

SOUTH SIDE BLUES: AN ORAL HISTORY OF THE CHICAGO SCHOOL

BY
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Oral history has always held a rather ambiguous position within the study of economics. While widely read and enjoyed, such interviews are secretly scorned, dismissed as merely a lightweight diversion. Yet, to conduct revealing and insightful work of this type is difficult, requiring a firm grasp of economics, especially individual contributions to the discipline. This article examines my own attempts to come to grips with the methodology and ideology of the Chicago School through a series of conversations with some nineteen of George Stigler's contemporaries. Oral history, if skillfully conducted, can prove to be one of the more useful tools in recapturing and understanding the past history of economics as a fiercely fought over discipline.

“We tell ourselves stories in order to live”

(Didion 1990, p. 11).

I. DOWN THE RABBIT HOLE

“In another moment down went Alice after it, never once considering how in the world she was to get out again”

(Carroll 1974, p. 14).

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I fell into doing oral history much in the same way that Alice fell down her rabbit hole. Like Alice, I was in some ways totally unprepared for the experience that was to come. To a degree, I would need to live by my wits and learn quickly on the job if I was going to survive my own mad tea parties. The particular white rabbit I was chasing was named George Joseph Stigler. In pursuing him, I was trying to pin down an elusive ghost.¹

My journey started innocently enough. After initially gaining some insight into the essential Stigler by analyzing his published work, I was intending to use a much-anticipated sabbatical leave to work my way through the archival records held at the University of Chicago library. Interviewing those who knew the dead economist was an afterthought, a 'why not talk directly to these people' idea. The inspiration for doing so came from an interesting set of memorial papers published in the *Journal of Political Economy* (1993) and edited by Gary Becker. I found myself wanting to ask those authors some further questions about their fallen hero. This formed the starting point of what might have appeared to be a rather quixotic quest.

My interest in one of the giants of post-war economics centered on a series of articles in which George Stigler fiercely defended standard price theory against all and

¹Readers will notice that this article does not follow the standard format of many economic pieces. Often this structure can best be encapsulated in three steps:

- Tell the reader what you are doing.
- Tell the reader what you are doing while you are doing it.
- Tell the reader what you have done, once you have done it.

Though I maintain no principled objection to telling a story in this form, it does seem to be unnecessarily restrictive to limit presentations to this rather narrow perspective. For those interested see Freedman (1993) for a detailed explanation of why a restrictive approach can be counterproductive to the broader aims of communicating economic ideas. However, to make the purposes behind this article perfectly clear, let me sum them up in the following way. This article examines my own attempts to come to grips with the methodology and ideology of the Chicago School through a series of conversations with some nineteen of George Stigler's contemporaries. I particularly hone in on the way that Stigler helped to shape economic thinking. In considering the value of, and techniques associated with, oral history, I also explore the methods I used, the difficulties I faced, and some of the particularly useful insights I gained by employing this approach. It is often difficult to arrive at a true evaluation and especially an honest feel for the pitched battles that defined economics in the past. Yet, the attitudes and emotions of the participants have often been essential in delineating both the issues and the conduct of the ensuing debates and controversies. Oral history, if skillfully conducted, remains one of the more useful tools in recapturing and understanding the past history of economics as a fiercely contested discipline. By using this technique, I achieved an understanding of the Chicago School; the battles it chose to fight, the tactics employed for waging these contests, and the basis for selecting them, which would have been quite unattainable through any alternative method. It is, of course, up to the reader to decide the extent to which I have been successful.

any rival explanation.² What struck me was his highly contentious approach. He would not grapple with an accurate depiction of an opposing theory head-on. Instead, Stigler preferred to create a highly flammable straw man, douse it liberally with kerosene, ignite it, and then dance triumphantly around the burning wreckage of the deposed theory. His combative strategy seemed more redolent of an adversarial courtroom battle where the sole objective is trouncing your opponent rather than analyzing the worth or validity of a contrary argument. My preliminary research seemed to indicate that all these key, hotly contested alternatives undercut the standard explanation behind distributive shares in any market economy. This distributional aspect harked back to the dissertation that Stigler had completed under the guidance of Frank Knight.³ I hoped my archival work would be able to shed some light on this question. I was less optimistic about what, if anything, was to be gained by conducting interviews. As is often the case, my naivety in this instance complemented my ignorance.

II. MY SECOND VOYAGE

Well then, it seemed to me next, since I'd wearied of studying the things that are, that I must take care not to incur what happens to people who observe and examine the sun during an eclipse; some of them, you know, ruin their eyes, unless they examine its image in water or something of that sort. I had a similar thought: I was afraid I

²I have examined this issue in a series of papers. See Freedman (1995, 1998a, 1998b, 2002). One of his close colleagues also clearly spells out his devotion to price theory.

Evidence of Stigler's attachment to neoclassical price theory is also given by that part of his work mainly critical of the work of others. Price rigidity, administered price inflation, the theory of monopolistic competition, and X-efficiency were prominent targets, and each of them denied the efficacy of the neoclassical analytical framework (Demsetz 1993, p. 888).

I was particularly interested in the issue of income distribution. This area formed one of Stigler's long-lasting preoccupations. It is the subject of his dissertation work (Stigler 1938) and the focus of the first of his five lectures presented at the LSE (Stigler 1949) relatively early in his career. The topic arises in a number of his subsequent writings as well. Clearly, he saw little, if any, virtue in income redistribution. Significantly, all his major critical targets depart from the unique equilibrium characterizing price theory. The alternatives undermine the idea of an efficient market distributing income according to a factor's marginal productivity. Yet, no aspect of this potentially dangerous implication of these theories merits an explicit mention in his fierce critiques. Either such results were simply superfluous to his objectives and motivations or we are faced with a clear case of a non-barking dog.

"Is there any point to which you would wish to draw my attention?" "To the curious incident of the dog in the night-time." "The dog did nothing in the night-time."

³The dissertation (Stigler 1938) had as one unifying theme the role of marginal productivity theory in imputing distributive shares. Rejecting this explanation undercut a key justification for accepting both the efficiency and equity of market shares. If, in fact, such shares have a characteristically arbitrary element attached, this opens the possibility for government efforts at redistribution. Stigler's dislike of any such attempts to change the status quo through such intervention is spelled out in the series of lectures presented at the London School of Economics in 1947. Clearly, a market system allowing for individual choice and self-determination is preferred if a society is to produce independent, responsible, and entrepreneurial citizens. "But we are persuaded that an economic system will not help us to move in the right direction unless it grants both opportunity and responsibility to the individual: the very uncertainty of our ultimate ethical goals dictates a wide area of individual self-determination" (Stigler 1949, p. 8). This stance remained consistent throughout his career.

might be completely blinded in my soul, by looking at objects with my eyes and trying to lay hold of them with each of my senses. So I thought I should take refuge in theories, and study in them the truth of the things that are (Plato 1975, p. 51).

When setting out to interview key individuals who had known, or worked with or against George Stigler, my innocence knew no bounds. I was ignorant to the extent that I was largely unaware of how unprepared I really was. But while my preparation was lacking,⁴ my instincts managed to come to my rescue. In retrospect, I know today that much has been written about how to do oral history.⁵ But back in 1997, one didn't automatically hit the Internet to ferret out otherwise hidden knowledge.⁶ And as I indicated, I didn't realize that any such preparation was necessary. The interviewing process seemed self-evident. Perhaps in the long run, this foolish approach was strategically beneficial. I was less self-conscious about dutifully following the right procedure and more willing to go with my instincts, which would, in any case, become more finely honed with practice.⁷ Many years later, when

⁴One glaring deficiency was in using the wrong equipment. This made the business of transcribing difficult as some of the recordings lacked clarity. I also once fatally forgot to listen for the useful click that told me when to flip to the other side of a tape. When interviewing Ronald Coase, I became so engrossed in the conversation that I lost the crucial fifteen minutes when he explained why George Stigler didn't actually understand the point of his (Coase's) 1960 article. At that famous evening hosted by Aaron Director, Coase clearly indicated to me that only Arnold Harberger had grasped his essential idea. The words are unfortunately lost. "Since he persisted, we invited Coase (he was then at the University of Virginia) to come and give a talk on it. Some twenty economists from the University of Chicago and Ronald Coase assembled one evening at the home of Aaron Director" (Stigler 1988, p. 75).

⁵Grele (1991) and Hoopes (1979) offer online editions describing how to go about the business of oral history. Reading through Moyer (1993) years after all interviews had been completed (my knack for closing barn doors long after any remaining horses had departed) I found I had followed a number of her most critical prescriptions. (See footnote eleven for further details.) I need to emphasize that I had no intention of conducting anything like a formal oral history when I set off to talk to these noted economists. I did not want to find out facts about the individuals in question, but rather to discuss their impressions of George Stigler. Of course, people reveal themselves in conversation whether or not they are themselves the topic under discussion. Those who have a rather restrictive concept of oral history may dislike me for lumping such conversations under this particular label. But I fail to see the real value of such didactic classification.

⁶Even back in 1997, I could have, without extraordinary difficulty, discovered how others, especially those outside economics, went about conducting oral history. It would also have been quite possible to have read more actual interviews of economists that existed in various publications. The point that needs to be emphasized in this regard is that I didn't think of myself, in any formal sense, as being engaged in an oral history project. I just wanted to talk to these people to see whether they had anything useful to contribute to my research. I had no expectations that the actual transcripts would themselves be of interest. Above all, it didn't enter my mind that I needed any training just to talk with these people and ask a few questions. That is what I can best characterize as the blatant naivety surrounding this particular effort.

⁷I definitely noticed an improvement and felt quite comfortable conducting my final interview with Mark Blaug. This didn't, however, prevent the occasional flat conversation, which largely reflected an inability to establish the appropriate degree of empathy needed for a successful outcome. I did start off with somewhat of a beginner's nightmare. An interview with Milton Friedman turned into a three-ring affair with the addition of his wife Rose and her brother Aaron Director. This could be described as the equivalent of learning to swim by being tossed into the deep end of the pool, wearing cement slippers. My lack of experience shows through, though it still would have posed a challenge had it come later on in the process. What was clearly true was my nervousness in this first interview and what retrospectively could be described as insufficient preparation. Had this interview been conducted at a later date, I would have had the opportunity of first reading material from the archival records held at the University of Chicago library as well as having the benefit of what others had said in previous conversations.

I had the leisure to check over the prescribed dos and don'ts, it turned out that those mysterious instincts were broadly on target. And even now I doubt that any amount of preparation would have made me into the compulsively methodical individual such guides seem to prefer. Besides, I didn't want so much to interview as to talk to the people I had targeted. These are two distinct activities. In a formal interview environment, individuals prepare themselves beforehand in order to provide the most informative answers possible. Interviewers often compose a precise list of questions, which they proceed to wade through dutifully, one item after the next. In what I like to think of as the extreme, or Japanese, approach, the questions are forwarded to the interview subject prior to the actual event. This I would label as being a rather low-risk, conservative approach. It lessens the chance for spontaneous episodes and at best provides deep but narrow responses. A prepared subject is more likely to simply repeat what he or she has said before, to switch on the automatic pilot. A skillful interviewer can use follow-up questions to tease out additional information of interest, but the subject will be able to fend off such attempts and be careful not to give away anything too controversial unless that is his or her deliberate intention. Removing preparation time also means risking memories that are distinctly faulty. Individuals have no recourse to fact checking prior to the conversation. Hazardous that risk is more likely to elicit offbeat or distinctly unfiltered impressions of quite relevant material. Pinpoint accuracy may not be too great a cost to pay if your main aim is focused on subjective evaluations.

