder in political science.

The central focus of George's professional life was at the University of Chicago. He came to the University of Chicago in 1933, a year after he completed an MBA at Northwestern, where he wrote a long term paper that examined the history of value, with particular attention to Smith and Ricardo. This may be the earliest indication of his interest in the history of economic thought. He once claimed that he came to graduate study to escape the Great Depression, but that seems unlikely—it is hard to picture the University of Chicago then or now as a refuge from hard times. I think it much more likely that a developing fascination with the field of economics was responsible. He came to the University of Chicago with a fellowship from the Department of Economics, a tribute, no doubt, to his strong undergraduate record at the University of Washington and recommendations from a Professor at Northwestern's business program. It was at Chicago, starting in 1933, that he met and formed lifelong friendships with Milton and Rose Friedman, with Aaron Director, with Allen and Ann Wallis.

I have a souvenir of that era, a preliminary examination schedule

from the Department of Economics, Spring Quarter 1935. The names of those taking the Economic Theory exam are formidable—but who is C. J. Shohan, and where did he pursue his career? Note the first two names on the Statistics Exam: Rose Director and Milton Friedman, before they married.

It was here that my father met my mother, when both were graduate students staying at International House. Theirs must have been an unusual courtship. I find in my mother's papers indications that she was wooed with the full force and technique of the then-emerging quantitative methods of social science. I find a carefully plotted time series that my father had prepared, tracking his spirits over the course of a typical day. The eye is drawn to a small peak at 10:30 P.M., identified as my father's nightly ping-pong game with Elton Woolpert, but

Exhibit 1

DEPARTMENT OF ECONOMICS

SCHEDULE FOR PRELIMINARY EXAMINATIONS FOR THE DOCTORATE

Spring Quarter, 1935

The schedule below shows the preliminary examinations requested for the current quarter. Will the Chairman of each Committee please be responsible for turning in the complete examination by at least one week before the date on which it is to be given?

<u>Dates</u>	<u>Examinations</u>	<u>Committees</u>	<u>Students Enrolled</u>
Saturday, May 11 8:30, S.S.R. 417	Economic Theory (New Plan)	Viner, Chairman Schultz Yntema Knight	Friedman, M. Shohan, C.J. Stigler, G.J. (Brookings) Wallis, W.A.
1:30, S.S.R. 417	Monetary and Cycle Theory	Mints Cox	
Saturday, May 18 8:30, S.S.R. 417	Financial System and Finan- cial Administration	Mints, Chairman Cox Meech Gideonse	Curtis, C.H. Shohan, C.J.
Saturday, May 18 8:30, S.S.R. 417	Government Finance	Leland, Chairman Simons	Stigler, G.J. (Brookings)
Saturday, May 18 8:30, S.S.R. 417	Statistics	Schultz, Chairman Cover Yntema	Director, R. Friedman, M. Jacoby, N.H. (Springfield)
Saturday, May 25 8:30, S.S.R. 417	Economic History	Wright, Chairman Nef Knight	Ostrander, F.T. (Williams) Shohan, C.J.

this peak is dwarfed by those beside it, when he was in the company of his bride-to-be.

The greatest influences on my father during his years as a graduate student were his own teachers, and by teachers I include both those on the faculty (particularly Frank Knight and Jacob Viner) and his fellow students, including Milton Friedman, Allen Wallis, Paul Samuelson (then an undergraduate), and Kenneth Boulding.

He had phenomenal energy and always had several projects going. He built us furniture, ran errands, washed dishes, painted houses, built wharves, replanked boats, sawed down small forests. To his friends in academia he offered criticism, debate, suggestions, references, redirection. He treasured his friendships, and glowed when he could recite good news about someone, whether it was someone else's skill at golf, or a clever new argument by a colleague. Conversely, he was in pain when the news was not so good.

He had an unusually accurate memory, whether for names, for friends' anniversaries, or for the details of the scholarly world of Victorian England he loved so well. A year before he died I read to him a nice quotation that a newly published dictionary attributed to Herbert Simon, saying that theory and data were like two blades of a pair of scissors (Sills and Merton 1991). The dictionary gave an earlier reference to an appearance of this metaphor to describe the attribution of value to either utility or cost of production, in the work of Alfred Marshall in 1890, but George was able without hesitation to tell me exactly where the same metaphor could be found a half-century earlier, deep in John Stuart Mill's *Principles*. From the absence of any mention by him of Adam Smith, I knew that Smith was silent on the matter.

He had high standards, and he believed in the importance of standards. We came to learn that absence of criticism was high praise. Not that he was harsh on us, or unable to praise, just that sometimes it took unusual forms. When a daughter-in-law-to-be baked her first cake, it turned out to be a close cousin to a concrete block, and he praised its weight and its strength. Their relationship survived nonetheless, and I think later cakes were the better for the criticism. Just so, I think his professional colleagues came to value his critical comments, with the possible exception of a few whose books he reviewed. There was one area where he was unstinting in his praise, however: he thought the intellectual atmosphere at the University of

Chicago over his long career, whether in Economics, Business, Law, or elsewhere, the quality of the people and debate was worthy of comparison to the age of Smith and Hume.

In closing, I will share with you an unpublished paper I found in my father's papers, dating from about 1950. It indicates that I was the author, but I would, if the 1950 date is correct, have been only nine years old, and the true author is not hard to guess. It was a submission to the *Journal of the American Statistical Association*, then edited by

Exhibit 2

One Fundamental Remark on Time Series

1. Even those who like to drool over the raw data, like Burns and Frickey, eliminate seasonal fluctuations from time series.¹

¹ I am indebted to Burns, and Frickey is indebted to me, for discussion of this point.

2. And those with a little vision or a lot of statistical assistance, like Henry Schultz, also eliminate the trend.²

² I never had the pleasure of discussing this with Schultz.

3. And a few transcendental scientists have actually eliminated the cycle from time series; I have Henry Moore and George Stigler in mind.³

³ Moore used periodogram analysis; I am indebted to him for telling me this. My father has used with great success a 1283 month moving average in as yet unpublished work.

4. There are even slobos who have taken out the random perturbations.⁴

⁴ My father, to whom I am also indebted for the correct spelling of "perturbation", tells me that the method is described in the well-known article, "Contributions to the theory of pagination," by the equally well-known statistician, Sequent.

5. But no one has yet taken an important time series and eliminated from it seasonal fluctuations, extirpated the trend, annihilated the cycle, and banished the irregular components. This I do on the enclosed diagram. My mathematical method will be described in an article sent to a higher-class journal.

Stephen M. Stigler

George's close friend Allen Wallis. The diagram that accompanied it was of course just an unadorned horizontal line.

III

Levy on Stigler as Dissertation Advisor

Introduction

I propose to defend the hypothesis by means of a worked example that the dissertations a scholar directs give a vantage into his thought that might be accessible no other way. The dissertation problem is one that the teacher has, for one reason or another, not solved. Dissertation problems acknowledge a boundary where it is worthwhile to expend resources moving the state of the discipline from unsolved to solved. Dissertation problems allow a Mandevillean revealed preference move to be applied to the space of problems in economics.¹

The dissertation problem I propose to talk about is not the one that satisfied the conditions for a Ph.D. by the University of Chicago trial-by-publication procedure. I rather think Stigler would deny “directing” that one!² The one I shall discuss is that which I set out to solve in 1968. Why did classical economics die? Stigler was nothing if not generous. That is a great problem to give to a student, even one with four graduate courses in the history of economics even before attending Stigler's lectures.³ And of course I failed even to get a handle on it before I was distracted by the content of Malthus's population theory. To the extent that Sandra Peart and I (Peart and Levy 2000) have made some progress on the problem, it is largely by following in steps that were suggested to me by Stigler. These steps I propose to remember today not merely because they serve as a memorial to a great teacher—although that would suffice to explain my presence—but because these steps might shed light on dark patches of his doctrine. What, in particular, did he mean by the “Coase Theorem”?⁴ Is it an optimistic proposition about the world? Or is it a pessimistic proposition about the limitations on the role of the economist in the world?

What Does the Economist Want?

Previous explanations for the death of classical economics have appealed to events within the space of theory. For instance, Ricardian

economics died because it was simply bad economics (Blaug 1958). Although Stigler had no quarrel with the interpretation of Ricardian economics offered in such accounts, this left, as he pointed out to me, the obvious problem: Why did it ever succeed?

Although this he did not say, I believe he was troubled by the implicit supposition that economists were motivated by the pursuit of the truth. I say this because the first thing he had me read was Robert Merton's account of priority fights in science. Priority fights, as an expenditure of a scientist's scarce time, make sense only if one supposes the scientist is doing science for something other than the pursuit of truth (Merton 1973). Moreover, he showed me his correspondence with Thomas Kuhn whose formative *Structure of Scientific Revolution* (1962) denied truth-seeking in any simple sense by making truth relative to a language community.⁵ Neither of these selections should come as any surprise (Stigler 1969).

My conjecture was that something changed in the belief state of late-nineteenth-century economists that caused them to think less well of the doctrine of fixed human nature than early-nineteenth-century economists. I had read Alfred Marshall's 1885 account that the economists of his generation no longer believed "Ricardo's" doctrine that man was a constant quantity.⁶ And thinking that denial of fixed human nature would throw doubt upon the long-period growth modeling that characterized Ricardian economics, I plunged in the non-technical critics of classical economics to see if I could find what persuaded the economists to give up fixed human nature. By what argument was the doctrine defeated?

