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The Chicago Counter-Revolution and the Sociology of Economic Knowledge

Robert Leeson

University of Notre Dame Australia, rleeson@stanford.edu

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Chapter 3

The Chicago Counter-Revolution and the Sociology of Economic Knowledge

7.1 Introductionⁱ

7.1.1 Outline of the Essay

George Stigler and Milton Friedman sought, and achieved, great influence both *internally* (within the economics profession) and *externally* (over a wider constituency).ⁱⁱ The intellectual and policy transformations that occurred from the late 1960s were the combined product of internal forces (apparently impressive historical relationships between money and prices etc.) and external anxiety about perceived policy failures (high inflation and rising unemployment etc.). There is, of course, a link between the two (Keynes 1936, 383-4). Stigler's *Theory of Price* was highly influential both within and outside the economics profession. When Stigler (1969a, 146) testified before the House of Representatives Select Committee on Small Business that his "own goal is a competitive economy" (not a goal shared by Edward Chamberlinⁱⁱⁱ), he was informed by the General Counsel that "this subcommittee has a long record of being greatly infatuated with some of your ideas"; understanding his textbook had cost "many hours of sleep" (Potvin 1969, 150, 152).^{iv}

This chapter is concerned with the *internal* phenomenon. One model that can be invoked to explain this internal phenomenon is the classical process whereby evidence is patiently accumulated until the weight of the argument favours one side or another. Alternatively, Stigler's 'model' of the sociology of economic knowledge construction and destruction can be used to examine the internal opinion-changing process in the "transition from the overwhelming defeat of Barry Goldwater in 1964 to the overwhelming victory of Ronald Reagan in 1980 - two men with essentially the same programme and the same message" (Friedman and Friedman 1982, viii).

This essay takes the second route and constructs what Stigler (1982a, 92) called a "science of science" explanation for the post-war Chicago academic and policy revolution.^v It is inspired by Stigler's (1983a, 544) Nobel Lecture - "learning more about how this search for new knowledge proceeds is itself a worthy search for new knowledge", and by his reflection that "It is true that in policy there is no tenable distinction between education and propaganda" (1967a, 285). Section 2 presents an overview of the 'sociological' perceptions of both Friedman and Stigler. Section 3 describes six aspects of Stigler's 'model' of the sociology of knowledge: the overwhelming hegemony of theory in economics (3.1); the crucial importance of internal

developments as the primary force behind scientific change (3.2); his distinction between the elites and the masses in any discipline and the ability of the former to set the professional agenda (3.3); the usefulness of the technique of the huckster in popularising economic ideas (3.4); and the unpredictable and therefore - to those in an hegemonic position - dangerous characteristics of the fox hunts of controversy (3.5). The sixth characteristic of Stigler's 'model' is a description of how to relegate to obscurity notions that might otherwise achieve prominence or even dominance (3.6).

The methodology of positive economics instructs researchers to tease out the *implications* of models and theories and to compare these implications with observed behaviour. Stigler's 'model' implies that paradigmatic challengers should unleash the unpredictable fox hunts of controversy, whilst those in hegemonic positions would be wiser to resist such challenges. Chicago *macroeconomics* was the paradigmatic challenger and Friedman successfully engaged his Keynesian opponents in a manner that could only undermine orthodoxy (Johnson 1972; Leeson 1998). Chicago *microeconomics* had powerful competitors originating from the two Cambridges; but "The American Way ... is to rely on competitive private enterprise" (Stigler 1969b, 1) and Stigler and Friedman declined to provide combustible material which could fuel the monopolistic competition revolution. Section 4 outlines the emergence of what Chamberlin described as the "Chicago School of Anti Monopolistic Competition". Section 5 describes Chamberlin's empirical challenge to Chicago (5.1); Stigler's views on the importance of testing economic theories and his encouragement of new and difficult research projects (5.2); and Stigler's and Friedman's exceptionally brief but firm refusal to engage either Chamberlin or Archibald in the kind of *microeconomic* race that Chicago was successfully initiating at a *macroeconomic* level: "The low point in the fortunes of imperfect competition" (Sutton 1989, 226) (5.3). Concluding remarks are provided in section 6.