To some extent the usual provisos attached to interviews did not really apply in my specific case. My search wasn't so much for additional biographical material about George Stigler but was instead more sensitive to opinions and evaluations of Stigler's work and work habits. These are quite comfortably categorized as impressions revealing the man himself. Certainly, a wealth of details and information accompanied these interviews with all the attendant problems that define selective and slanted memories.⁸

Finally, at the level of personal images the analysis might again be useful to historians primarily because of what it reveals to be normal and predictable. If its claims about cognitive structuring are correct, one would fully expect economists to make contradictory self-presentations at different times and in different places, perhaps even within single utterances. This does not make their personal testimonies worthless as sources of information, but it does mean that those testimonies should seldom be expected to form single coherent narratives (Reay 2007, pp. 150–151).

Information, however, does not form the core of these interviews, but rather comes as an additional bonus. This is clear in the very best of these conversations. Paul Samuelson provided a wealth of details, but, after reading his answers carefully, his view of the whole Chicago project and those who created it generates the more interesting insights. In particular, what Professor Samuelson seems to convey is a long-standing sense of frustration with people like Stigler, Friedman, and Wallis,

⁸E. Roy Weintraub (2005) has a particularly useful paper discussing the pitfalls of depending on remembered events. One way to look at the problem is that ideally we would like to be able to choose the Proustian option of recapturing the past. Instead we are left pawing through the crumbs of memory.

and the tactics they employed.⁹ Indeed, what people think they remember may at times be as equally revealing as what actually occurred. I scrupulously sought for something that approximated a balance of impressions by talking to some nineteen people, all of whom had very different relationships and reactions to George Stigler.¹⁰

In contrast to the more formal interview, there is something much more serendipitous about conversations. What they lack in structure, they make up in surprise and unexpected insight. Even more than is the case with formal interviews, the empathy between the parties becomes essential.¹¹ If people are comfortable with one another and become interested in the conversation, they are more likely to be expansive and less likely to be defensive. Engaged and relaxed individuals involved in a serious discussion will be apt to allow candid opinions to slip out, as was made evident in my conversations with Paul Samuelson and a number of other individuals.

People in our profession have always been kind of scared of Milton Friedman as a polemicist. So, he gets away with a certain amount of murder. And when he's safely dead and when they've salted his grave against any revival, the daggers will

⁹In conversation, Samuelson points out that Friedman would just go silent if proved wrong and that Stigler would never admit a mistake.

He [Stigler] also never answered criticism. I never understood that. ... And a lot of people would tell me that if they wrote to him complaining about something, he would answer something like 'Well, if you're the kind of person who believes that then you're just the kind of person who believes that' (Conversation with Paul Samuelson, October 1997).

¹⁰These impressions could vary noticeably. Subjectivity may generate views orthogonal to one another. While Milton Friedman seemed convinced that his relationship with John Kenneth Galbraith had been amiable over the years, Paul Samuelson remembers a more 'us' versus 'them' approach characterizing Friedman and his close colleagues. Given their viewpoint, Galbraith's economic and policy positions would have transformed his behavior into that of a subversive. Here, Friedman seems to be identifying good relations with polite behavior: "I've known Galbraith over the years on and off. And I get along very well with Galbraith. But as the same time, I have observed that he is more interested in making a statement. That he is willing to sacrifice the substance for the rhetoric. And I don't think George ever was" (Conversation with Milton Friedman, August 1997).

They defended each other. Aaron Director, for example would never have written a good letter of recommendation for somebody who wasn't a staunch conservative but neither would Milton. And I remember for years after I left the University of Chicago, when they were contemplating influential appointments they would ask me about the person, 'Is he really sound?' In fact, Milton once showed his naiveté to me, but it wasn't about appointments. He said 'Tell me the truth, is Galbraith a Commie?' You know, the amount of naiveté that's in that (Conversation with Paul Samuelson, October 1997).

¹¹As one of my guides, read after the fact, insists:

- be understanding and sympathetic;
- show interest and respect for the person and what they know;
- listen carefully, not asking questions that have already been answered;
- don't impose your own ideas and opinions (Fittell 2003, p. 1).

Again, I seemed to have followed these precepts without explicitly recognizing the need for them while conducting my series of conversations.

come out. I'm flogging the point. That doesn't change his overall status. . . . One of the notable characteristics of George Stigler was his humor. There was an element of cruelty in that humor, and maybe that grew over the years. There are people who are still alive who carry the scars, and will to their grave (Conversation with Paul Samuelson, October 1997).

Part of establishing the ease with which one can elicit confidential remarks depends on an ability to securely park one's ego at the door. My opinions and thoughts were largely immaterial. The conversation was never intended to be about me. Certainly, one of the keys to success was developing a sense of when to shut up. The views of those with whom I spoke were paramount. This statement implies that I was not there to argue a point and especially not to convert them to one of my preordained views. Antagonism effectively destroys empathy when the objective is to create a comfortable atmosphere that allows the individual to open up.¹² In fact, the hope was that those interviewed would come away without any definite idea or concern about my own rather evolving stance. I would put forward possible ideas and interpretations simply to get an interesting response. Whether they agreed or disagreed was largely immaterial. I was as happy to have any tentative ideas shot down as supported. (Though, perhaps, I was secretly pleased to have Gary Becker agree with a possible hypothesis: "But his views did become more consistent. I agree with you on that. Other people may not think so, but I think definitely that was true. He began to re-think some positions he had just inherited" [Conversation with Gary Becker, October 1997]).

In my case, I was even more fortunate since the conversations I held were not about the person being interviewed. Instead, they were about a third person, George Stigler. Most individuals will be more eager to protect themselves and their own reputations than that of a third person. (Though, clearly, what someone says about a third person is often quite revealing about him or herself.) I was particularly fortunate in talking about someone who, in his lifetime, had generated strong feelings, whether positive or negative. What was often expressed was an acute feeling of loss, as if someone larger than life had left the scene.

He was a great person to be around. It was fun to be around him because he made professional work fun. And he was a great gossip about the profession. You could always find out what was going on and what George's opinions were about what was going on. Every dinner you had with him was entertaining. He was a man of great wit and great style and I miss him very much (Conversation with Harold Demsetz, October 1997).

Without thinking, then, without any attempt to weigh up alternative approaches, I chose the high-risk option when I went off with tape recorder in hand. I didn't at the time expect to use these conversations for anything other than raw material. I was, in fact, surprised when so many of them turned out to be interesting in and of

¹²The two exchanges that provided the least amount of satisfaction were those conducted with Armen Alchian and John Kenneth Galbraith. Two people with more opposed approaches and views would be hard to imagine. So the failure cannot be placed to either their economic stances or their respective personalities. For whatever reason, in these two cases I failed to make a connection despite my efforts. The sad reality is that if some sort of empathetic relationship is not established early on, then no matter what strategies you desperately attempt to adopt, success is likely to prove elusive.

themselves. They all rambled, as could be expected, given the approach I employed. And those where I didn't manage to establish any real empathy with the person were those that were least satisfactory. They were mostly lifeless, filled with what sounded like recycled material. But in many cases, in the course of a conversation, we managed to hit on ideas and topics that provided ground for fresh exploration. Of course, when I claim that I went into an interview situation not knowing what I was doing, I don't mean to imply that I wasn't thoroughly prepared. In each case I was fully knowledgeable of Stigler's work and the work of the person being interviewed. I had also prepared a series of topics I wanted to discuss in the course of the conversation. If you end up reading all the interviews, it becomes clear that there were common themes brought up in all the conversations. My difficult job was to listen to each person's answer while thinking about where I wanted to go next. Being a conversation, it was largely unstructured in that if a response was provocative or otherwise interesting, I was prepared to follow that up rather than march linearly from question to question. To emphasize the point once again, I viewed my objective as getting the individual to open up and reveal his or her thoughts. I served simply as a medium to facilitate an interesting flow of conversation. In such circumstances, my thoughts or opinions were insignificant. I wanted to understand what these people thought rather than foolishly trying to convey my own ideas. What I had to do to accomplish this would vary from individual to individual. For example, the very easiest conversation was the one conducted with Paul Samuelson. As a routine, I would often begin a session with a softball question to put the subject at ease and try to create an atmosphere conducive to reminiscing. Usually I asked how they came to know George Stigler.¹³ Professor Samuelson spent some forty-five minutes answering that question, while I did a very good imitation of the slave boy in Plato's *Meno*. At that point Paul Samuelson announced he had 'shot his bolt' but would be willing to take any other questions. We then spent another forty-five minutes covering additional territory. The point is that I was as active or passive as I deemed necessary to get the individual to open up. This was certainly a judgment call, which I did not always get right, but in general I think I managed to be more effective than I would have thought possible. However, what remained a core requirement was convincing these people, who were kindly giving up their time, that it was worth the sacrifice.¹⁴

In contrast, what I think to be an equally invaluable conversation with Gary Becker demanded much more on my part. (In fact, following the eighty minutes spent with Professor Becker, I felt the need of a good lie down.) In looking at the interview, one aspect that stands out is the additional effort expended in suggesting possible interpretations, asking more questions, and following up on answers. In comparison, I

¹³In the case of Paul Samuelson, the question was simply, "Let's start with some background information. I know you met in the '30s in Chicago" (Conversation with Paul Samuelson, October 1997).

¹⁴Paul Samuelson, in the flow of our conversation, was interrupted by his secretary, who reminded him that, as expected, the chairman of the Bank of Italy was on the phone. He looked at me and asked how much longer I would be. I replied "No more than thirty minutes." Samuelson then turned to his secretary and asked her to have the chairman call him back in another hour. In a parallel fashion, many were willing to talk only as long as the conversation remained of interest. Gary Becker, at the onset of our talk, tossed out something of an implicit challenge to see if it was worth spending time talking to me. I must have passed since the conversation continued for another eighty minutes and was one of the more interesting, though most exhausting, ones for me.

had only to give Paul Samuelson his head. What doesn't come through by simply reading the interviews are the pauses. While there are few with Samuelson, with Gary Becker there is often a delay while he carefully considered the question and all his possible responses. However, what was common in all these sessions was the intensity with which I was required to listen to what they were saying. I couldn't just nod my head and proceed to the next stage of my mapped route.

Except for the first and last of these conversations, I managed to squeeze in the following interviews in October of 1997. They're listed in the order in which they were conducted:

1. Milton Friedman, Rose Friedman, Aaron Director
2. Harold Demsetz
3. Arnold Harberger
4. Armen Alchian
5. Gary Becker
6. Lester Telser¹⁵
7. Sam Peltzman
8. Clair Friedland
9. Ronald Coase
10. Stephen Stigler
11. Sherwin Rosen
12. Paul Samuelson
13. Robert Solow
14. John Kenneth Galbraith
15. James Kindahl
16. Paul Sweezy
17. Mark Blaug

The core of these individuals came, as I have mentioned, from the memorial *JPE* volume. Others were added as circumstances dictated. For instance, it was Milton Friedman who suggested that Rose Friedman and Aaron Director also be present.