I tried to read S. T. Coleridge and failed to understand his argument.⁷ I'd learned from Earl Hamilton where Thomas Carlyle called us the "dismal science" so I read that pamphlet and saw nothing but hate. I looked into Charles Dickens and didn't see an argument. Then I gave up. It took close to 30 years to realize that I had in fact begun to read the right things but I didn't know enough to read them rightly.⁸ I would remember what I'd read as the pieces started to form themselves into a solution.⁹

One of Stigler's persistent themes—which may be more apparent to me than to others¹⁰—is his worry over the fact that there is no hard-and-fast distinction between the economist and the subject mat-

ter of economics. Here is a particularly funny version of this concern:

The economist, and his brethren in the social sciences, have a second level of difficulty not shared by the physical sciences. Our main elements of analysis are people, and people who are influenced by the practices and policies we analyze. Imagine the problems of a chemist if he had to deal with molecules of oxygen, each of which was somewhat interested in whether it was joined in chemical bond to hydrogen. Some would hurry him along; others would cry shrilly for a federal program to drill wells for water instead; and several would blandly assure him that they were molecules of argon. (1966:8)

More seriously, if everyone is the same in deep structure (Stigler-Becker 1977) what then do we make of the economist? What's in it for him? If the economist is inside the economy, maximizing just like everyone else, why would we expect truth-seeking on his part?¹¹ But if the economist is not truth-seeking, just maximizing, by what warrant does he tell other maximizers how to behave?

To see how the "truth-seeking" rabbit comes into the hat consider how one argues that a policy is a "mistake." One takes the announced goals of a policy and compares them with the results. If the results don't match the goals then the articulated goals—as a proposition in logic—are shown "false." Therefore, it is argued, the policy ought to be reformed. Why is there reason to believe that the "goals" are the goals? To identify the two is to assume truth-seeking, not utility maximizing. Perhaps the "goals" are pure persuasion.

Why cannot we observe the goals by the usual revealed preference move? When Stigler published *The Citizen and the State* he contrasted the traditional view of "mistaken" choice—a view that supposed truth-seeking—with his revealed preference view.

The study of the announced goals of a regulatory policy is useful work, and I am delighted that these essays contributed to the development of the now widespread practice of studying the actual effects of public policies.

Often, but of course by no means always, the public policies seem not to achieve much toward fulfilling their announced goals. . . . Eventually the question insists upon posing itself: in such cases, why is the policy adopted and persisted in?

It seems unfruitful, I am now persuaded, to conclude from the studies of the effects of various policies that those policies which did not achieve their announced goals, or had perverse effects (as with a minimum wage

law), are simply mistakes of the society. A policy adopted and followed for a long time, or followed by many different states, could not usefully be described as a mistake: eventually its real effects would become known to interested parties. *To say that such policies are mistaken is to say that one cannot explain them.* (1975:x, emphasis added)

Lacking a Rational Choice Theory of Scholarship

The passage I emphasize seems to me to be the center of the Stiglerian message: *To say that such policies are mistaken is to say that one cannot explain them.* The economist is not outside the economic process motivated by pursuit of the truth. It is this motivational heterogeneity that gives economists warrant to direct their fellows.¹² If the difference between an economist and an ordinary person were simply a matter of knowledge, then the economist could simply give (or sell!) the information and effect the change without any necessity of direction. Thus Stigler's "Coase Theorem" is in my reading not so much optimism about the world as pessimism about our knowing enough to make it better, in the technical sense that economists would use that term.

Now let me connect up his insight with the problem that Peart and I have been working on: the death of classical economics. As everyone here knows Stigler's fondness for Adam Smith, it is appropriate that I read the passage in which Smith simultaneously announces the analytical egalitarianism that Stigler would defend so elegantly and puts the philosopher on notice that his choice is to be modeled:

The difference of natural talents in different men is, in reality, much less than we are aware of; and the very different genius which appears to distinguish men of different professions, when grown up to maturity, is not upon many occasions so much the cause as the effect of the division of labour. The difference between the most dissimilar characters, between a philosopher and a common street porter, for example, seems to arise not so much from nature as from habit, custom, and education. When they came into the world, and for the first six or eight years of their existence, they were perhaps very much alike, and neither their parents nor playfellows could perceive any remarkable difference. About that age, or soon after, they come to be employed in very different occupations. The difference of talents comes then to be taken notice of, and widens by degrees, till at last the vanity of the philosopher is willing to acknowledge scarce any resemblance. (Smith 1776:28)

But this was not the first passage in which Smith announced his doctrine of fixed human nature. Here is an earlier one, from the *Lectures on Jurisprudence*:

Whenever commerce is introduced into a country, probity and punctuality always accompany it. These virtues in a rude and barbarous country are almost unknown. Of all the nations in Europe, the Dutch, the most commercial, are the most faithfull to their word. The English are more so than the Scotch, but much inferiour to the Dutch, and in the remote parts of this country they [are] far less so than in the commercial parts of it. This is not all to be imputed to national character, as some pretend. (1978:538)

In Smith's work and that of his followers, explanations of behavior based on incentives are offered in competition to the "vulgar" explanations based on racial differences.¹³ But what explanation can we find in classical economics that allows us to predict the content of a scholar's theorizing? The content of theory is not the same thing as the amount of time one spends at the task. Smith had no difficulty comparing the output of Oxbridge with Scottish universities by appeal to the proposition that if one withdraws incentives to diligence, diligence vanishes.¹⁴

In this gap we can find racism infecting the center of British neo-classical economics. Consider Walter Bagehot's racial explanation for the abstraction in classical economics by appeal to Ricardo's Jewish heritage.¹⁵ At his Cambridge inaugural Alfred Marshall offers this insight as his own:

BAGEHOT

For this trade Ricardo had the best of all preparations—the preparation of race. He was a Jew by descent (his father was one by religion), and for ages the Jews have shown a marked excellence in what may be called the "commerce of imperceptibles." . . . The fact remains that the Jews have now an inborn facility in applying figures to pure money matters. . . . The writings of Ricardo are unique in literature, as far as I know, as a representative on paper of the special faculties by which the Jews have grown rich for ages. . . . I know none but Ricardo's which can awaken a book-student to a sense of the Jewish genius for the mathematics of money-dealing. His mastery over the abstractions of Political Economy is of a kind almost exactly identical. (1880:152–53)

MARSHALL

And as to their tendency to indulge in excessively abstract reasonings, that, in so far as the charge is true at all, is chiefly due to the influence of

one masterful genius, who was not an Englishman, and had very little in common with the English tone of thought. The faults and virtues of Ricardo's mind are traceable to his Semitic origin; no English economist has had a mind similar to his. (1885:153)

They [Ricardo and his followers] regarded man as, so to speak, a constant quantity, and gave themselves little trouble to study his variations. . . . This did little harm so long as they treated of money and foreign trade, but great harm when they treated of the relations between the different industrial classes. (1885:154–55)

From there racial characteristics enter the economist's tool kit. At his 1890 Presidential Address before Section F of the British Association, Marshall would explain the difference between English and German economists' views on state regulation by racial differences. To find the optimal institution one needs to find that which is most in keeping with the racial characterization of the people for whom it is designed. We think of the economist as a spokesman for the race and work backwards.¹⁶

Today we might view such racial views of economists as simply prejudice. But "prejudice" is a "mistake" with negative externalities. And I repeat the center of George Stigler's teaching—*To say that such policies are mistaken is to say that one cannot explain them*. And if we do not explain such "mistakes" by appeal to rational choice considerations, there are alternative explanations.

IV

Goodwin on Stigler as a Referee

FOR THE NEWBORN JOURNAL *History of Political Economy (HOPE)* in 1969, George Stigler was more than a referee. He was advisor, critic, and friend. He also provided two things in short supply for us at that time, and therefore of inestimable value: credibility with a skeptical outside world and a sense of confidence that we would in fact succeed. These qualities that radiated from George were in contrast to what we sometimes encountered from the big dogs of the profession elsewhere. In England in 1968, by contrast, Lionel Robbins at an inaugural meeting of historians of economics loudly proclaimed that *HOPE* was a bad idea and that he would advise his "pupils" to take no part in it. Luckily they ignored this advice.

Certainly George was not warm and fuzzy with me or about *HOPE*. He was very businesslike. He wrote short crisp letters with a decisive position on all questions at hand. He also quite often picked up the phone and delivered his judgment orally. You never had to wonder about his advice. Once when he called Duke and I was not there, having just gone on leave to work at the Ford Foundation, he tracked me down in New York and, when I picked up the phone, he barked, "What are you doing up there? Get back down to Duke where you belong." And he wasn't kidding. He didn't like philanthropies, even though we gave a grant to Milton Friedman. However, I always had a clear sense that George had *HOPE's* interests at heart, and that by serving on our advisory board, publishing on our pages, and mentioning us favorably to others, he added substantially to our reputation.