¹⁵Professor Telser preferred not to be taped: "I have to live with these guys." It is a distinct loss since he provided one of the more fascinating sessions. This prohibition extended to any written notes as well, though I did attempt to record some of my memories of what transpired once I was out the door. While I am focusing here on taped conversions, there are obvious second-best solutions when taping or direct contact proves infeasible. Specific questions can sometimes be usefully answered via written communication. My own experience is that this works satisfactorily if you limit your questions and don't expect extensive follow-up responses. For instance, Dennis Carlton was quite helpful in explaining George Stigler's reaction to his own paper on price flexibility (Carlton 1977). My response to Mark Perlman's memorial essay (McCann and Perlman 1993) drew a rather wiry email (April 1996) from Professor Perlman, questioning whether it was worth the effort of doing any further research on the economist he referred to as "Boy George." Last, Paul Sweezy, like the two previous academics, when questioned about his objectives in writing his 'kinked demand' article (Sweezy 1939), supplied me with an illuminating written response (May 1994). All of which shows that in terms of oral history, every avenue should be explored and none automatically ruled out of court.

AD: No, I didn't get to know him then.

Milton Friedman: I came back to Chicago for a year in '34 to '35 and that's when I first got to know George. Rose got to know George . . .

Rose Friedman: A little bit in his first year.

Milton Friedman: But then we really got to know him when I came back (Conversation with Milton Friedman, Rose Friedman, and Aaron Director August 1997).

At UCLA primarily to interview Harold Demsetz, I decided to include Armen Alchian as well, since he was part of the Chicago West contingent. It was Harold Demsetz, clearly pleased with our conversation, who subsequently decided to call Arnold Harberger on the phone and set up a quick interview.

I did not have a lot of day-to-day contact with him. His workshop used to meet over at the Law School for example, the other side of the Midway. He was a professor in the Business School as well as in the Department. But in any case I really feel that he is one of the most interesting and in some sense, most enigmatic characters in our profession in this last century (Conversation with Arnold Harberger, October 1997).

Besides the original core of people in Chicago, I sought out Stigler's son Stephen. His permission had to be gained to view the Stigler archives and it seemed worthwhile to have his personal impressions while I was there.¹⁶ And I couldn't pass up the opportunity to question Ronald Coase about Stigler's Coase Theorem since I was already there on campus.

I think he was one of the most difficult people to explain because there is no one like him. I've described how in an argument he jumps around.^[17] He puts in a bit of theory,

¹⁶One perennial question that is always tempting to ask the child of a distinguished economist is whether he or she applied economic principles at home. Stephen Stigler assured me that this was not the case.

As a family, we were not as economically thorough in our upbringing as some people might be. I know some families that would think *everything* was economic. But that's not our family. In many ways my father was, if you wish, inconsistent in some respects. He was very generous with his time, with his money, with his expenditure on his family and towards his friends. He was not somebody who approached most of life with—his personal life—with any narrow, economic constraint (Conversation with Stephen Stigler, October 1997).

Almost all his colleagues and certainly his close friends were emphatic about his generosity, despite his strong belief in narrow self-interest as the cornerstone of economic analysis.

Now did it square? I think not in the following sense. And I raised this issue with him in a slightly different guise. When George was skeptical about the altruism issue I said, 'Look George, look how generous you are to your children. Are you doing that out of self-interested motives? Who are you kidding? You're not doing it out of that.' And he looked at me and he didn't answer and he knew he wasn't doing that out of self-interested motives. He was actually very generous with his family, as he was with people in general. So, I think George, when pressed hard like I did there, would admit there is a motive beyond simply selfish self-interest (Conversation with Gary Becker, October 1997).

¹⁷His biographical piece on George Stigler is one of a series of such essays that are found in Coase (1994). They are all short and insightful sketches in miniature that well repay a careful reading.

a bit of statistics, a reference to the earlier economists. It's like no one else's form of argument that you can recall (Conversation with Ronald Coase, October 1997).

Lester Telser, a long-time colleague at Chicago, had himself written a short biographical sketch about George Stigler. But in particular I was puzzled by a curious statement made in Stigler's autobiography that seemed to differ from Telser's own published views (Telser 1960). To be precise, what Stigler claims as an explanation of resale price maintenance (Stigler 1988, p. 163) seems to be more of a hypothesis, which, in the case study provided by General Electric, lacks evidential validity.¹⁸ Then again this incongruity should not have been surprising. The two of them endured more than a few differences over the years.

Lester says to me [Claire Friedland], "That George is so stubborn. He is so unreasonable. I told him his research was wrong. I explained it to him. I was right. George was wrong. And he won't listen to me." Lester is just letting off steam. He goes out Stage Left; George arrives Stage Right and says, "That Lester is crazy. I don't know why I pay any attention to him. But let's try it his way" (Conversation with Claire Friedland, October 1997).

I then headed for Boston to hear what some of his opponents might have to say. Clearly, Paul Samuelson was a prime candidate for an interview as someone who had known him since the 1930s.

I probably saw George the most in my final senior year, which was the academic year 1934–1935. George and Allen Wallis had taken squatter rights possession of a storeroom in the basement of the social science research building where the economics department was. . . . The reason that I saw them [Stigler, Friedman, and Wallis], and a great deal of them, was that to make some money during the Great Depression I had what was called an NYAS (National Youth Administration) scholarship supplement. . . . I don't know how much an hour I received, maybe twenty-five cents an hour, but they had to find some perfunctory work for me to do. And just as in *Pinafore*, I polished up the brass on the door. I was given the job of dusting off in that Department Records Room the pictures of the great economists, Bohm-Bawerk, John Stuart Mill, David Ricardo, Adam Smith, and maybe Knut Wicksell (Conversation with Paul Samuelson, October 1997).

Moreover, Professor Samuelson had been on the receiving end of Stigler's wit as well as being cavalierly dismissed by Stigler as unlikely to achieve any eminence as an economist: "Paul Samuelson, preceding George on a panel, ended his remarks with 'I know what George Stigler's going to say and he's all wrong.' George stood up, said '2 + 2 = 4.' And sat down" (Friedland 1993, p. 782).

It may merely be prejudice, but I'm inclined to write him [Paul Samuelson] off as an economist. Two of his recent jobs . . . were pure mathematical exposition, as is also his current *Economica* item (which, by the way, has already been done better by Wold), and his textbook suggests that he doesn't know anything that hasn't appeared in the *Survey of Current Business* (Hammond and Hammond 2006, p. 97).

¹⁸When questioned, Telser agreed with my perception and seemed puzzled by Stigler's claim.

Solow became a last-minute addition when I discovered the close friendship that had developed between the two.¹⁹

There was a sixty-fifth birthday party for George that the University of Chicago put on, and my wife and I flew out to be there. I remember that we were all put up in a University building, the Center for Continuing Education which had bedrooms. We came in one evening and the next morning we walked into the dining room where all the other guests at this party were having breakfast and a hush fell over the whole dining room. Finally a friend of mine, I think it was Si Rottenberg from the University of Massachusetts, came over and said, “We were all wondering, ‘what are you doing here?’” I had to explain that I was there because I adored George. And so we remained close friends until he died (Conversation with Robert Solow, October 1997).

I had already written a paper (Freedman 1998b) on the adversarial relationship between Galbraith and Stigler, and since I was in Cambridge already, thought to follow it up. That almost automatic sparring continued for more than four decades, with everything Galbraith wrote sure to receive a stinging review from George Stigler.

He saw me as somebody who was a menace to that pure supply and demand market system. At that AEA Conference in Philadelphia, there was a big crowd which attended the meeting where I was presenting a paper. I was on my way to my meeting there, and I overheard somebody say, “Well, it is time to go to the meeting to hear George Stigler kill off Galbraith.” And George made a systemic, orthodox and very plausible attack on the idea that one position of strength in the market tended to beget another, which was my case (Conversation with John Kenneth Galbraith, October 1997).²⁰

¹⁹In talking to Robert Solow (and reading the correspondence between the two), it isn’t difficult to see why a close friendship developed. Both were very clever and quick witted and willing to stand their ground. In fact, standing one’s ground against potential body blows delivered by George Stigler was a *sine qua non* of gaining his respect. Those who couldn’t, bore the scars. (There doesn’t appear anything deliberate or malicious about his behavior. Rather, he was intrinsically a very competitive and combative person.)

He slaughtered his students. Right at the beginning of our conversation together I realised (I don’t know whether I grasped this consciously) that you always had to give back as good as he gave. He’d say, “Oooh, I think you’d better watch out for this chapter. You know you didn’t take this into account.” I’d say, “Yes I did.” I think I argued with him. And the moment I did that, our relationship was brilliant. No problem dealing with those students who stood up for themselves (Conversation with Mark Blaug, November 1998).

“Here are some notes I’ve sent to . . . on her thesis. She really should learn to write, and possibly to think” (Letter from George Stigler to Lester G. Telser, 18 January 1985).

²⁰The two papers are Galbraith (1954) and Stigler (1954). Together they provide a wonderful example of Stigler on the attack. Galbraith himself was hardly loathe to take his own shots as well.

One of his more famous comments, I suppose in the ’50s, or early ’60s, was when he said that it was one of the tragedies, one of the misfortunes of our time, that so few people read Adam Smith while so many people read Galbraith. And I was immediately asked for a comment on that and said, “George Stigler’s regret is not that so many people read Galbraith and so few read Adam Smith, but that no-one reads Stigler at all” (Conversation with John Kenneth Galbraith, October 1997).

In contrast to Galbraith, Jim Kindahl had been his co-author of *The Behavior of Industrial Prices*, a landmark work published in 1970. He had also been one of my teachers in graduate school, one whom I greatly admired. Moreover, Amherst was only a short hop away from Cambridge.

I was teaching at Amherst College at the time. One day the phone rang and a voice said, ‘This is George Stigler.’ Now, only George Stigler could say, ‘This is George Stigler,’ end of sentence. He would expect that I would know who he was. And I did of course. Any Chicago PhD student knew who he was... He said he would like me to come, to take a year off, and work with him. This is out of the blue. This is a man I’d never met in my life. He said Milton Friedman had recommended me and Milton’s recommendation was good enough for him. He didn’t need to know anything more (Conversation with James Kindahl, October 1997).²¹

I had consulted Paul Sweezy when writing my original article on George Stigler and the Kinky Demand Curve (Freedman 1995).

It’s very simple as a model, in its form as you know. And it made sense. And I still think it’s pretty good. A pretty good point about the pricing of a monopoly product, but I certainly never thought it would ever be a great achievement or breakthrough or anything like that. It turned out to have a much more illustrious career than I expected it to have... I didn’t pay much attention to Stigler in those days. I was probably in one of my ultra-left moods, or something like that (Conversation with Paul Sweezy, October 1997).

²¹The similarities to how George Stigler hired his researcher of more than three decades, Claire Friedland, are what might technically be defined as spooky.

I had an imaginary conversation in my mind later on, and it was between George and Milton. George got my name from Milton. But he didn’t tell me about this imaginary conversation. I imagined George, who was very old fashioned as I said the other day, saying to Milton, “Do you know a girl . . .” I know he said ‘girl’ . . .; “Do you know a girl who would work for me full-time?” . . . And the funny thing is that though this conversation was imaginary, George’s son Steve found among George’s papers a little piece of paper on which was written my name, spelled wrong, which is perfectly understandable, and then he had written down the phone number of the Federal Reserve Bank. Those were the notes he took of his conversation with Milton, the one that I had imagined (Conversation with Claire Friedland, October 1997).

What is worth noting is the absolute trust this group, centered on Stigler and Friedman, displayed in one another’s judgment.

Now, what you have to understand with somebody like Allen Wallis, and so to a degree those people who were in his circle, is that Allen Wallis had the sharpest priors—I’m using the language of Bayesian probability—of anybody I ever knew. Almost no new data could change his view for this reason. On the other hand, if he thought of somebody as a dangerous, or an incompetent thinker, but Jimmy Savage assured him that the man was very smart and had good judgment that carried more weight with Allen Wallis than a two-year study of the person’s vitae and an audit of his writings. There’s an in-group of the good guys and the much larger out-group (Conversation with Paul Samuelson, October 1997).

Lastly, when Mark Blaug had kindly contacted me concerning the same article, he had revealed himself as being Stigler's first PhD student while studying at Columbia.²²

I was immediately and mainly attracted to George Stigler's lecturing style. I don't think he was a particularly good lecturer. But the lectures were like a lot of his articles. Lots of sarcastic jokes. And very arrogant. Quite cynical. This was much more the kind of lectures that I like. So, I attended the lectures very religiously. As a result, I decided I wanted to do a Ph.D. in this area. Eventually my dissertation became my first book on Ricardian economics, on what happened to the Ricardian system after Ricardo's death (Conversation with Mark Blaug, November 1998).

So while what passes as a grand strategy was put together in a haphazard manner, I did end up interviewing an interesting core of key individuals. About the only principal I failed to snag in my net was Allen Wallis, who, with Stigler and Milton Friedman, formed the Three Musketeers of Stigler's graduate years at Chicago.²³

²²This seems to be a bit of an exaggeration. At the time of George Stigler's sixty-fifth birthday in 1976, an attempt was made to list all his PhD students, those committees where he acted as chair. According to Columbia's records, others had preceded Mark Blaug (1954–55), including possibly Richard Cyert and Mark Perlman.

²³The relevant period was the academic years of 1933–35. This initial year at Chicago for Stigler and Wallis marked Friedman's year away at Columbia. Wallis himself would leave for Columbia to study under Hotelling in 1935–36. The three were classmates at Chicago for only one fateful year. Yet, Allen Wallis would bring them together from 1942–1945 to work for the wartime Statistical Research Group in New York. Wallis was also instrumental in finally bringing George Stigler back to Chicago as the Walgreen Professor while Dean of the Chicago School of Business. This is memorialized in a ballad the three wrote to celebrate Stigler's forty-seventh birthday:

Twenty-five years for the tale to unfold
Yo-ho-ho and again there are three
Walgreen was good and Kimpton was bold
Yo-ho-ho and again there are three (Friedman 1993, p. 771).

Charles Walgreen earned his fortune from a chain of drugstores. In the thirties, his niece, attending the university, had claimed that the teaching was heavily under the influence of communist agitators. The resulting state inquiry led to headlines in the *Alumni News*, reading "Red Scare, No Hits, No Runs, a Few Errors." In reaction to what seems to have been an embarrassing fuss for all parties concerned, Walgreen, shortly before his death, established a Chair for the Study of American Institutions in 1939. As his son, Charles R. Walgreen Jr., recalled in a letter to the chancellor at the time of George Stigler's funeral, the chair, long held by Stigler, was initially under the Department of History.

Rather than filling the Chair, eminent individuals gave series of lectures from time to time. I am sorry to say that some of these individuals had beliefs that were extremely liberal and differing substantially from my Father's. Following discussions with Larry Kimpton, the Chair was moved to the School of Business. My Dad would have been delighted with the appointment of George Stigler (Letter dated 16 March 1992 from C.R. Walgreen to the Chancellor of the University of Chicago—Box 33 Stigler Archives).

Larry Kimpton followed Robert Maynard Hutchins as chancellor of the University of Chicago (1951–1960) and, along with Wallis, was instrumental in luring Stigler to Chicago. Interesting, or perhaps ironically, 1958 also marked a controversial decision by Kimpton to stamp down on the *Chicago Review*, a literary journal published by the university. The magazine had stirred controversy in the local press for publishing excerpts from William Burroughs' *A Naked Lunch* as well as writings by Jack Kerouac. Kimpton would give up university life in 1960 for an executive position with Standard Oil of Indiana. He stayed with the corporation until 1971, retiring for health reasons.

I should have made a greater effort to contact him, as he died the next year in 1998.

III. TRAPPING THE QUARRY

I hope I'm not only speaking for myself. I do have this impression that I know what George thought... Well, the only thing that I hope will save us from the trap of thinking that Claire knew what George thought is that you've interviewed other people. There's that mountain of evidence argument again. If the others don't agree with me, forget everything I said, especially since some of those others might have known George better. A few not only knew George better but also know more about economics than I do. So, if I've made some mistake, I know that they will correct me (Conversation with Claire Friedland, October 1997).

The difficulty is always in knowing how to frame a conversation so that it will be fruitful. Given my approach, this was always going to be chancy. Certainly, the opening question was simple. It was usually some variation of how the individual came to know George Stigler. Once, though, the conversation was started, how could it best be carried forward?

In a sense, if it was a real conversation, the discussion progressed naturally, given some guidance from me. However, it was important that I kept focused on my objective of shining a light on how George Stigler actually worked and thought. By definition, the responses would be subjective evaluations. My hope was that by asking the same or similar questions, or bringing up key points with each individual, I would indirectly achieve a sort of second-hand discussion among the participants while I remained a discreet midwife. The best way to convey what I gained by this method is to look at a few key areas.

A. A Priori Beliefs, Empirical Evidence, and Marketing

It was just that he was so enthusiastic about quantitative measures. He thought that he was going to change the world... I was sitting with Aaron Director at the time when he gave his Presidential Address^[24] and we did look at one another at the time to try to see what each one thought about all of this (Conversation with Ronald Coase, October 1997).

The natural question to ask of someone who was almost messianic about using empirical evidence to test hypotheses was whether his own views and beliefs ever changed due to such testing. It is unfortunately possible to reduce empirical evidence

²⁴This address appeared in print as "The Economist and the State" (Stigler 1965). The end of the address has at least a whiff of a Huxleyian 'Brave New World' to it, with the economics profession emerging in a purer form as cold, hard evidence takes over from simple assertion.

to a mere rhetorical device that allows an economist to better market his or her prejudged opinions.²⁵ And it is undeniable that Stigler fully understood the importance of marketing and was adept at using it. He might be seen as carrying marketing to heights not previously considered. The Chicago counter-revolution emphasized style as well as content. As evidenced by his pioneering paper with Gary Becker (1977), convincing target audiences that the flow of services provided are closely congruent to their given preferences is vitally important.

Part of it is the persuasion. There's no question. I mean George Stigler, I remember when I was a young person, wired and said 'Selling is very important in your research. So write better. Work on writing because that is important. You've got to sell what you are doing. I think he's exactly right. You've got to sell what you are doing (Conversation with Gary Becker, October 1997).

Because Stigler early on saw the importance of marketing his views, part of his technique was to state his research and his results in the strongest way possible. Certainly, this partially reflects his personality, as a number of discussants happily pointed out. But it was more than that. As George Stigler himself would have admitted, and as Claire Friedland points out, he liked to stir things up. He very much

²⁵It is important to note that this question doesn't imply that George Stigler ever consciously doctored or employed data with a view to putting across a given point. This judgment has sometimes been disputed. Certainly, Gardiner Means reacted strongly to Stigler's attempts to excoriate the idea of administered prices (see Stigler and Kindahl 1970). Means initially did battle with the National Bureau of Economic Research in trying to keep the book from being published and then in taking the work to task in the *American Economic Review* (1972). The two men clearly held irreconcilable and unyielding viewpoints: "Both of them, both of these men had taken strong positions on this . . . I almost come close to saying, that they had taken these positions before I was born. That's not true, but it certainly was before I was born as an economist" (Conversation with James Kindahl, October 1996). However, after talking with both Kindahl and Friedland, who did all the actual empirical work, I can't imagine that either one of them in any way altered, or played fast and loose with, the data they had available.

What we said wasn't very convincing. At least the data said so. In my memory at least, the data says, 'Both of you guys are horses' asses, both two sides are off the wall, but Means I do remember writing a paragraph that says in effect, 'Well then maybe we are all off the wall, but Means is more off the wall than we are' (Conversation with James Kindahl, October 1996).

What are more controversial are the interpretations drawn from the empirical work. In these we see evidence of George Stigler's bedrock and seemingly unalterable beliefs. "It was George Stigler who wrote those things into the book. Why he wrote them is another question" (Conversation with James Kindahl, October 1996). Here we see a few factors at play. The need to market one's core beliefs combined with a great reluctance to admit to any mistake. Stigler was at no point dishonorable. But facts don't speak for themselves. Statistical analysis needs to be interpreted. It is a common flaw to find in results exactly what one expected. We all are, to some degree, victims of our own carefully crafted Kantian categories, which make it difficult to recognize unexpected or even unwelcome outcomes. We see what we want to see. These ideological biases are inherently more dangerous than any outright charlatanism, since the former is done on a good-faith basis.

wanted to have a positive impact on the economics profession.²⁶ From that standpoint, the worst thing that can happen to one's work is for it to be simply ignored. The cardinal rule then in the marketplace for ideas is to get noticed.

But it got people riled up and they jumped in. It's not the result that we have today. But we have a whole literature as a result. So he was very much this kind of a person who would say, 'I'll state this result as strongly as I possibly can, even if it's not completely justified by the evidence.' I mean I think at some level he said to himself, 'I'll make the strongest case I can and then if that stirs people up...'. In many ways he was using a bully pulpit that he had acquired from his stature in the profession. He was able to do that (Conversation with Sam Peltzman, October 1997).

What these series of conversations end up reinforcing is the inherent difficulty in coming to grips with exactly what George Stigler was marketing. Was it merely the results of his empirical investigations, the facts as he knew them, or was he trying to push some very deeply held beliefs by using his evidence as a vehicle for promoting *a priori* conclusions? Such a quandary is not easily resolved. This makes actually trying to discover the source of a person's beliefs not unlike trying to tap dance on top of quicksand. To succeed, only the greatest of skill combined with the lightest of touch stands any chance whatsoever. When faced with choosing between empirically or ideologically grounded, perhaps it is best to dodge the issue by following traditional economic reasoning and accepting those core beliefs simply as given. "I don't know where his views came from. That's a hard question. Who knows about things like that" (Conversation with Sherwin Rosen, October 1997)?

Otherwise we are forced to become amateur psychologists, looking at genetic and environmental markers. It is quite easy to point out, as a number of people did, Stigler's basic shyness, sensitivity, and even a sense of insecurity that was well camouflaged beneath a gruff exterior and an almost compulsive habit of distancing himself by the

²⁶He dismissed the idea of rhetoric, especially as encapsulated by Donald (now Deirdre) McCloskey, out of hand. A 1985 NSF proposal submitted by McCloskey is met by a full dose of Stiglerian scorn. In fact, his last book review eviscerated the volume edited by Klamer, McCloskey and Solow (1989). "To date, the only clear consequence of the study of rhetoric for economics appears to be conferences and volumes such as these" (Stigler 1989, p.840).

Paradoxically, he was a great practitioner of the art and tended to push his points to the breaking limits. He considered this to be the best way to compete in the market for ideas. It was for those who disagreed to prove him wrong.