In order to regain a sense of George as a referee I retrieved from our files most of the reports he submitted to us from the first in November 1969 until the last in July, 1988. Our records are tolerably complete and they suggest the following. Over two decades we sent George 70 papers. We keep our files of manuscripts received in two series: those rejected and those ultimately accepted and published. George made the life of this historian of his refereeship easy by directing the majority of his reports to the first series, those consigned to oblivion. The data are roughly as follows: of the total of 70 papers that he received, he recommended reject outright 50, accept outright four, revise and resubmit seven, decline to read on various grounds six, and other three. Unlike many of the exceptionally distinguished referees to whom we sent papers George almost always read whatever I sent him. He did not pick and choose what might appeal to him, and he sent papers back almost only when he had read them already for another journal. He was tough but fair. As far as I can judge from reading the reports all together one more time, what mattered to him most about a manuscript was that it be faithful to the texts (he knew so many of them himself practically by heart), that it demonstrate familiarity with the literature both primary and secondary and above all that it teach him something, that it contain novelty of some sort. Ideology seemed to play little part in his judgments. One of his first recommendations for acceptance was an edited version of lecture notes taken in

a class of Adam Smith, edited by Ronald Meek, whose ideology was notably different from George's own. George wrote: "It would be reasonable to deny publication to an early set of lecture notes if the lecture is by Professor Smith. Meek's introduction is also sensible and persuasive." Yet he could not resist adding, "I think that Meek emphasizes less interesting aspects of the notes, but such differences of interest are inevitable by a Marxist!" (It should be recalled that Ron preferred to be called a "Meeksist.") Over the two decades as our referee George was an equal-opportunity referee. He advised against, in addition to run-of-the mill submissions, some of his close colleagues, Chicagoans of all kinds, fellow Nobelists, and even his own students (whose work should not have been sent to him, but we did not always know of the connection!).

We have had a rule at *HOPE* from the beginning that except in the most unusual circumstances we provide the author with all the comments received on a paper, with names deleted of course. The practice has sometimes been controversial because, for obvious reasons, it can be hurtful. But we have accepted the logic of the courts on this question and conclude that the accused should be able to face their accusers. And indeed, when unhappy authors can demonstrate that referee reports we send them are inaccurate or blatantly unfair we willingly move to additional referees. Why I describe this practice is because George sometimes tested its limits. George never wrote a long or nuanced referee report. Typically, he sent a few comments on specific points on a separate sheet and a pungent and pithy overall assessment in the body of a letter. We passed all of these along to the authors and sometimes, but not often, with dire consequences. I could offer many vivid examples of George's comments; let me offer just a few. Of one paper he wrote: "It is inevitable that *HOPE* should occasionally publish pretentious nonsense—every journal occasionally falls from grace. But you should at least insist that it be historical nonsense, and on this ground alone this piece of claptrap should be rejected. I made a couple of remarks but my heart was not in it." Of another he said, "The only connection that [this] paper has with economics is that he [the author] studied economic journals rather than those in some other field. The only connection that this paper has with the history of thought is that he

submitted it to HOPE. This latter connection should be severed.” Or again, this “manuscript strikes me as excessively vague and exceedingly long. His thesis is not specific enough in formulation or documentation to allow any real testing . . . I would reject this loose, inflated piece.” Should we have sent, as we did, comments like this to the authors? I am still not sure.

George often engaged in a practice as referee that I have always welcomed as editor. He lectured me, as well as the author of the manuscript, on matters of general interest, as if we were students in the history of economic thought class. Paul Samuelson does the same thing, and he once prefaced a lecture to me over the phone on some subject or other by saying, “You had probably gone to the men’s room when this topic was dealt with in graduate school, so let me bring you up to date.” These lectures by George, Paul, and others were often so good over the years that I have been tempted to extract them from referee reports and publish them as a book with a title such as “Nuggets from Nowhere.” Let me give you some examples of lectures by George embedded in these reports.

He complained of a temptation in the history of ideas to undertake brief inconsequential pieces at the expense of serious research. In one case he wrote, “this note is painfully cute and allusive. . . [the author] ought soon to be taught that an endless supply of small notes is not substitute for a serious research undertaking.” George was worried also, as many of us are, that the history of economics may become a launching point for attacks on the modern profession. Of a paper in this genre he wrote, “The author is profoundly discontented with modern economics. Modern economics would be equally disappointed with this paper.” George took an unambiguous position on the question of blind refereeing. He wrote of one paper that we thought we had rendered anonymous in deference to the author’s wishes, “You should provide authors’ names. They provide useful information. This author identifies himself in the last footnote, but you should save this referee, at least, the trouble. A feeble reward of the referee is to learn something about others.”

It is well known from his controversy with Bill Jaffe and other statements that George was uneasy about the use of biography in the history of ideas (1982a). A point he made repeatedly in these reports

is that a distorted picture of an economist and his personality may be constructed by picking a single point in a career for description. About one paper on Frank Knight he wrote to the author: "There is often argument over which portrait of a scientist we should cherish: the vigorous young man, the mature man at the height of his career, the venerable and wise elder man. You go to the extreme of Knight in his eighties, and I should hope that when your paper is published, it is complemented by a portrait of one of the younger Knights." And about another paper on Knight: "I don't like so personal a piece on so old a scholar." For obvious reasons, George tended to get most of the papers on Knight for review; and he was, as usual, a tough critic. Here is what he thought about a third. "It pays no attention to Knight's uncertainty as to what changes in institutions would improve man, or to his violent criticism of many institutional changes (such as social planning). Nor does the paper notice Knight's one-sided myopia with respect to the effects of individuals and groups on political institutions. Knight deplored economic theories of politics, including mine!"

George was sensitive to the problem in the history of economics that scholarship tends to occur in little flurries surrounding anniversaries or publications of complete works or other events. In 1973, when commenting on another paper on Adam Smith, George anticipated the challenge that would soon be presented in 1976. "We shall all face the problem, in 3 years, of saying something new about Smith, and like [this author] most of us will fail to solve it."

Why was George Stigler so interested in the history of economic thought that he was willing to devote a substantial amount of his time and energy to helping a new, struggling, rinky-dink journal get on its feet? I don't think his interest was hagiographic—he worshipped few saints—or gossipy. He certainly did not pursue history as a refuge from the modern discipline, where he remained a powerful force until the end. I think he truly believed that the history of economics was important for progress in economics, for the training of students and for the conduct of sophisticated research. Like Kenneth Boulding he believed that economics has an extended present. Too bad there are not more like him in the profession today!¹⁷

V

Naples on Stigler as Textbook Writer

GEORGE STIGLER'S (1942) TEXTBOOK, *The Theory of Competitive Price*, set the paradigm for graduate and intermediate economics education in the postwar period. It illustrated the logic and power of marginalist theory at a high level of abstraction. In 1946 Stigler kept Perfect Competition as the referent, but added chapters on the theory of Imperfect Competition, where both assumed rising marginal production costs and short-run profit maximization. Stigler's textbook went through five editions in all by 1987.

Stigler's analysis hinged crucially on claiming that certain productive factors were indivisible for the firm even in the long run, and that beyond some point there would be decreasing returns to scale. This permitted him to "assume that diseconomies of large-scale production set in soon enough to insure numerous firms and therefore competition" (1942:160).

These theoretical arguments did not coincide with the empirical evidence of the period that there was no tendency for larger businesses to have higher unit costs over the long run.¹⁸ For him, "If the most general structure of a given type of economic system is in question, . . . the task is analytical: to explore the implications of a set of assumptions" (1942:19). Recognizing that simplicity could be key in attaining theoretical preeminence (1942:8), Stigler used bold, expert strokes to capture the imagination of the profession. Stigler's textbook communicated the view that the economy was both egalitarian and a level playing field, where all firms were equally powerless and none had access to more or better information. This resonated in an environment where "Competition was and is a policy as well as an analysis" (1942:24).

This essay addresses Stigler's analysis of the source of long-run rising marginal costs, whose corollary is that firms reach an optimum size beyond which they cannot efficiently expand. Stigler's study of the indivisibility of the managerial function that set limits to company growth drew on Austin Robinson's (1932) book on business and industrial structure. Unlike Robinson, Stigler did not address counteracting tendencies.

Stigler's Analysis

Stigler motivated his discussion of limits to company size in the long run by introducing the concept of indivisible services. Some resources are fixed for the company even in the long run and have to be used to varying degrees. "[T]heir productive services are underworked or overworked at most outputs" (1942:133). The need to vary utilization of this fixed factor implies that indivisible services, "by their very nature, lead to alternating stages of increasing and decreasing returns to scale of plant" (1942:134).

Stigler's discussion of the types of productive services that are subject to economies of scale paralleled Austin Robinson's analysis (1932:16), which had identified five categories of productive services whose scale economies might or might not eventually be exhausted: technical, managerial, financial, marketing, and the influence of risks and fluctuations. Stigler also identified five categories: management, machinery, marketing, financial, and research, which last Mr. Robinson had included under management.¹⁹

In each case, Stigler identified a few examples of the named productive service that would give larger enterprises an advantage over smaller ones.²⁰ The only type of service that he judged ultimately indivisible and therefore subject to decreasing returns was management. It was his analysis of management's coordination failure that provided the linchpin for his theory of limits to company size, thereby assuring a pre-condition for Perfect Competition.

Stigler began by asserting that "[e]mpirically there is strong evidence that when the number of men in a plant is doubled, the supervisory, coordinating, and decision-making staff must be more than doubled" (1942:134). He cited no source. He referred to the benefits of the division of labor as being outweighed by "the cost of loss of unity and expedition. The larger unit is more unwieldy and cumbersome. . . ." (1942:134)

Stigler grounded his view of the limitations on management in the long run in his analysis of the short-run diminishing returns derived from extending the division of labor.