I realize that you've exercised as much self restraint here as I have by giving up chocolate, but I think "never before employed", "wholly novel", and "utterly" should be deleted on the ground that sentences employing words "wholly", "never" & "always" are always wholly wrong & never right (a rule I learned in Forest Hills High School for use in multiple-choice exams) (Note from Claire Friedland to George Stigler 6/9/72).

use of his corrosive wit.²⁷ Alternatively, we could focus attention on the influence of Viner and Knight in forming his bedrock conservatism and adversarial nature.²⁸ Undoubtedly, the role of the Cold War era in sharpening a sense of the importance of

²⁷The view that Stigler projected a definitive public image is widespread.

He had this kind of, I want to stress, a *public* persona. I mean Claire Friedland who you are going to talk with tomorrow, knew him much better than I did during his career. But his *public* persona was this very strong Protestant father figure. And you just don't cross that father figure. Behind it there was something else. You could see a side of him at times which was very shy. This was kind of a shell that he used to defend himself. You could see that aspect of him. But the public persona was unequivocal. And it was this public persona which was very much responsible in some sense for his image. He was a real leader. . . . Harold [Demsetz] would tell you that. You just dreaded the warning look . . . that dirty look from George. That is the way he led (Conversation with Sam Peltzman, October 1997).

Claire Friedland and Rose Friedman agree that despite his corrosive wit, there was a very shy and perhaps sensitive individual behind the façade. He never seemed to learn how to control that reflexive wit, which came through fairly unfiltered, whether faced with an equal or an unsure graduate student: "He didn't soften his remarks in order not to hurt their feelings or in order to allow them to save face. He really went after them and did his best to demolish their arguments. That was George. And you don't make a lot of friends that way" (Conversation with Claire Friedland, October 1997).

Again, his habit of putting distance between himself and others does seem at odds with his clear generosity and kindness to both family and friends. However, being a friend did require you to accept that at moments you would be the target of his particularly sarcastic wit.

That's a good question. He was very generous. No question. He had a very tough exterior and he could be sarcastic and biting on people. George, if it was a choice between a good joke and making somebody feel bad, he'd take the good joke every time. That's how he was. He wasn't deliberately mean to some people, he didn't dislike them a lot, but he had such a terrific wit about him. You had to get used to it. It took me some time to get used to it. It took my wife a while to get used to it. But she began to love him too (Conversation with Gary Becker, October 1997).

That he could appear unkind was widely acknowledged. Stigler never seemed to recognize that just because one is able to be witty at someone else's expense doesn't mean one should. Yet, his remarks come across as largely unfiltered. If a clever remark came to mind, it simply seemed to be articulated no matter the target.

He could make mincemeat out of anyone, because he was so truthfully clever and objective. And the amazing thing is he was never, ever like that on an individual basis. I can see that distorted very much what people were saying about George. I think that was a flaw in his character. Okay, if he had wanted in one or two sentences to make me look like a jackass, but he would occasionally do that with younger people, or inferior, I don't mean that literally, I mean in rank, people with lower status than him. I thought that was terrible. He would just pounce on them. And I would tell him so. He would agree it was terrible. But he couldn't help himself, he couldn't resist (Conversation with Robert Solow, October 1997).

²⁸At times George Stigler could appear to take on the adversarial approach that defines the courtroom where victory is the only value that matters. To the extent that this was true, it seemed allied to a particular Chicago approach that was perhaps strained through the personalities of Frank Knight and Jacob Viner. The final ingredient was the almost unlimited appeal that controversy had for George Stigler.

I don't think you're getting at any thing that is really specifically George Stigler. I think you're getting at something that is (a) the atmosphere at Chicago, and (b) intensified by Knight. That an academic is concerned not with being diplomatic, not with trying to avoid hurting people's feelings, but an academic is concerned with saying what's right. Telling the truth, or trying to get at it. And if you disagree with somebody you don't say, 'Well, now there may be something in what you say.' You say, 'That's a bunch of nonsense' (Conversation with Milton Friedman, August 1997).

the work he undertook should not be dismissed. Such influences and determinants are easy sources of speculation, but almost impossible to pin down. The more interesting issue that can be tackled is what might have caused George Stigler to change his mind or, even more radically, whether any evidence would have possibly undermined his firm belief in efficient markets. Views provided during the course of my conversations are far from conclusive, but do provide interesting possibilities.

At the very least, taking empirical work seriously should imply a capability of being surprised by one's results. If however, one accepts Melvin Reder's conception that a belief in a Tight Prior Equilibrium²⁹ forms the core of the Chicago approach, the legitimacy of empirical research is undermined as evidence becomes the handmaiden to prior beliefs. A rather lengthy quote from Reder makes this abundantly clear:

Any appropriate inconsistency of empirical findings with implications of the theory, or report of behavior not implied by the theory, is interpreted as anomalous and requiring one of the following actions: (i) re-examination of the data to reverse the anomalous finding; (ii) redefinition and/or augmentation of the variable in the model, particularly the permissible objects of choice and the resource constraints; (iii) alteration of the theory to accommodate behavior inconsistent with the postulates of rationality (constrained optimization) by one or more decision makers (resource owners); (iv) placing the finding on the research agenda as a researchable anomaly.

TP [Tight Prior Equilibrium] implies shunning (iii): i.e. the subject matter of the tight prior is the adequacy of this approach to theory as an explanation of whatever behavior is considered as economic. It is not appropriate to characterize this prior as "dogmatic", and to suggest that it implies placing no credence in the possibility that (iii) is appropriate. It suffices to say that TP adherents—with variation among individuals—focus attention upon (i) and (ii) and, failing a quick resolution of the anomaly move to (iv) but pay little attention to (iii) (Reder 1982, p. 13).

The variety of identifiable and subjective views presented in regard to this issue is particularly interesting throughout the course of these extended conversations. All are based on observing the same individual and even the same set of actions. George Stigler undoubtedly was one of the great champions of empirical work. He conscientiously followed his own precepts in trying to make such evidence a key part of his own research. Ironically, the issue in interpreting the evidence of Stigler's performance is essentially the task of evaluating what is equally available for all to observe and examine. The problem lies in teasing out the meaning. Unfortunately, researchers all too often persist in bringing to a subject their own preordained interpretation.

²⁹Whether this truly characterizes the work conducted by Stigler, or, in a wider context, the Chicago School, is a contention that is expectedly controversial with no consensus at all forthcoming.

His instinct would be, say, the evidence needs to be re-examined very carefully and so on, and so forth. But, yes. I think he would be. 'If that's what the evidence is, that is what it is and we have to, we *have* to, you know at the end of the day if we are convinced that that is what the evidence is, that is the truth.' Not our pride. Our pride is not the truth. But he was complex... Who was it, Mel Reder who wrote a piece in which he said 'the distinguishing characteristic of Chicago economics is "the tight prior equilibrium"'. That was wrong, I told Mel it what wrong, but it is easy to see how somebody could come to that conclusion (Conversation with Sam Peltzman, October 1997).

The interesting thing is he was a great enthusiast for quantitative methods. So, it doesn't seem altogether consistent. But he certainly was. On the other hand, he knew what the answer was going to be. He just regarded it, as I say, as a way of persuading other people (Conversation with Ronald Coase, October 1997).

Clearly, George Stigler's views did change. The most notable instance is his analysis of monopoly and anti-trust. The young George Stigler seems to have followed the teachings of his mentors at Chicago, particularly Henry Simons and Frank Knight. His views, as expressed in a basic review of monopoly theory (Stigler 1942), are quite conventional. In more popular articles (Stigler 1952), he unreservedly sees big business as the scourge of market competition and willingly testified to that belief when appearing before the Subcommittee of the House Judiciary Committee in its study of monopoly power in the steel industry (1950).

Certainly, it is my belief—speaking as an economist and not as a lawyer—that the basic trouble with the anti-trust laws . . . is their failure to recognize that when an industry is made up of, or dominated by, a few big firms, there is inherently a structure inconsistent with efficient competitive operation (Stigler 1988, pp. 98–99)

Stigler (1988) has a chapter in his autobiography in which he elegantly describes his conversion from conventional thinking to a position where issues of monopoly or oligopoly are of minimal importance. Aaron Director, one of the few individuals who clearly influenced his thinking, must have played a key role here. Director, of course, a genuine iconoclast, had migrated to the Chicago Law School in the post-war period to fill the position vacated by the death of Henry Simons. "He was an iconoclast. But he didn't develop new data with respect to industrial organisation. He didn't develop and articulate new theories. He just said that the conventional belief wasn't so" (Conversation with Paul Samuelson, October 1997). He taught a course along with Edward H. Levi on anti-trust law, which could rightly be claimed as the Petri dish that brought forth the culture of Law and Economics. This newly established approach would then influence thinking in this area for many decades to come. Certainly, by the time George Stigler (1963) came to write a classic paper on oligopoly, there had been a clear break with his earlier thinking.

I think he is maturing as a person. He's getting away from Simons. He begins to understand that there must be good reasons why Eastman Kodak dominates the film industry. Obviously there must be market forces involved. Why wasn't capital flowing into an industry with high basic returns? He is asking himself the kinds of questions that just didn't occur to Simons. A guy like Simons would just say, 'Well, they're too big. Break them up! The text books tell you, *the more firms, the better*'. And that's it. Simple. End of story. Advertising screws up people's minds. Tax advertising and it'll be fine. So, he is beginning to understand that market forces are deeper than simple textbook stories. That's one of the lessons that I learned from him (Conversation with Sam Peltzman, October 1997).

His long-term research associate, Claire Friedland, makes this view clear. By embracing this approach, George Stigler became evidence driven. Nothing else but accumulated evidence would ultimately make up his mind.

George would never just write an article in which he said, “Yes anti-trust laws were mistaken, and a lot of the policies that they forbid were really efficient.” This is the result of the work that’s been done in Law and Economics over the last fifty years, since Henry Simons and especially due to Aaron Director. George would never say a thing like that without having an empirical test, because anybody can have a theory. George said, in his 1964 Presidential Address, you can find a theory to support any policy and the question is, “What is the evidence?” So I think of George as very, very empirically oriented. If he didn’t like something, like monopolistic competition and game theory, it was because he felt that empirically they didn’t hold up. He also felt that intellectually they didn’t hold up. But I think the empirical side of it was very strong. And not just empirical research, examining evidence, but whether it moved the profession ahead, whether it provided useful concepts to the profession (Conversation with Claire Friedland, October 1997).

Nonetheless, it is difficult to imagine the particular type of empirical evidence that would lead him to doubt market efficiency or to see regulation as advantageous. Again, this need not reflect a conscious choice. When faced with comparative mountains of evidence, choices would have to be made and results interpreted. Discovering exactly what we expected to find is a trap not easily avoided.

He would come across empirical work which was contradictory to other empirical work. Somehow it always seemed to him that the empirical work which favoured his side was done better than the empirical work which didn’t³⁰ (Conversation with James Kindahl, October 1997).

A third alternative presents a possible means of escaping from these two clearly marked extremes. George Stigler had a passion for consistency and an affinity toward comprehensive explanation. Employing this approach implies that empirical evidence clearly would have caused Stigler to change his mind, but only insofar as it accorded with the more consistent and clear-cut view of economics that he had developed painstakingly over his long career. Namely, his view on monopoly and anti-trust did evolve and become more in line with what he accepted as a mountain of undeniable evidence. But his hard-found convictions also sat quite comfortably with his unshakable belief in competitive markets. Results that reinforced that pre-existing predilection would have been naturally more congenial to him.

I think he went to a more satisfactory position, absolutely. The earlier view, as you say, he picked up, that was the literature, he hadn’t really thought it through. I mean, you know, he hadn’t thought through everything at that point, and he hadn’t really thought it out. As he thought through more and more, I think he came to a more

³⁰The key point here is that James Kindahl was George Stigler’s co-author of a pioneering empirical work on industrial prices (1970). As he made clear in our conversation, they very much complemented one another and he had great respect for Stigler’s work. But there was a pre-existing tendency to know what the results of this monumental study would be before the investigation commenced. What is interesting is that this could happen despite Stigler’s seriousness when undertaking empirical research and his clear attempts to be scrupulous. “What you saw was part of the ongoing development of my own scepticism. I worked with George Stigler who was one of the finest economists and thought more carefully than most others” (Conversation with James Kindahl, October 1997).

satisfactory thesis on the issue... But his views did become more consistent... Other people may not think so, but I think definitely that was true. He began to re-think some positions he had just inherited. Inherited you know, from his teachers and so on, or from the literature and he put it in a more consistent framework (Conversation with Gary Becker, October 1997).

B. The Ideological Honey-pot

I have just finished reading your essay “An Introduction to Privacy in Economics and Politics.”³¹ If I had a postcard with a picture of a famous economist like Adam Smith, Karl Marx or George Stigler I would write on it. But since I don’t have a postcard, I will just say that it looks like ideology to me (Letter from Douglas North to George Stigler, 10 August 1979).

No one would question that George Stigler was clearly conservative in outlook. His non-research output makes this amply clear. No one joined the Mont Pelerin Society³² out of a desire to advance causes favored by the radical left. As Claire Friedland remarked, “George was probably the last man in Chicago still wearing a fedora back in the sixties” (Conversation with Claire Friedland, October 1997). Why he became so receptive to conservative beliefs is an interesting question, but one perhaps better explored by employing techniques more familiar to other disciplines. Stigler saw himself as continuing in the tradition of the classical economists, especially as epitomized by the liberal tradition of John Stuart Mill. Like everyone else, he had a set of beliefs, an ideology, though possibly more defined than most. As pointed out before, his conservative beliefs did not lead him to consciously become an advocate for conservative causes through his work (specifically what we may term his “scientific output”).³³ In fact, as will be noted in the next section, he came to dismiss such advocacy as being essentially useless. But the real underlying issue is whether his very conservative beliefs, his belief in efficient markets and the efficacy of the price system, ever came to color his empirical and more technical research. Here opinions diverge widely.

³¹See Stigler (1980).

³²The Mont Pelerin Society was established in 1947 and named after the Swiss location of its first meeting. Deliberately created by Friedrich Hayek to stem the dominance of left-wing ideology in public discourse, George Stigler, as well as Milton Friedman, Frank Knight, and Aaron Director, were all charter members.

³³A long-running difference in Stigler’s approach to that of his close friend Milton Friedman was over the legitimacy of advocacy. Just as he eschewed pronouncing on economic topics outside his field of expertise, he also avoided, to a large extent, preaching the virtues of given policy positions.

And he, on more than one occasion asked me the question—he never gave the answer himself and I never knew how to answer the question myself—‘Did I think that Milton Friedman would be remembered most for his polemics on policy or his scientific work on things like the consumption function?’ He never gave the answer himself and I didn’t know what the answer to that question was. I still don’t know what the answer to that question is, but he obviously thought about this issue. This was an issue in his view and I would have to infer from his own behavior that he was of the opinion regarding his good friend and respected colleague, that Milton’s scientific work would be the hallmark by which he was most remembered (Conversation with Harold Demsetz, October 1997).

What is uncontroversial was his own position on ideology expressed often and in many places. Ideology just did not have an effect on economic theory. It ran orthogonal to his insistence on rationality.³⁴

I don't know how important ideology is, but think it is unimportant. You don't know how important it is, but think it is important. My position is better because I try—feebly and so often unsuccessfully—to use a trusted theory of human behavior to explain social phenomena. Your position is worse because you try—with marvelous ease—to explain the mysteries by a *deus ex machina* (Letter from George Stigler to Milton Friedman, 29 March 1984).³⁵

Unfortunately, one's stated intentions need not be consistent with what one actually accomplishes.³⁶ This may come down to the issue of interpretation and how one

³⁴There was an almost unanimous agreement that Stigler wanted to push the idea of rationality to its limits and beyond, unlike his mentor Frank Knight, who would be willing to place greater weight on emotion, or Mill, who saw the importance of custom.

Personally, Gary and George were much closer than George and I. Much closer and in many ways, I think, they were much more on the same wavelength, especially in this area. How far you could push rational choice, that kind of stuff. George was absolutely enthusiastic about everything Gary was doing, with the family, with marriage, with this, with that, with everything. But the outside world apparently wasn't. And apropos to the outside world, the graduate students at the end of the year would put on a party where they would perform a play. 'The economics of mud' by Gary Becker. 'The economics of—fill in the gap—' by Gary Becker. You know, it was a standing joke. It has become a standing joke. But George loved that kind of thing. As I do. I mean, I agree with it. I think it is a very powerful part of the message of economics that there's growth in its thinking, and standards and a reason to take economics seriously. But in terms of actually working in that area, the 'Tastes' paper, I told both of them I thought it was absurd. Gary and George were much closer in those terms than George and I was (Conversation with Sam Peltzman, October 1997).

³⁵As Stigler thought through many of the issues surrounding price theory, his objective seemed to encapsulate a need for a consistent theory, one that would increasingly be able to be generalized. Just as the elements of consumer sovereignty and rationality had to be the core explanation of economic activity, so it needed also to be the case for the political marketplace. In which case, ideology would need to be tossed aside when analyzing policy as well as market outcomes.

I guess I'll be willing to give a paper, although in some ways a scrawny one on the subject: *Laissez Faire: Policy or Circumstances?* It argues that public policies are not importantly influenced by ideology... It's scrawny because I have not been able to devise an acceptable line of testing (Letter from George Stigler to Roger Noll, Center for Advanced Study in the Behavioral Sciences, Stanford CA, 14 November 1983).

³⁶As both Sherwin Rosen and Gary Becker made clear to me in somewhat different ways, economists should be concerned with the actions people take, not what they claim their intentions to be: "Well, because I had the same view as George, on that issue. That I don't think you can talk with restaurant managers, in fact, about such things. You know they are not trained. They know in a certain deep sense, but they are not trained to articulate why things are happening" (Conversation with Gary Becker, October 1997).

If however I saw data based on responses to questions like: "How do you feel this morning? What do you feel about Richard Nixon? Did your wife and you have an argument this morning?" I don't pay attention to *that* kind of data. There is this definite bias in Economics. You see what people *do*, not what they say. Because, you can never competently judge their motives, or what is in it for them [laughs]. You've got to study their behavior, pure and simple (Conversation with Sherwin Rosen, October 1997).

decides whether there is a link between one's beliefs or ideology and the outcomes that research may produce. It does seem fair to say that Stigler strongly defended neoclassical price theory. He was skilled, as few others are ever able to be, at extending the reach of price theory and dispelling seeming anomalies.

He was interested, I would say primarily, in a particular sort of puzzle and it's a typical Chicago puzzle. And I don't mean that in any bad way. It's the sort of puzzle that the Chicago School's presuppositions require. Show me an apparent anomaly, something that does not seem to be explicable using the Smithian apparatus and the Marshallian apparatus and I will show you that it can be explained that way. That was exactly the sort of thing that George went looking for. And that's not a bad thing. I'd have to say that it can actually be very good... If you have a very clever man whose life is devoted to showing how you can explain apparent contradictions without altering the basic formulation, that's what you tend to do with empirical work as well (Conversation with Robert Solow, October 1997).

For this reason, it has been strongly suggested that the core work coming out of the Chicago School during the days of Stigler and Friedman was highly predictable. You knew what to expect before you began and the only item of suspense was exactly how the particular researcher would achieve this predetermined conclusion. Again, even were such claims to be true, it need not invalidate the work itself.

It seems to me that when you get to his later work, say with Becker, you know what the conclusion is going to be before you start the argument. In a sense, you're assembling arguments to support a conclusion. I mean, that may be unkind and untrue but it's an impression. And, it's even more so in the work of Richard Posner. Have you read any of that? It seems to me that the plot is always the same, and the characters stay fixed (Conversation with Ronald Coase, October 1997).

Some, such as Paul Samuelson, would take a diametrically opposed stance, not accepting that economic work is generally ideologically free. Furthermore, Samuelson proved willing to support his case with the sort of empirical evidence that George Stigler would automatically demand to see. (Whether Stigler would accept such evidence is, of course, an interesting question.)

Well, this is a popular Friedman view too. And it's wrong. I say that flatly. But it's interesting—I have somewhere a National Bureau Yellow Jacket manuscript of a research study by Victor Fuchs from Stanford University, Jim Poterba from this University and Allan Krueger of the Woodrow Wilson School at Princeton.³⁷ They did an extensive sampling of economists in two areas of economics. And what they did was they gave a whole set of questions on what each person's factual opinion was on that question. And so he had all these factual differences in the group. But they also asked questions about their value judgements. They asked these in whole different areas. The third less important area were their political affiliations, which I presume these days

³⁷Fuchs, Victor R., Alan B. Krueger, and James M. Poterba (1997). This was later printed in the *Journal of Economic Literature* (1998). Curiously enough, Paul Samuelson has a published article in the same volume.

would be Republican or Democratic, but they might have gone further. Then they tried to see how you explained the differences in policy recommendations. And their finding is the opposite of Milton Friedman's. There was very considerable degree of consensus on factual matters. There were some differences in the degree of confidence they had in their answers. The confidence intervals varied quite considerably. Now what they found was the difference in their policy recommendations were—I'm using your language, not their language—ideologically premised values. They were not fact driven. Now there are a few cases like the minimum wage, or Ricardian comparative advantage, where you can almost get certain unanimity, free of ideology. But these are exceptions in my opinion (Conversation with Paul Samuelson, October 1997).

Again you might argue that George Stigler, in his most important work, deliberately avoided any hint of preaching or any concerted attempt to push forward certain policy positions. Although this may not be overt, still, even in the core of his more empirically founded work, the possibility that results lined up all too easily with ideological presuppositions cannot be entirely ruled out. Again, this would not be unique to George Stigler. However, it would be particularly worth noting in such a careful and conscientious economist (and one of the best of a remarkable post-war cohort).

Oh, he was a true believer. He wouldn't like that term. But put that in because he thought in that sense. He was absolutely convinced that he was right. It wasn't a doctrinal battle. It was a battle of facts. I almost said good and evil. He was sure... . He was absolutely sure the economy was on his side and if research was properly done it would show this. He really believed that he understood how the world works. And the way the world works had been shown to him by the theory of price... . His [Gardiner Means'] facts were wrong. Wrong. Wrong. Wrong. And he knew it. How could they be anything but wrong if he thought the economy was not competitive? . . . My guess is that he came to his conclusion from an *a priori* position. But then my guess is that most economists make their judgements on *a priori* beliefs and not on empirical evidence (Conversation with James Kindahl, October 1997).