It is possible, but somewhat artificial, to consider the . . . case of specialization of labor as also representing an indivisibility. . . . the shoemaker has

only so much ability to learn (from instruction and practice), and the more he learns of one process, the less he learns of the others. This single unit of learning ability, then, is cultivated subject to diminishing returns. (1942:134)

In the long run, the shoemaker has no greater mental capacities, hence efforts to expand beyond what a single person can do must generate decreasing returns.

“Management and control, the entrepreneurial functions” are likewise a “human factor” (1942:134). In principle all factors should be variable, but Stigler grounded this particular source of decreasing productivity in the “intensive utilization of the learning ability of a man” (1942:138). He continued,

What is true of the laborer is even more true of the entrepreneur. The ability to administer and to make decisions, which is one of the most diverse of all “natural” endowments of men, can be utilized fully in a large plant, where most (but not all) of the details and routine may be delegated. (1942:138)

His reference to “full utilization” implies that efforts to exceed that optimum would impose decreasing returns.

This analysis could be directly applied to the limitations on an owner-operator trying to manage an expanding concern, whose individual abilities would be finite. But if Stigler’s management were limited to the case of a lone entrepreneur, his theory could be legitimately criticized as inapplicable to the world of publicly owned corporations whose owners are legally and *de facto* distinct from their hands-on managers. Is Stigler’s “management” one person or many? He straddled the issue, making an argument based on the limitations of a single mind to motivate his theory of the indivisibilities of management without addressing the distinct character of management once it is separated from ownership.

When Stigler turned from the entrepreneur to management *per se*, he alternated between two of the meanings of the word: management as a hierarchy of personnel who make decisions based on uniform criteria, and management as a task that may or may not ultimately be executed by one person. Stigler described the task of coordinating large groups as more difficult than for smaller units, without explaining why this should be true. The magnitudes of input and output flows for a

small plant will be smaller than for a large plant, but the coordinating task is in some degree comparable, and as Robinson recognized there may be economies of scale rather than diseconomies. For instance, centralized purchasing, which encompasses coordinating input inventories, would experience economies of scale (1932:64; see also Stigler 1958:134).

Robinson emphasized coordination across multiple units rather than the monitoring within a large unit as the more common difficult task.²¹ Stigler described a trade-off between delegation of authority in a managerial hierarchy, which implies "no unity of policy or uniformity of performance" (1942:138), as against centralized decision making, which implies delays, "red tape," etc., echoing arguments Robinson had made (1932:43–44).

For Stigler, the human-resource problem was summarized by a return to the perspective of the lone entrepreneur as final authority. "[M]anagement, and control in general, inherently face a problem: the final authority to make decisions cannot be subdivided or delegated" (1942:138). This echoed his initial argument that the mental capacity of a single individual set a limit to the advantages of any division of labor. He maintained that management could be seen "as an indivisible productive service, so that as the firm grows in size, the coordinating and decision-making faculties are used more intensively subject to diminishing returns" (1942:134).

Can Diseconomies be Avoided?

Stigler claimed that "Between these two extremes [of inefficient bureaucracy and chaotic decentralization] the large firm attempts to steer a middle course, but it never achieves that compactness, flexibility, and singleness of purpose which are possessed by every well-managed medium-sized firm" (1942:138). Stigler did not address why medium-sized plants cannot be as responsive as his medium-sized firms, yet retain through a central administration the beneficial economies of scale for research and marketing he himself recognized.

Robinson (1932:108–116) had identified several strategies whereby large concerns could avoid diseconomies of scale. Foremost was what is today called decentralization:

the breaking up of the firm into various wholly separate departments. . . . Their work is coordinated and planned by the central management. The local managerial staff is usually of very small dimensions . . . to manage the plant and keep the records. . . . In many large firms the division between departments is a very rigid boundary, and the departments are coordinated only by the financial and planning control of the higher administration. In this way the unit of actual organization [at the local level] is kept sufficiently small to be manageable by persons of the ability that is available." (1932:108–109)

Here Robinson anticipated Stigler's focus on the ability of the individual, but provided the ready solution to any such limitation of talent or capacity in the form of a decentralized management structure.

A second means to counteract coordination failure was vertical disintegration, whereby the company divests itself of a production or managerial process that would otherwise suffer scale diseconomies. Finally, Robinson was cognizant that "[i]n no department of industry has progress been so great in the last ten years as in that of management" (1932:47; see also p. 95). Robinson described how "even in the office the machine is rapidly making a place for itself" (1932:40). He detailed the various organizational structures that had evolved to streamline management's coordinating effectiveness.²² While Stigler and Robinson (1932:47) shared the view that ultimately management difficulties would constrain the size of the corporation, Stigler's *Principles* addressed none of the counter-influences Robinson had detailed.

Conclusion

It is ironic that Stigler's paradigmatic textbook for Perfect Competition would draw its explanation for constraints on company size from an author who simultaneously argued that competition, while real, is not Perfect (Robinson 1932:9). For Robinson (1932:9), competition in practice was closer to what Stigler's Chicago compatriot, Joel Dean (1951), would describe in his own trend-setting graduate MBA economics textbook: Imperfect Competition in a world of uncertainty and change, with constant costs for the plant if not the corporation.²³

Stigler was not writing to discover how the economy worked, his textbook was designed to show the clarity of vision provided by a particular analytical lens in an environment where other alternatives

were also emerging. Perhaps Stigler's definitiveness should be understood in the context of a paradigm under challenge by Institutionalists and Keynesians in the policy arena as well as in intellectual venues. He strove to provide unerring leadership for an economics based on scarcity, with managerial capacity as the ultimate scarce resource. His goal was to elucidate the scientific basis for successful prediction and therefore policy intervention, or as he put it, control (1942:3).²⁴

VI

**Hammond and Hammond on the Stigler Letters
and Correspondence**

WHEN GEORGE STIGLER DIED it was most appropriate that his close friend and indefatigable colleague, Milton Friedman, prepare the biographical memoir for the National Academy of Sciences. In that memoir Friedman wrote that "for nearly sixty years he was either my closest friend or one of my closest friends. My debt to him, both personal and professional, is beyond measure" (Friedman <http://www.nap.edu/html/biomens/gstigler/html>, p. 1). Their relationship harked back to their student days at the University of Chicago when they and their future wives, Margaret "Chick" Mack and Rose Director, along with another economics student, W. Allen Wallis, and his future wife, Anne Armstrong, became, as Friedman described them, a "sextuple whose lives were intertwined from then on" (Friedman <http://www.nap.edu/html/biomens/gstigler/html>, p. 2).

Stigler and Friedman were professional colleagues during World War II, working for Allen Wallis at Columbia University in the Statistical Research Group. When the war ended, Stigler returned to his pre-war faculty position at the University of Minnesota and helped Friedman obtain a faculty position there teaching economics and statistics. Their friendship deepened at Minnesota, as they shared an office and collaborated on *Roofs or Ceilings? The Current Housing Problem* (1946). Stigler also completed revisions of *The Theory of Competitive Price* (1942). At the year's end they both left Minnesota for faculty positions elsewhere, Stigler at Brown University and Friedman

at the University of Chicago, where he took over for Jacob Viner in teaching price theory.

Having parted, the two began corresponding regularly by letter. Friedman initiated a discussion of price theory in an August 12, 1946 letter. This discussion continued in more than 50 of the letters stretching well into the 1950s. These and their other letters are in the George Stigler Papers at the Regenstein Library, University of Chicago, and the Milton Friedman Papers at the Hoover Institution, Stanford University.²⁵ Three of the letters are reproduced here.

The August 12, 1946 letter is reproduced as Exhibit 3. The opening paragraph refers to the impending publication of *Roofs or Ceilings* (Friedman and Stigler 1946). In the second paragraph Friedman questions Stigler's critique in *The Theory of Price* of Alfred Marshall's proofs of the Law of Diminishing Returns.

Stigler responded in a letter that we estimate was written on or about August 19, 1946 (Exhibit 4). This letter opens (as Friedman's previous letter had opened) with news about their forthcoming essay on rent control (Friedman and Stigler 1946). Stigler goes on to tease Friedman about waiting so long before reading *The Theory of Price* (Stigler 1946a). Had Friedman taken a look at the book a few months earlier they could have discussed the subject in the University of Minnesota office they shared. In this letter Stigler offers several methodological distinctions about proof and what proof means in economics, in addition to responding to Friedman's queries about the Law of Diminishing Returns.

In 1947, Stigler moved from Brown to Columbia University where he stayed until 1958, when Allen Wallis, then Dean of the Graduate School of Business, persuaded him to accept the Charles R. Walgreen Professorship of American Institutions at the University of Chicago. This reunited the sextuple of friends in the same academic community where they first met. From 1945 until Stigler relocated to Chicago, Stigler and Friedman exchanged approximately 100 letters. Alfred Marshall's economics was a frequent subject of their letters. Consider the letter reproduced in Exhibit 5, which we date at approximately September 1948. Stigler responds (1) to a draft of Friedman's article on the Marshallian demand curve (Friedman 1949) and

Exhibit 3

August 12, 1946

Dear George:

I got the carbon of the revised rent article this morning and can find only one typographical error in it (p. 3, line 5 under I, wherever should be whenever). I think I independently sent you my Portland address: 335 S. W. Woods St.

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

Says Marshall (p. 150) "We learn from history and by observation that every agriculturist in every age and clime desires to have the use of a good deal of land; and that when he cannot get it freely, he will pay for it, if he has the means. If he thought that he would get a good result by applying all his capital and labour to a very small piece, he would not pay for any but a very small piece." To restate: If the law of diminishing returns were not true; 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. Our hypothesis leads us to expect a certain result, we find that result, hence our hypothesis is not contradicted.

True so far as it goes, says Stigler, but there is a hidden assumption, namely that the whole production function is of a very special class (of which linear homogeneous is the chief example). Stated differently: the hypothesis of diminishing returns to variable proportions is not enough to explain the general desire for land; nor, if properly supplemented, is it the only hypothesis that will explain this fact. Suppose additionally labor and capital are applied to a piece of land yielded diminishing returns, but increasing labor, capital, and land yielded still more rapidly diminishing returns; then people would still want till very small plots and let the rest of the land lie idle. But this hypothesis is contradicted by the facts, so can be rejected. Suppose, now, additional units of capital and labor yielded constant or increasing returns when applied to the same piece of land, but increasing capital, labor, and land yielded more rapidly increasing returns. It would then follow that people would have a great desire for land, as we observe. This is Stigler's sound

Exhibit 3 (continued)

2

point. The facts Marshall adduces are consistent with his hypothesis; but, says Stigler, they are also consistent with an alternative hypothesis.

To rehabilitate Marshall, it is only necessary to see what ~~inferred~~ flows from the alternative hypothesis. Does not this alternative hypothesis imply that whoever got started first in adding land, capital, and labor to his initial supply could outbid anyone else just getting started? Hence, if the alternative hypothesis were valid, we should expect to observe not only that men everywhere desire to have the use of a good deal of land but that everywhere ~~only~~ a few (perhaps only one) and only a few succeed. Agriculture would be organized in gigantic farms and most farmers would be hired laborers. This we do not observe; hence the alternative hypothesis is contradicted; and the original remains the only simple hypothesis consistent with the observed facts. R

Can we go one stage farther and rehabilitate Stigler by taking account of the fixed factor of entrepreneurship? I think not, though I haven't thought this through clearly. The attempted rehabilitation would proceed by setting up another alternative hypothesis consistent with the two facts so far stated. Suppose there is increasing returns to additional labor and capital applied to a fixed amount of land and entrepreneurship, but still more rapidly increasing returns to additional labor, capital, and land applied to a fixed amount of entrepreneurship, and still more rapidly increasing returns to an increase in labor, capital, land and entrepreneurship. Then since one individual can provide only a limited amount of entrepreneurship, the two observed facts would follow, together, of course with enormous increasing external economies to the agricultural industry (since I have two entrepreneurs with two units of labor, capital, and land producing more than twice what one ~~unit~~ entrepreneur with one unit of each produces). But are these for the world industry, or for one nation? If the latter, the nation that started first would presumably eclipse the world. And of course, I am, in effect, denying the validity of experimentation: the same circumstances do not give the same result; but that is on a different level. In any event, I doubt that you will like this rehabilitation, though I should leave that for you to say.

You may ask, why all this fuss when Stigler accepts the law on other grounds, namely, technological experiments. The reason is that economic empirical evidence of the kind given by Marshall 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.

And so to close, while you contemplate ecstatically the increasing returns from size of house and estate

Yours,

Exhibit 4

My dear collaborator:

I called up the boys on Saturday morning and found that the damn thing was already in type and they weren't promising that they would be able to change anything. Nevertheless I read my list to them and then mailed them a copy. We shall see.

Let me say at the outset, in commenting on rent, that I resent comparisons with Marshall, who did not even play a good game of auction.

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:

1. Because you are freed of dependence on non-economic data? Perhaps, but this is clearly a move in the direction of a closed, formal system - which you don't like.
2. Because it is more efficient? That depends on the case.
3. Because it is more elegant? No, this is pure formalism.
4. Because?

The big trouble with an indirect proof is that by the time it becomes rigorous it is usually terribly complex and cumbersome. Of course you did not even begin to fill the holes in Marshall's argument. How about non-economic institutions, such as inheritance? How do you handle constant returns, which would presumably lead to wide variety of size (which we observe) and yet, with just a touch of entrepreneurial diseconomies, does not get out of hand. How do you know that your demonstration that one nation may monopolize a commodity does not explain a considerable range of facts?

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.

As a digression, it is worth remembering that Marshall often, perhaps usually, thought of diminishing returns as an historical law describing the returns to land as population grew. And I doubt that this was in any real sense scientific; it was an idea acquired from earlier economists that casual observation did not refute.

And in any case, only a crackpot would spend 7 months staring at the ceiling and then suddenly begin to read a book by the now ex-officemate when he knew in advance that he wanted to argue about it.

George

Exhibit 5

Dear Milt:

I am a weary housewife, but still have enough energy to write a confused letter. Two weeks ago we left Canada and stopped the first night at Greensburg, Pa., to visit Chick's brother for a day. Jerry started running a temperature and went to the hospital with inflamed ears. An X-ray then revealed pneumonia, and he is still in the hospital, mending but slowly - its virus, and not amenable to penicillin. I left with the boys 10 days ago to start them in school, and have been holding down the fort with the boys - and the neighbor's help - since. I hope that Chick and Jerry will get back by the end of this week.

Your utility article is splendid in its revision. I'm amazed at the thoroughness with which you revised the first draft.

On Marshall, I've done a little reading and would like your comments on the following:

1. Edgeworth's article on demand curves in Palgrave's Dictionary, which surely reached Marshall's notice but was not, I'd guess, revised on the critical print *(in my form)* in later ~~editions~~.
2. In the pure theory of domestic values, Marshall gives a definition of the price axis, p. 3, that would be ambiguous on your interpretation. See also p. 15.
3. How can the Friedman demand curves of different individuals be added? Since the individuals use different quantities of various goods, there must be different compensating changes in other prices, so the functions will not have common arguments. Yet Marshall added the demand curves of different classes.
4. Marshall says purchasing power pertains to the country and not to the individual (Memorials, p. 207).
5. Principles, pl 109, to cite again: "The purchasing power of money is continually changing..." This is sensible as an observation on a Fisher P; it is less obviously valid in discussing reaction of quantity of sugar to price. And when he says that the Broader Changes can be measured roughly, again he must have a cost of living index in mind. For surely the variations appropriate to sugar, etc. price fluctuations are small, and hence he has a unemployable technical apparatus on your reading.

On the methodology, I'll also think some more. 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 this worth looking into. If I predict basing points in industries where Yale men are over Princeton men, and love to rib Petter'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 predictions, good theories would be chance events, as likely to come from Seymour Harris as now more likely, - than from Smith or Marshall. Only an infinitesimal portion of scientific work would be rewarding. And I don't believe this.)

Arthur, I've just heard from Moore, wants me to write another service industry monograph. It will be interesting to see whether I can be overpowered.

Regards,

George

*can you spare
any 5 minutes
so my student
can work on
it?*

(2) to an early version of Friedman's famous essay on economic methodology (Friedman 1948). Stigler also praises Friedman's article coauthored with L. J. Savage on the analysis of utility maximization under conditions of risk (Friedman and Savage 1948).

We present these three letters as evidence of how these two historically important figures in 20th century economics worked out ideas in private discussion and debate prior to their dissemination in books, articles and classroom lectures. The letters convey their affection and rapport as close friends, along with the intellectual tenacity and rigor that marked two future Nobel laureates.

VII

Medema on Stigler as Promoter of the Coase Theorem

THE DISSEMINATION OF IDEAS is a function both of the elaboration of the ideas themselves and how subsequent commentators use those ideas. At times, and indeed perhaps most of the time, this process appears to be relatively straightforward. In other instances, however, the transmission of ideas would appear to the historian to take a detour. Such is the case of the Coase theorem. George Stigler left a substantial and highly influential legacy within economics, one significant, but relatively unknown (outside of Chicago) element of which is his role in the promulgation of the Coase theorem.

In "The Problem of Social Cost," Ronald Coase set out to show that the Pigovian framework for the analysis of externalities was fundamentally flawed. The Pigovian approach saw externalities as an instance of market failure that could only be remedied through the application of appropriately-specified tax, subsidy or regulatory instruments imposed by the government. Coase demonstrated, first, that under standard neoclassical assumptions—those underlying the Pigovian analysis—and, specifically, the absence of transaction costs, private, market-like mechanisms would assure the efficient resolution of the externality problem so long as property rights were assigned over the resource in question. Coase *then* went on to argue that both the Pigovian analysis of externality correction and his own discussion of how market-like mechanisms would work were fundamentally flawed because the former ignored the costs associated with govern-

ment activity and the latter the costs associated with the operation of markets. This leaves us, according to Coase, with a choice among imperfect alternatives, and the solutions to the problem of externalities can thus only properly be dealt with by engaging in comparative institutional analysis.

Stigler's take on all of this seems to have been rather different from that of Coase. There is no doubt that Coase's analysis sparked something in Stigler, as he relates in print on two different occasions what he has called the "Eureka" story—the story of how Coase, then at the University of Virginia, came to Chicago to present his work on the allocation of broadcast frequencies using the market mechanism. The story relates to an evening at the home of Aaron Director and bears brief repetition here, as it speaks to the impact that Coase's insight had on Stigler.