In an almost unconscious reprise of the conundrum facing George Stigler himself, each of those interviewed faced similar evidence concerning how this consummate economist conducted himself. But, as noted before, evidence does not speak for itself. What I was able to gather is some very sharp differences in subjective evaluations, which raise interesting questions without providing any definitive answers.

I gradually realised, I don't know when I realised, that he was one of the most fascinating examples of how economists act even though they say that economics ought to be value-free (and it is sometimes value free). I find it extremely difficult to resist reading a lot of economic theory, politically. Now, you can see the way George Stigler had a wonderful nose for attacks on neo-classical economics that were dangerously capable of undermining the very foundations of orthodox economics. It's no accident that he went for Gardiner Means' *Administrative Prices*, Harvey Leibenstein's *X-Efficiency*, Galbraith's *Affluent Society* and his other thing, the *Kinked Oligopoly Curve* of Sweezy. These are all ideas that are very, very dangerous, subversive even of orthodoxy. It's those ideas which roused his critical fire. Obviously he thought his views about the markets and so on were identical to Milton Friedman's but they were much

more carefully disguised. He never, (unlike Milton, came out with them openly, or wrote about them, as Milton did, in *Capitalism and Freedom*, where Milton sort of let go. You know, you could read right through all of George Stigler's stuff, even on industrial organisation, and his implicit endorsement of markets, and the condemnation of all government intervention, which of course eventually inspired his work on *The Theory of Regulation*. This proved to be an example where ideology is productive. The trouble with ideology is that it can blind you, but it can also sometimes create a spurt. Who would have thought up *The Theory of Regulation*, unless you were already inclined to regard all government action with deep suspicion, always inclined to believe it does more harm than good (Conversation with Mark Blaug, October 1997).

C. *The Paradox of Legitimacy*

But what exactly he would allow as a proper realm for government? He got into this—I remembered his name for it after talking to you—'paradox of legitimacy' he called it, or sometimes he called it the '*problem* of legitimacy.' At the time of his death, this was the problem he was working on. It was *very* much of concern to him... But he was very much concerned about how you could call something inefficient if the political arena, where we do have a democracy, more or less, where we do have representative government, has allowed it to happen (Conversation with Claire Friedland, October 1997).

I was particularly struck by an article published posthumously by George Stigler (1992), the last work he completed before he died. What made it interesting was his seeming defence of the indefensible. The article in essence starts off with a defence of US sugar subsidies. It seems to be a version of a market dictum attributed to Armen Alchian; namely that what is, is efficient. (Underlying this supposition is once again a sense of the test of time. Any inefficiency would create an incentive for individuals to change the existing status quo.)

In this latter view, every durable social institution or practice is efficient, or it would not persist over time. New and experimental institutions or practices will rise to challenge the existing systems. Often the new challenges will prove to be inefficient or even counterproductive, but occasionally they will succeed in replacing the older system. Tested institutions and practices found wanting will not survive in a world of rational people. To believe the opposite is to assume that the goals are not desirable: who would defend a costly practice that produces nothing? ...

Consider the following example. The United States wastes (in ordinary language) perhaps \$3 billion per year producing sugar and sugar substitutes at a price two to three times the cost of importing the sugar. Yet that is the tested way in which the domestic sugar-beet, cane, and high-fructose corn producers can increase their incomes by perhaps a quarter of the \$3 billion—the other three quarters being deadweight loss. The deadweight loss is the margin by which the domestic costs of sugar production exceed import prices. Lacking a cheaper way of achieving this domestic subsidy, our sugar program is efficient. This program is more than fifty years old—it has met the test of time (Stigler 1992, p. 459).

According to Claire Friedland (Conversation, October 1997), this was the problem occupying George Stigler's mind in the last years of his life. It is not, in fact, difficult to see why he was driven to it or why it generated controversy even among his closest colleagues. Essentially, this assigned a very minimal role to economists in policy matters, if any at all.

'Consistency is the hobgoblin of small minds,' somebody once said. He was not consistent. That's clear. And this inconsistency has led to a lot of misunderstanding. There are people outside of Chicago who read him this way. With Becker it is even more powerful. It's all part of Becker's stuff about optimality and redistribution. Outsiders kind of read both of them as 'This is kind of the senescence of the Chicago school. They have become toadies for big government, apologists for big government.' And I could see why. It is a really subtle kind of distinction we are making here between the two. But look, if you're going to regulate, conditional on wanting to redistribute income, I can't tell you that this is wrong. So, if I don't like it, if I tell you it's wrong, it has to be because I don't like the resulting redistribution (Conversation with Sam Peltzman, October 1997).

This controversial approach is really a matter of taking the ideas of consumer sovereignty and rationality seriously, which George Stigler undoubtedly did. If we remember Stigler's drive for greater consistency and comprehensiveness, his grappling with this final issue becomes almost inevitable.

Something he was thinking about was how to reconcile consumer sovereignty, or voter sovereignty, with his previous notions of inefficient government. Can we say this is illegitimate if the public wants it? Is that consistent with our extreme position on consumer sovereignty, which is that no matter what horrible things the public wants, as free market economists we can never question it. That's certainly one of the basic principles of neo-classical economics. Consumer sovereignty is both the end of the story and the beginning (Conversation with Claire Friedland, October 1997).

Starting from this premise, we must logically return to the tenet fabricated by Armen Alchian, someone "who is more Catholic than the Pope, who never went to University of Chicago but is a real Chicagoan" (Conversation with Paul Samuelson, October 1997). What must be what is most efficient.³⁸ To emphasize this point once again, if markets meet the test of time, then an inefficient situation must eventually be replaced by an efficient one. There is clearly an incentive for some entrepreneur to attempt to more closely meet consumer preferences if they are not currently being met and met in

³⁸The distance from this to John Stuart Mill's methodological credo is far from unbridgeable. Rather, the two reveal a distinct union of spirit.

Since, therefore, it is vain to hope that truth can be arrived at either in Political Economy or in any other department of the social sciences, while we look at the facts in the concrete, clothed in all the complexity with which nature has surrounded them, and endeavour to elicit a general law by process of induction from a comparison of details; there remains no other method than the *a priori* one, or that of "abstract speculations" (Mill 1965, pp. 148–149).

We need only to chant, along with Hegel, "Das Denken ist das Sein," to complete the picture.

an efficient manner. Seizing such opportunities is how economic profits are gained. This must extend to the political marketplace as well, again with a lag, but certainly over time. If the current government or set of existing institutions are not meeting voters' preferences, then they will be replaced. The parallel logic tying together the two markets is compelling. Competitive markets of any type must ultimately align themselves to reflect individual preferences. It is, then, no more the role of an economist to question the objectives of those political voters than to evaluate the choices consumers make in the marketplace. Besides, as Stigler (1971) has pointed out, legislation, especially in the case of regulation, reflects relevant interest groups. Accepting these imposed constraints, an economist's role is automatically limited to understanding the political arena. Despite concentrated effort, this particular market mechanism will continue to run its course unimpeded by the policy prescription of any academic.³⁹

Milton Friedman: There's no problem. It's true, that George did want to change things.

Aaron Director: But he preferred to study them, not to change them.

Milton Friedman: He preferred to say that he preferred to study them.

Aaron Director: He preferred to study them. I should quit the argument.

Milton Friedman: It was partly a long-running difference between him and me.

Aaron Director: You're right.

Milton Friedman: And he liked to stress, 'I just want to understand the world and Milton wants to change it.'

Aaron Director: That's right. And predominantly I think that is correct.

Rose Friedman: You would have to have them both psychoanalyzed (Conversation with Milton Friedman, Aaron Director, and Rose Friedman, August 1997).

It is perhaps these policy implications or lack of policy directions that make Stigler's drive for consistency in his work and thought most controversial. If we are to evaluate the Chicago Revolution in economics, along with its connection to the Mont Pelerin program, then the dual aims seem to consist of undermining the post-war Keynesian orthodoxy, both in its theoretical approach and its policy imperatives. George Stigler's direction toward the end of his career (one which had been growing for a number of decades) quite clearly puzzled many, especially those within his own camp.

I'm sure that if you ask him almost any question of economic policy he was right next door to Milton or me or any of a number of more or less free-market oriented

³⁹For Stigler, the individual could do little. He or she was, for example, 'free to choose' because power essentially is surrendered to the workings of the marketplace. In the same way, politicians acting in a democracy largely surrender to the forces of the political marketplace and affect events only at the margin. That, for Stigler, is the advantage that such economic and political systems have over those of collectivism and totalitarianism. "The men who get elected in a landslide can get some things done for a year or so before their hands are tied. Otherwise, presidents affect only the important social, political and economic trends marginally" (George Stigler quoted by Ramsey 1984, p. 8)

people with a great deal of respect for market forces, etc. Yet, almost I would say not too long after *Roofs or Ceilings?*⁴⁰, George began to pull away from advocacy and one of his favorite sayings was that over the course of the centuries, on the issue of free trade, the economists win all the debates in the halls of the universities but the protectionists win all the debates in the halls of Congress... I think that the reason why you should know why people get dumb policies passed is to learn how better to fight against those dumb policies, rather than any other reason. I am a different style of missionary from Milton, but I'm definitely a missionary. I never thought of myself in any other way. I have strong views about what is good policy, bad policy, in thinking that economics has a lot to teach us, even when it is unpopular, even when governments don't do it... I don't think much of this fatalistic view of these political economy guys that it is just determined by these underlying factors and what can we do about it? I think that would be a terrible vision. In fact, I don't think George was there. But why was he as reticent as he became over the last decades of his life? ... So, the question really is, how does the one George transfuse into the other, or how do these two Georges live side-by-side in the same head. That is a puzzle that I've always had (Conversation with Arnold Harberger, October 1997).

What comes across in these series of conversations is the impression that, despite his great respect for Milton Friedman, George Stigler went his own way according to his own lights. He examined the issues and evidence, and did not accept claims blindly. (Though it does seem the case that he was more than willing to defer to Milton Friedman in matters focused on macroeconomic issues where he refrained from claiming any great comparative advantage.)⁴¹ The two large influences in his later years were Aaron Director and Gary Becker. It is only by understanding Stigler's relationship to these two close friends and colleagues that we may begin to puzzle out his paradox of legitimacy.

⁴⁰Arnold Harberger refers to the only formal collaboration between George Stigler and Milton Friedman (1946), a pamphlet commissioned by the Foundation for Economic Education and later distributed in a condensed form by the National Association of Real Estate Boards. The purpose of the pamphlet was to directly shred the post-war policy of rent control. Ironically, the authors ran into some bitter controversy when the foundation discovered that the two turned out to be ideologically suspect on the matter of income equality. The disputed sentence: 'For those, like us, who would like even more equality, not only for housing but for all products, it is surely better to attack directly existing inequalities in income and wealth than to ration each of the hundreds of commodities and services that compose our standard of living' (Stigler and Friedman 1946, p. 10). This apparently subversive statement warranted a footnote from the commissioning foundation. For those interested in the details of this surprising controversy, see Hammond and Hammond (2006).

⁴¹This is in distinct contrast to those economists who are happy to pontificate at large to a variety of media outlets.