At the beginning of the evening we took a vote and there were twenty votes for Pigou and one for Ronald, and if Ronald had not been allowed to vote it would have been even more one-sided.

The discussion began. . . . My recollection is that Ronald didn't persuade us. But he refused to yield to all our erroneous arguments. Milton would hit him from one side, then from another, then from another. Then to our horror Milton missed him and hit us. At the end of that evening the vote had changed. There were twenty-one votes for Ronald and no votes for Pigou (Stigler, quoted in Kitch 1983:221).

Commenting on this evening some two decades later, Stigler called it "one of the most exciting intellectual events of my life" (Stigler, quoted in Kitch 1983:221; see also Stigler 1988:73–90).

Such was the influence of this idea on Stigler that, in the next edition of his classic text, *The Theory of Price*, we find a discussion of Coase's analysis of externalities—or a part of it, at least. In the process of presenting the externality problem, Stigler introduces the reader to "The Coase theorem," claiming that "The Coase theorem thus asserts that under perfect competition private and social costs will be equal" (Stigler 1966:113). Perhaps reflecting the debate at Director's home a few years earlier—not to mention the pervasiveness of Pigou's influence, including on his own thinking—Stigler goes on to remark that "It is a more remarkable proposition to us older economists who have believed the opposite for a generation, than it will appear to the younger reader who was never wrong here" (*ibid.*).

That Stigler would have chosen to focus on the zero transaction

costs world that Coase laid out as a Pigovian-rebutting fiction is at once surprising and not surprising. It is surprising in the sense that it was Stigler who, shortly before Coase's social cost analysis, pioneered the analysis of information problems in economics (Stigler 1961), and transaction cost problems are, after all, ultimately reducible to problems of information (Dahlman 1979). Set against this, however, we have Stigler's seemingly unflagging belief in the efficacy, and efficiency, of markets, evidenced throughout his writings and placing him four-square in a leadership position in promulgating the so-called "Chicago view of the world." This view served to distinguish Chicago from both the interventionist ideology often associated with institutionalism (on life support but not immune from Stigler's criticism) and the emergent neoclassicism that purported to show, with great mathematical (read: "scientific") flourish, that government prescriptions could cure all manner of market failures. Stigler showed us the virtues of markets at a time when the performance of markets was being increasingly called into question. While Coase's position was that the governmental cure was often worse than the disease, Stigler's stance was that the patient was in all respects quite healthy.

The simplistic take on Stigler's introduction of the Coase theorem into economic discourse and analysis is—depending on one's perspective—that he got economists (a) to understand how gloriously markets work in all manner of contexts or (b) to occupy themselves with a piece of theoretical minutia that is either wrong or bears no correspondence to reality. But that would be to miss the import of the Coase theorem. For the theorem provides the theoretical underpinnings of modern law and economics and, most importantly, for its normative prescription that rights should be assigned so as to comport with the dictates of efficiency. The logic here is straightforward: the Coase theorem says that people will voluntarily bargain to efficient rights assignments if not precluded from doing so. Thus, in instances where rights claims are litigated, transaction costs must be sufficiently high to preclude this bargaining process. As such, the courts should assign rights in the way that these contestants would have voluntarily agreed to if the transaction costs had not precluded the bargain (see Medema 1999).

It is interesting to note that Coase, upon being honored as one of the "four founders" of the modern law and economics movement by

the American Law and Economics Association, stepped to the podium and managed to disavow his progeny—something he has done on multiple occasions. Stigler was not among those honored by this group, yet the irony here, of course, is that a case can be made that it was Stigler's *use* of Coase's ideas that provided law and economics with such a crucial component of its theoretical apparatus—a task for which Coase, whose interests were very much elsewhere, had little taste. Coase's interest has been in attempting to uncover the institutional underpinnings of the market system, whereas Stigler's preference was to take them as given.

Shortly before his death, Stigler penned some brief "Notes on the Coase Theorem" that appeared in the *Yale Law Journal* following a paper alleging to have shown via experimental evidence that the Coase theorem is in error. In his commentary, Stigler asserts that "Ronald Coase taught us, what of course we should already have known, that when it is to the benefit of people to reach an agreement, they will seek to reach it" (Stigler 1989:631). He goes on, however, to allow that, in many instances, the benefits of reaching an agreement will be outweighed by the costs. But by this point, of course, the horse was long since out of the barn and an entire edifice of legal-economic analysis had been built up around the Coase theorem. Somehow, I expect that Stigler was pleased.

VIII

Samuels on Stigler as Political Economist

IT IS IRONIC, PERHAPS, but nonetheless true that understanding the tall, gruff, sarcastic, caustic, humorous, larger-than-life, even overpowering George Stigler requires more attention to subtlety and nuance than I can develop in these remarks. This predicament is due in part to the nature of the relevant topics, in part to his frequently ideological mode or level of discourse and in part to the fact that he seems to have chosen to emphasize only a few slices of a larger and more complex analysis; or so it appears to me.

For reasons that I am not sure I fully comprehend, George behaved uniformly pleasantly to me. Both he and my colleague Walter Adams, about whom more in a moment, could aim caustic barrages upon ob-

jects of their respective scorn. Walter and I were colleagues in the same department, for a period with catty-corner offices, and we were friends, but that did not prevent Walter from directing ammunition my way. Although George and I saw each other only occasionally at meetings, George and I were friends without any cannonballs hurled my way. Like Walter and I, George and I had a mutual respect for each other; or so I like to think.

We agreed on a great deal but also disagreed on very much; and as is often the case both the areas of agreement and disagreement are important. George was a neoclassicist, a certain type of neoclassicist; if he was more than a neoclassicist he was also an Austrian economist; his version of neoclassicism was an interesting blend of Marshallian and Austrian economics. I am more eclectic than George, and certainly an institutionalist of the old school. In any event, I think he appreciated not only that I took the Chicago School seriously but that I lauded the old, Frank Knight-Henry Simons version of the School relative to his version.

George believed not only in neoclassicism but in the Chicago version of neoclassical economics, at least that version of Chicago economics of which he, perhaps even more so than Milton Friedman, was the leader. George was its chief cheerleader, its foremost practitioner, the principal, but by no means the sole, author of absolutist formulations of neoclassical doctrine. He sought both to defend it against all comers and to extend it by reformulation to absorb what hitherto had been excluded. What had been for Knight "only" a relatively absolute absolute was for George an undiluted absolute. And Knight treated those who disagreed with him better than did George, for example, in the matter of Gardiner C. Means. George wanted, in the words of Jim Buchanan, to create an intellectual fortress, one that was both impregnable and emitted no hint of susceptibility or peril.

George was no man's fool, however; nor was he anyone's tool; he had an enormous strength of character. He supported politicians whose ideology and policies were congruent with his own. He was a sometime advisor to Ronald Reagan. But he had a visceral dubiety and antipathy to politicians per se and to politics in general, even those he supported. We recall the reception given him October 27, 1982 by the Reagan White House upon the announcement that he had been

awarded the Nobel Prize in economics. At a news conference he was asked about the current economic situation. He called it, quite properly given conventional economic definitions, a depression. He called so-called supply-side economics a gimmick. He assigned the grade of “incomplete” to President Reagan on the economy. The response was to turn off the power to the podium and to lead George from the press briefing room.

There were two fundamental differences between George and me on the theory of economic policy. By drawing the comparison, however, by no means do I want to suggest that I was his equal in influence.

First, I, together with Walter Adams and Harry Trebing, favored a market economy. But we were wary of a capitalist economy if such meant a capitalist-dominated structure of social power. And we did not believe that a capitalist, or market, economy was *necessarily* a competitive economy. George, however, not only favored a market economy, he both equated it with a capitalist economy and believed that such an economy was *necessarily and intrinsically* a competitive economy. Perhaps George was only marginally less skeptical of capitalists than he was of others; but he was more outspoken about the others. We both accepted versions of the “capture” theory of public utility regulation—though for George this theory meant that regulation was absurd whereas for me the theory only identified a predicament requiring continuous attention.

Second, Adams, Trebing and I had a feeling that George neglected and thereby obfuscated the deep and fundamental economic role of government in a market, or capitalist, economy and in the larger system of legal-economic, private-public governance. In our view, government is already present in the status quo; government is inevitably present; the issue is always whose interest is to count; and, *inter alia*, intervention, so called, involves changing the interest to which government is giving its support and not the introduction of government into a situation in which it hitherto has been absent. *Inter alia* includes a view of Coase’s work quite the opposite of what George called the Coase theorem—a view that, as it turns out, is Coase’s own.

In both these respects we felt much closer to Frank Knight than to George. Like Knight—but much less like George—Walter, Harry and I

had a deep, if reluctant, regard for government and politics as a peaceful mode of working out solutions to problems that existed, or would have existed, independent of government and politics, however much they took their form from government and politics.

In short, we had a mixed view of George's politics, his scholarship and the impact of his powerful personality. But he surely had a mixed view of ours as well. The combination of these mixed views made our relationships that much more interesting.

IX

Friedland on Stigler and Economic Policy

AT GEORGE STIGLER'S PRESIDENTIAL ADDRESS to the American Economic Association in December of 1964 titled "The Economist and the State," he said, "A theory can usually be made to support diverse policy positions . . . [and] which part of a theory is decisive is a matter of empirical evidence" (1965:131). I would like to talk today about the path along which this distrust of theory, untested by empirical evidence, led him in his thinking over the 25 years that followed his 1964 address.