I imagine when all's said and done—I don't remember George writing particularly on the real bills doctrine or the quantity theory—George would say 'inflation is everywhere a monetary phenomenon.' This is like taking a personality loan from people whom he admires, who believe that kind of thing. More than most, I think, George kept out of things that he felt he wasn't entitled to an opinion on. Most economists would say 'How do you spell 'gold'? And then they'll tell you what we should be doing about gold or anything else that you can imagine (Conversation with Paul Samuelson, October 1997).

On the whole, if I tell you what I think, I will tell you only what I think. But I will also tell you that this is almost an *a priori* statement. I think that George was more broad minded than Gary Becker, and so a collaboration between them would have been good for Gary and bad for George . . . but that's a purely *a priori* statement. I really don't know what it was like when they worked together (Conversation with Robert Solow, October 1997).

IV. A POSTMAN BIDS FAREWELL: WHAT WAS GAINED

'Indifferent! Oh no—I never conceived you could become indifferent. Letters are no matters of indifference; they are generally a very positive curse.'

'You are speaking of letters of business; mine are letters of friendship.'

'I have often thought them the worst of the two,' replied he, coolly. 'Business you know may bring money, but friendship hardly ever does.' (Austen 1996, p. 98).

Going through the Stigler papers at the University of Chicago, I was struck by the sheer volume of correspondence.⁴² These are the sort of letters almost no one receives today. Where twenty years ago I would purchase frequent aerograms to keep in touch with distant friends, such overseas post is almost never seen today. Personal letters have shrunk to fill an occasional postcard or birthday greeting. Instead, we have the Internet and text messaging. Certainly, almost all professional discussion and collaboration among academics come via email. For all the great advantages in speed, this bonus inevitably comes at a cost. The quality and character of communications seem to change as the underlying technology evolves. Today, messages and ideas are transmitted virtually in an instant, making it easier to transcribe pure stream of consciousness rather than deliberated thought. The transmissions are not only more ethereal in substance but also in pure longevity. For centuries, historians have pawed their way through piles of often revealing letters. In the future those files will be empty. Such communication is essentially vaporized soon after receipt.⁴³ Given this forbidding reality, interviews may gain even greater prominence in piecing together

⁴²One of Stigler's secretaries mentioned that periodically she would be instructed to go through the files of letters and thin them out. Fortunately for future historians of economic thought, she was always called off the task to accomplish more pressing matters. As a result, correspondents at the end of the alphabet generally fared better.

⁴³Technological Polyannas inevitably believe in a better tomorrow. For example, emails are now being archived at many universities, making the next generation of researchers more privileged, if not more blessed. The underlying assumption here is an unwavering belief in digital solutions to all problems. However, my own experience, and that of others with whom I have discussed the issue, is that universities are often incompetent at accomplishing technological, administrative, and almost any other set of objectives imaginable. They often lose emails, either in the process of archiving them or when some administrator decides to switch to some other mail program. Fans of all things new also share a curious conviction about the duration and durability of such storage. Still, ignoring all such technical quibbles, the prime problem remains. Emails and letters are not really close substitutes for one another. The former is no more than a pale version of the latter. The medium of communication influences the style and content of that communication. I remain unconvinced that emails are somehow superior in content to that of the standard letter.

the past. In any case, the potential value of the interview should be self-evident. Who today wouldn't like to be able to sit down to a tête-à-tête with Adam Smith or chat with his great friend David Hume about Smith's thought and idiosyncrasies?

More than anything else, talking to the people who were there for an event, in this case the Chicago counter-revolution, gave me a distinct feel for life in the trenches. I understood more about the way in which the battles were fought and why. In particular, subjective feelings and viewpoints come across in interviews in a way they never can by using any other investigative technique. Being in Chicago and talking to so many people who were part of the battles Stigler and Friedman fought, I began to understand, if only imperfectly, the intensity with which they practiced their trade. And I could see how at least some of that intensity emanated from George Stigler, his conscientiousness, his unwavering puritan ethic, and an almost workaholic personality. When faced with people of such accomplishments, my reaction is usually to wonder when they ever found time to sleep.

The result of all these interviews was to shine a rather diffuse light on a complex character, perhaps, in doing so, highlighting the complexities involved in tackling first-rate economic work. "I really feel that he is one of the most interesting and in some sense, most enigmatic characters in our profession in this last century" (Conversation with Arnold Harberger, October 1997).

No one who spoke denied the care and conscientiousness that George Stigler lavished on his data. Few, then, would feel moved to quarrel with his son's considered observations.

I could only quote my experiences with him in other instances. He was open to being convinced by contrary evidence. He did feel that if you have something that has proven itself through one sequence of tests, that the evidence to overturn it had to be conclusive. One did not overthrow theories casually (Conversation with Stephen Stigler, October 1997).

However, there is another sense in which a wide array of people also concluded that he heavily marketed his results until they squeaked. He championed his underlying conviction of market efficiency almost by *force majeure*. It was not sufficient for his theory to account for a large majority of all the available evidence; it needed to encompass 100% of the total.

I don't think it was clear in his mind what the distinction was, between stating a powerful position that covers a lot of the cases, and what happens in a particular event. When confronted with a particular application he literally almost believed it. He wouldn't back off and say 'Well, look, that's part of the remaining 10 or 20 percent, which is doing better than explaining just fifty percent—tossing coins—that's just part of the noise.' Methodologically he understood that but in his gut I don't think he really understood that (Conversation with Sam Peltzman, October 1997).

Anyway, the essence of it was that the people there absolutely denied anything about price flexibility, hidden discounts or things like that. They put out a price book and that was that. Everybody paid the same price. There were quantity discounts that were clearly stated in this price book. Anyone who bought large quantities got the

large quantity prices, and small quantities got small quantities prices. George Stigler was clearly disappointed in that. He came back and he told me, 'Well, we'll get to the bottom of this' (Conversation with James Kindahl, October 1997).

Perhaps a need to sell his own results in the marketplace for ideas induced him to assume a cloak of unshakable confidence he truly did not feel.

You have to remember this kind of public persona he had. If something bothered him a lot I saw the side of him that said, 'I don't know what to do about this problem!' But the rest of the public saw that other side. 'Here's what I've *done* about this problem, and isn't it convincing.' (Conversation with Claire Friedland, October 1997).

He was, however, reluctant to admit mistakes.

I told George about it.^[44] I asked him what to do about it. This was some twenty years later. George's answer was that there was no point in making a big fuss about this mistake because it was twenty years ago and nobody cared... But we have never done anything *publicly* to correct our mistake (Conversation with Claire Friedland, October 1997).

How much, if any, of this was ideological in essence and how much of it attached to which aspects of his work is difficult to determine solely from the various conversations undertaken. For all Stigler's fair-mindedness, he saw the perceived threats to liberty and freedom in the fifties and sixties as dead serious.

The proof that there are dangers to the liberty and dignity of the individual in the present institutions must be that such liberties have already been impaired. If it can be shown that in important areas of economic life substantial and unnecessary invasions of personal freedom are already operative, the case for caution and restraint in invoking new political controls will acquire content and conviction (Stigler 1975, p. 18).

This led to a seeming contradiction that identified him clearly as a vocal critic of the campus excesses and the constraints posed to academic freedom in the sixties, yet recognized his willingness to be mute during the fifties reign of terror perpetuated by the advent of McCarthyism.

I really have absolutely no idea. But it wouldn't surprise me that he had a schizophrenic attitude to attacks on intellectual leadership depending on which direction it came. A lot of right-wing people must have found McCarthy to be vulgar,

⁴⁴Claire Friedland refers here to her joint paper with George Stigler (Stigler and Friedland [1962]). See Sam Peltzman's (1993) insightful article on Stigler's work on regulatory theory for a discussion of the error and its consequences.

It was a decimal point error. That's why it was such a terrible error. Our coefficient was wrong by a factor of ten. But our standard error was off by a factor of ten as well. So our t-statistic was OK. But our remarks about the size of the effect were wrong. This was all entirely my fault because I'd never seen a computer before and I had made a mistake configuring the data (Conversation with Claire Friedland, October 1997).

brutal but basically less of a problem than the alternative. Basically on the right side of the issues. Not unlike what you read that people said about Hitler, in the early '30s, when he was on the rise. Yes, he's a gangster. Yes, he's a hooligan but he's anti-communist. He's going to keep back the communists and control trade unions. These are benefits. And they didn't realise that they were rearing a Frankenstein's monster who would eventually chop off their own heads. A lot of people were sympathetic, who would have been horrified if they had foreseen what would happen (Conversation with Mark Blaug, October 1997).

What does appear true about all academic ideologues is the one-sided nature of their skepticism. While those on the left are deeply skeptical about market outcomes, they may hardly question the results of potential government intervention. On the right, the tinge of iconoclasm shifts in the opposite direction. Markets can do no wrong and government intervention is automatically deemed guilty unless proved innocent. While sounding absolutely assured is the key to good marketing, more modesty and a willingness to acknowledge what we do not know may get us into less trouble.

It's hard to imagine an empirical observation that would convince most members of this department [University of Massachusetts] and the University of Chicago to change their minds. My personal view is that if someone holds a view it cannot be dislodged by any conceivable empirical data. Evidence from a data system doesn't convince them. These people have made their decisions already. They've become true believers and no amount of empirical evidence will ever convince them by definition (Conversation with James Kindahl, October 1997).

From these conversations, the question of whether Stigler could be moved only by certain selective mountains of evidence remains unresolved. What does come across is his need to market his ideas, which acted as a curb on his otherwise healthy sense of skepticism. Successful marketing moves us away from the sort of skepticism and essential modesty that Knight claimed was a key component of a scholar's personality: "Frank Knight ... tells us that the 'basic principle of science—truth or objectivity—is essentially a moral principle, in opposition to any form of self-interest. The pre-suppositions of objectivity are integrity, competence and humility'" (Coase 1994b, p. 15).⁴⁵

If we take Stigler's own work to heart, to succeed in the marketplace for ideas, you have to focus on meeting your listeners' given preferences, convincing them that what you are selling meets their requirements. Unfortunately, this may entail a sacrifice. As Gary Becker points out, success in terms of marketing need not lead

⁴⁵This quote is particularly apropos given the deep regard in which George Stigler held Frank Knight, despite his later theoretical break. However, when Ronald Coase was asked whether Stigler could be characterized as acting with humility, he replied:

He didn't show it. He may have had it, but it wasn't that apparent. He always appeared confident, sure of himself, but in a way that suggests to me a sense of insecurity. I don't know whether others felt that, but I did. He was always very nice and kind and helpful in many ways. Always. But I often wondered how far he agreed with what I was saying. I think he thought I was all right, but a little odd (Conversation with Ronald Coase, October 1997).

to a desirable outcome, one to which George Stigler, given his abiding respect and devotion to the economics profession, would happily subscribe. Whether Stigler fell into this seductive trap where *a priori* beliefs determine factual findings is a question posed by these conversations rather than resolved by them. An honorable man may be true to his methodological reasoning and techniques, but be possessed by a creed at times so powerful that it can blind him to uncongenial facts or at least leave him feeling that he needs must willfully ignore whatever contradictory evidence arises.

It may be that in the long run good ideas do surface but they surface faster, if written in a persuasive fashion. Moreover, bad ideas may be put persuasively. And they may gain the necessary threshold. However, taking that same analogy in competition among ideas, there is a presumption, although not a certainty, that in the longer run, the good ideas are going to compete out the bad ideas. But that may take a long time and may not even always operate. There's nothing necessary about that. Nothing guaranteed about that (Conversation with Gary Becker, October 1997).

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