I will briefly flash back from 1964 to 1954 when Stigler's interest in the role of empiricism in the methodology of economics was in its infancy. In his 1954 essay, "The Early History of Empirical Studies of Consumer Behavior," Stigler observed, "Utility theorists as a class have always expressed the greatest enthusiasm for empirical work compatible with abstention from it" (1965a:198). Stigler went on to show great admiration for the late eighteenth-century collectors of consumer budgets, such as Frederick Morton Eden and David Davies, and to express his profound regret that their work was not integrated into consumption theory (by Quetelet, Ernst Engel and others) until after the revolutionary activity of the 1840s, coupled with the new mathematical theories of probability of LaPlace, Cournot, Poisson and Gauss. I know I'll surprise many of you who thought of Stigler as an anti-Keynesian by quoting from his summary paragraph on the discussion of demand curves in which Stigler lets John Neville Keynes express his methodological position: "the functions of statistics in economic theory are to suggest empirical laws . . . to supplement deductive reason-

ing by checking its results, and submitting them to the test of experience . . . [in the] interpretation of particular concrete phenomena" (1965a:232–33). In 1965 Stigler published his collection *Essays in the History of Economics* (1965a), adding a statistical study of the development of professionalism in economics (1965a:31–50). Thus, in the decade beginning in 1954, Stigler had so dedicated himself to empiricism and its role in the deductive-inductive process that he himself was applying it in that most unlikely area, the history of economic thought.

I must return to the twentieth century and my primary topic. As you all know, the deductive-inductive process of scientific reasoning derives its name from the need to deduce relationships and then *induce* someone to pay for their empirical testing. It is here I'd like to introduce two characters into my story who were necessary to Stigler's development of his theories of the economics of politics: Charles R. Walgreen and W. Allen Wallis. Charles Walgreen, the drugstore tycoon, having caused a very disruptive Red scare at the University of Chicago in 1936, soothed his conscience by giving to the university a great deal of money, and then saved face by specifying that it go to hiring a professor of "American Institutions." As this was a non-existent field of study at the time, the grant accumulated interest for 20 years, until W. Allen Wallis, the new dean of the Graduate School of Business, had the imagination to suggest in 1958 that *business* was an American Institution, thus obtaining control of the Walgreen grant and allowing him to hire away from Columbia his fellow graduate student and friend from the very days of the Walgreen Red scare, George Stigler. Stigler was the first Walgreen professor (Bob Fogel was the second), with an unheard-of salary (some say 25,000 1958-dollars) and, equally unheard of in economics at the time, a full-time research assistant, myself.

Stigler, in this pre-computer world, immediately set me to testing his theories: first the "Economics of Information" (1961), relating the cost of information to price divergence, and in 1962, "Information in the Labor Market," applying and testing his 1961 theory with regard to the dispersion of wage rates. To be specific, testing whether "workers will search for wage offers . . . until the expected marginal return equals the marginal cost of search" (1962:96). Obvious, but new at the time.

I'll come back to this information cost theory and its key role in his political theory.

When Stigler made his call to arms with regard to evidence that I quoted at the outset of my talk, we were completing what appeared to be the first systematic attempt to measure the effects of public utility regulation, rather than assuming that the effects were in the intended and anticipated direction (Stigler and Friedland 1962²⁶). Our study, though marred by my embarrassing calculation error (my first attempt to enter the electronic world²⁷) found no significant effect of regulation on electric rates, and in fact found some tendency of regulated rates to favor commercial over residential consumers. (I should add here that as early as 1946 in his study of minimum wage laws, Stigler had found no relation between legislated changes in relative wages and the level of poverty (1946a).) Our electric rate result was the stimulus for a mountain of subsequent empirical studies, by Stigler, his students, and many others, on the effects of regulation.²⁸ Many of these found regulation to have no effects, perverse effects (protecting high market share, established firms from entrants, discounters, imports or substitute products), or, at the very least, "unanticipated" effects. I've put unanticipated in quotes because Stigler then asked himself why these perverse results, and why the apparent frequent "capture" of the regulators by the major firms in the industry. (Stigler ended a debate with former SEC chairman Manuel Cohen with his famous remark, "It is of regulation that the consumer must beware" (Stigler and Cohen 1971:17).

Stigler saw the now-familiar capture problem as giving a name to our ignorance. Why the capture? Was this a series of simply mistaken appointments to regulatory bodies? Mistakenly designed legislation? He often said that a mistake is something we don't understand, yet.²⁹ The idea that bureaucrats are less competent than market actors, that they might be corrupt or that they lack monetary incentives has been around since Adam Smith. In Stigler's "Smith's Travels on the Ship of State" (1982 [1971]) he reminds us of Adam Smith's very modern view that "all legislation with important economic effects is the calculated achievement of interested economic classes" (1982 [1971]:139, Stigler's paraphrase).

In 1971, almost 200 years after *The Wealth of Nations*, Stigler added

to Smith's generalization his own insight from his 1961 "Economics of Information": in a democracy, the cost to the consumer of informing himself on political issues of regulatory policy make it likely that the system will implement "strongly felt preferences of minorities [over] lesser preferences of majorities and minorities" (1988b [1971]:220). For those with large stakes in the political outcome (e.g., firms seeking subsidies, protection and other government favors), there is a large incentive for devoting assets to rallying votes, employing skilled specialists (lobbyists) and providing sympathetic legislators, and political parties, with information and campaign contributions. Thus in his 1971 "The Theory of Economic Regulation" (1988[1971]), Stigler went beyond the capture-competence question to ask some highly political questions: Were market interventions deliberate attempts to redistribute income? If so, from whom to whom, and what determined the timing and form of these interventions and the identities of the winners and losers? Stigler's attempts to pin down the answers occupied us for the next 20 years. In those years his dear friends Sam Peltzman (1976) and Gary Becker (1985) and many others also refined, improved and tested Stigler's special interest theory. At the time of Stigler's death in 1991, we were making empirical observations on the precise characteristics of the (hypothesized) winners and losers in regulatory redistributions through agricultural and welfare policies, as well as the rent control policies about which Friedman and Stigler had collaborated 45 years before (Friedman and Stigler 1946).

In Stigler's later years, he developed something that, if I weren't talking about George Stigler, I might call a doubt. Not about the special interest theory itself, but about the legitimacy of the gains achieved by these special interests. This reevaluation was not articulated beyond a narrow audience, which I would like to broaden somewhat today. The audience was indeed narrow: the Hume Institute of Scotland in 1986 and a Zurich audience assembled by the Bank Hofmann in 1988 (1989a). At the Hume lecture he refined the special interest theory and offered an explanation of persistent trends in regulation in the conditions of economic life that make possible government transfers among economic actors; he presented this as an alternative to ideological explanations, using the prevalence of *laissez-faire* in the nineteenth-century American economy as a test

(1986:6). In a very different second test he gave his view of modern financial market deregulation as *following* the de facto breakdown of the old financial structure as a result of the prevalence of institutional investors and the growth of competition from nonbank banks.³⁰ Thus he found changing economic conditions a sufficient explanation for deregulation without recourse to “a revulsion against public regulation: In explaining why one does not walk from Dover to Calais it should suffice to point to the Channel, without adding that such a long walk would be fatiguing” (1986:8). The rethinking of the self-interest hypothesis in this talk represents Stigler’s search for an efficiency explanation for the role of government in serving special interests: “we use coal when it is the most efficient resource to heat our houses and power our factories. Similarly we use the state to build our roads or tax our consumers when the state is the most efficient way to reach these goals” (1986:3–4).

Just as he had combined Adam Smith’s self-interest concept with his own cost of information theory to arrive at the 1971 political theory of regulation, Stigler, speaking in Zurich in 1988, brought two of Chicago’s favorite concepts, revealed preference and consumer sovereignty,³¹ to shed light, or perhaps cast a shadow, on his political theory of regulation: to find the income redistribution Americans expect from their governments look to “the actual practices of the times” (1989a:42). If the Clean Air Act protects Eastern coal markets from the Western coal industry³² and if the cost of the Act is borne by electricity consumers, both these effects reflect the revealed preference of the American public. “Who has a better right to interpret the public interest than a legislature [democratically] chosen by the people . . . ?” (1989a:45).

I want to assure you that Stigler shows, elsewhere in his work, no naïveté about problems of legislative representation (log-rolling, party discipline, etc.): in his Zurich speech he allowed that it is not surprising “that a general tax or trade law runs a thousand pages—after all, that is only two pages per congressman” (1989a:49). Under the heading “The problem of legitimacy,” he acknowledges “Many people are uncomfortable with the notion that every act of a sovereign legislature is by definition an act to improve the [social] welfare of the society. How can all favors obtained by special interest groups be considered

social improvements?" (1989a:45). Thus Stigler, famous for having taken outspoken, extreme (especially in debates) anti-regulatory stances throughout his career, suddenly finds himself turning on its head another favorite Chicago (and Stigler) concept, efficiency.

I shall end my odd essay on Stigler's odyssey, from measuring the effects of economic regulation to explaining that legislation, with his own un-Stigler-like formulation of this puzzle: "The answer must be that we have no more authoritative criterion of the social welfare. All other criteria lead to proposals to change public policy that have not been accepted by the people's duly elected representatives" (1989a: 45–46). As Stigler never resolved this dilemma, I leave it for others to contemplate.

Notes

1. Stigler (1966:68): "Some economists, disenchanted with the subjective overtones of utility theory, have resorted to an alternative approach, that of revealed preference. The philosophy of this school was well expressed by Bernard Mandeville, a most penetrating man:

I don't call things Pleasures which Men say are best, but such as they seem to be most pleased with; . . . John never cuts any Pudding, but just enough that you can't say he took none; this little Bit, after much chomping and chewing you see goes down with him like chopp'd Hay; after that he falls upon the Beef with a voracious Appetite, and crames himself up to his Throat. Is it not provoking to hear John cry every Day that Pudding is all his delight, and that he don't value the Beef of a Farthing?

The essence of the approach is to look at observed behavior, and from it deduce certain properties of tastes."

2. My dissertation is entitled "The Content of Classical Economic Theory." It was approved by George Stigler in 1979 after two chapters from that dissertation had been published in *History of Political Economy*.

3. Two at Berkeley from J. M. Letiche; at Chicago one from Bert Hoselitz; and one from Roger Weiss.

4. Stigler (1975:107): "Coase asserted an almost incredible proposition: if transaction costs were zero, the theorem on maximum satisfaction would always hold, no matter how the rights and duties of parties were assigned by the law . . . In this regime of zero transaction costs, no monopoly would restrict output below the optimum level because consumers would pay the monopolist not to do so. Such miraculous corollaries of the Coase theorem greatly enlivened an early week in courses in economic theory."

5. I suggested that one might detect a Kuhnian revolution by attending to

the fact that Kuhn (correctly) pointed out: after every "revolution" textbooks acquire new heroes. Thus, after the advent of non-standard analysis, Bishop Berkeley shows up in calculus textbooks! This awakening of interest in new authors ought to be something that could be detected by studying changing prices in the rare book market. Stigler refused to let me try this. He used the word "heart-breaking" to describe the difficulties I would face. I think he was prematurely pessimistic about this.

6.

The chief fault, then, in English economists at the beginning of the century was not that they ignored history and statistics; but that Ricardo and his followers neglected a large group of facts, and a method of studying facts that we now see to be of primary importance.

They regarded man as, so to speak, a constant quantity, and gave themselves little trouble to study his variations. . . . (Marshall 1885:154–55)

7. Francis Yates would give scholars the key to that door (Levy 1992).

8. It took nearly 20 years and an appreciation of experimental economics to make sense of something I learned in Stigler's lectures: in Adam Smith's argument, trade depends upon language (Levy 1992). Therefore, in Smith's formulation, trade can be used to identify the human in a way that our rational choice axioms cannot (Levy 2001b, 2001c).

9. Stigler's persistent attack on the hypothesis of time preference, an area in which he remained loyal to Frank Knight's teaching (Levy 1995), has an anti-racist history (Peart and Levy 2000).

10. One of the problems on a mid-1960s price theory core exam from which I studied was this: "Never a charge for credit' is the same as 'never a discount for cash.' How can economics explain the behavior of people who do not know these are equivalent?" As a Berkeley graduate with an interest in logic, I rather naturally thought in terms of a difference between theory and metatheory: the subjects of our theories need not have the same theories we do. (Levy 1992 continues this thought.) Stigler told me that he had no idea how to answer his question. The problem-centric nature of Chicago was impressed upon me most vividly by this and the problems in the back of Friedman (1962).

11. Stigler (1963:59) pointed out that one checks empirical results asymmetrically. What confirms one's beliefs may very well be checked less thoroughly than what confutes it. The thought that econometricians have preferences was not uncommon at Chicago at the time. Feigenbaum and Levy (1996) give the references and a model of the folk wisdom.

12. Levy (1992) proposes an account of why ordinary people might pay for direction as a partial response to the Stiglerian message.

13.

Is it not, then, a bitter satire on the mode in which opinions are formed on the most important problems of human nature and life, to find public in-

structors of the greatest pretensions, imputing the backwardness of Irish industry, and the want of energy of the Irish people in improving their condition, to a peculiar indolence and insouciance in the Celtic race? Of all vulgar modes of escaping from the consideration of the effect of social and moral influences on the human mind, the most vulgar is that of attributing the diversities of conduct and character to inherent natural differences. (Mill 1965:319)

David Hume's notorious statement of Negro inferiority is found in his essay on "national characters" (Hume 1987).

14. These are the pages that Stigler purported not to admire (Stigler 1966)!

15. Walter Bagehot's position at the *Economist* came through his friendship with W. R. Greg, later to become a co-founder of the eugenics movement. Greg's attack on "abstract" economic man in J. S. Mill's account for its neglect of the "fact" of race is discussed in Peart and Levy (2000). Bagehot's role in creating the illusion that Mill's economics were unoriginal is something to which one might attend. When Stigler demolished this claim (Stigler 1965a:1–15), it was so widespread that he did not find it useful to ask how it came to be.

16.

The advantages of a bureaucratic government appeal strongly to some classes of minds, among whom are to be included many German economists and a few of the younger American economists who have been much under German influence. But those in whom the Anglo-Saxon spirit is strongest would prefer that such undertakings, though always under public control, and sometimes even in public ownership, should whenever possible be worked and managed by private corporations. We (for I would here include myself) believe that bureaucratic management is less suitable for Anglo-Saxons than for other races who are more patient and more easily contented, more submissive and less full of initiative, who like to take things easily and to spread their work out rather thinly over long hours. (Marshall 1925:274–75)

Sandra Peart pointed out the importance of this argument.

17. All the quotations that appear in this section of the paper are from referee reports by George Stigler for *History of Political Economy*, contained under restriction in the Special Collections Library of Duke University. The authors and titles of papers refereed are omitted to protect the innocent.

18. For a summary of the empirical evidence of the 1930s and 1940s, see NBER (1943:ch. 5) and Johnston (1960).

19. In later editions Robinson provided an extended discussion in the section on risks.

20. For marketing, Stigler remarks on the benefits of having each salesperson handle multiple products, selling to the full region or nation that

(fixed-cost) advertising can reach, and hiring specialist buyers (1942:137). Robinson (1932:70–71 and 117; 64–65) addressed points (1) and (3). Stigler downplayed financial indivisibilities, although he highlighted the “[t]he investigating and managing costs of a loan” not increasing directly with loan size, and the lower cost of borrowing large sums in the direct market (1942:137). Robinson emphasized that easier access to finance promoted larger corporations and vertical integration (1932:60–62), especially in established industries. With respect to research, Stigler observed that any research outlay is a fixed cost, so that company expansion reduced its unit costs (see also Robinson 1932:39–40), and that by undertaking several research projects, large companies could hedge their risks (1942:137).

21. Robinson did observe that should managing a large technical unit become cumbersome, “[a]n attempt is made to reduce the optimum size of the technical unit by increasing its specialization” (1932:109) in a narrower product range. But he more commonly discussed multi-unit management. For instance, he recognized that where output fluctuates seasonally or cyclically, “the best organization of a firm may be a multiplicity of small technical units working under a single large management” (1932:115), and that placing each plant in different regional markets would reduce transportation costs (1932:114).

22. Robinson describes the Line or Departmental organization, the Functional type of organization, and the Line and Staff form (1932:45–46).

23. Dean’s (1951) model also recognized the separation of management from ownership and possible deviation of managers’ motivations and objectives from stockholders’, and that long-run profit seeking might substitute other short-run goals for profit maximization, for instance, sales targets.

Dean, a Chicago business Ph.D., was on the Chicago business faculty and published his first book in 1936 to some acclaim as Stigler completed his Chicago economics doctorate. Stigler (1939) used indivisibilities to answer Dean’s (1936) empirical documentation of constant short-run costs and his challenge to marginalism. They would overlap on the Columbia University faculty from 1947–1957, where Dean had a joint appointment in economics and business. Dean came to devote much effort to teaching managers how to use present-value calculations to allocate resources more profitably, as if to provide the unifying principle to management efforts that Stigler had found lacking and so help the historical record confirm his rather than Stigler’s view of competition.

24. “The important purpose of a scientific law is to permit prediction, and prediction is in turn sought because it permits control over phenomena” (1942:3).

25. In all there are 211 Stigler-Friedman letters, written between May 1945 and February 1991, in the collections.

26. The work for our 1962 *Journal of Law and Economics* article was actually done, and published, in 1964, but Aaron Director, the editor at the time,

was catching up on missed issues. I'd like to add that Director, Rose Director Friedman's brother and a close friend of Stigler's, celebrated his 100th birthday in September 2001.

27. See Peltzman (1993:820–22) for details.

28. See, for example, Pashigian (1985) and Linneman (1980).

29. David Levy gives a fuller discussion above on Stigler's concept of mistakes (see section III, above).

30. Based on Jarrell (1984).

31. These may be crudely summarized. Revealed preference: what the consumer (voter) wants is what the consumer (voter) pays for (votes for); consumer sovereignty: consumers are the best judges of what they want, and competitive markets tend to (and some say "ought to") reflect what consumers want. I think it is interesting to note that the revealed preference concept is behind Stigler's work using citations as a measure of intellectual influence (Stigler and Friedland 1975, 1979; Stigler, Stigler, and Friedland 1995). The frequency of self-citation, by the way, is examined in detail in Stigler and Friedland (1979).

32. Based on Pashigian (1985).

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