This essay attempts to impose a framework on Stigler's thought that he would have recognised as a fair representation.^{vi} Two types of (diametrically opposed) readers may mistakenly conclude that I am inferring that Stigler and Friedman engaged in some sort of conspiracy (a view which Chamberlin came close to expressing^{vii}). The Chicago counter-revolution influenced the Thatcher-Reagan agenda (a topic that is outside the scope of this essay) and its opponents feel they were manoeuvred from influence by devious means. Equally, some Chicago economists such as Harry Johnson (1971) and Don Patinkin (1969, 1972) have inferred that they detected some Chicago "chicanery" in operation.^{viii} D. McCloskey (correspondence 2 June 1997) also observed that "I am one Chicago economist who can attest that George was little interested in scientific evidence, though he talked about it a lot - because he knew the culture honored it and he wanted to seem a scientist".^{ix} But other Chicago economists have informed me that they regard Stigler as having been driven primarily by the weight of scientific

evidence and only marginally by the sociological perceptions discussed in this essay.^x Indeed, one of "Stigler's Laws" was paraphrased by Harry Johnson (1976, 19) as "the more scientific it is, the less sociology helps to understand it". But it is reasonable to infer that Stigler was motivated by a determination to further Chicago-style policies and perceptions (because he concluded that these perceptions were well supported by the evidence). This essay is concerned to delineate the sociological influences that Stigler and Friedman brought to bear on their economics in general and the monopolistic competition revolution in particular. What follows is based on the assumption that Stigler's stated views on the sociology of economic knowledge are a reliable vehicle for such an analysis.

2 Stigler and Friedman

Stigler was a perceptive amateur sociologist of economic knowledge; he was awarded a Nobel Prize for his work on information theory and the functioning of markets, which he regarded as his most important contribution (1988b).^{xi} He also wrote about 'The Direction of Economic Research' and the market both *for* economists and *within* economics (1991, 1963b, 1967a, 1982a). Stigler thought that he had penetrated the veil of "idealistic view[s]" which "misdirects attention" but which are "deeply imbedded in professional economic thought. His 'model' of the sociology of economic knowledge was repeated in his Nobel Lecture (1983a, 536, 538, 542) and elsewhere; it consisted, among other things, of a human capital explanation of the reason why ideas in economics can be either fertile or sterile. His American Economic Association Presidential Address on 'The Economist and the State' began with a discussion of Smith's belief in the "efficiency in the system of natural liberty" (1965a, 2), and contained several perceptive comments about the sociology of economic knowledge. These themes recurred throughout his career and informed his choice of research topics and (presumably) his method of popularising the Chicago message. His perception was that there were basic similarities between 'Economic Competition and Political Competition'; and that political decision makers were endogenous participants in the political-economic process (1972a). Stigler (1987a, 52) "frequently found great comfort" in reflecting on the confusion and misinformation that circulated among elites of which he disapproved.^{xii} He (1963c, 109) advocated competition between alternative perspectives on the grounds that it was unwise "to give all our baskets to one egghead". This paper employs Stigler's analysis to explore the competition between the Schools of economists that sought influence in the postwar period.

Milton Friedman never devoted as much ink as Stigler did to these sociological themes; but he was also a gifted amateur,^{xiii} in addition to being a revolutionary historian (Friedman and Schwartz 1963). He and Stigler were, intellectually and personally, very close: "Mr. Micro and Mr. Macro at Chicago" (Becker 1993, 762).^{xiv} They were also both affiliated with the Mont Pelerin Society^{xv} and the Hoover Institution. Friedman (1986, 84) acknowledged his general intellectual debt to Stigler,^{xvi} and it seems likely that he was influenced by Stigler in at least

three crucial areas; his methodology of positive economics (section 5.2), running regression races between the quantity theory and the Keynesian consumption function (section 5.3) and the idea that misinformation in the labour market was responsible for deviations from the natural-rate of unemployment.^{xvii} They were part of that extraordinary wave of Nobel Prize winning Chicago economists: Friedrich Hayek (1974), Friedman (1976), Theodore Schultz (1979), Stigler (1982), Merton Miller (1990), Ronald Coase (1991), Gary Becker (1992), Robert Fogel (1993) and Robert Lucas (1995).

Stigler (1988a, 108-9) concluded that "by 1980 there remained scarcely a trace of the two Harvard traditions of Chamberlin and Mason in the current work of economics"; but monopolistic competition now appears to be fulfilling the promise that Harrod (1934, 470) discerned (it is currently central to new trade theory, game theoretic models and the New Keynesian revival). He had previously accepted, in part, the profession's "hopelessly exaggerated opinion" concerning the importance of monopoly; but by the end of the 1950s he thought that "the doctrine" of monopolistic competition was "exhausted". It is likely that Stigler contributed to this process: his "negativism probably hindered further development ..." (Rosen 1993, 816).^{xviii}

Hostility to various approaches to economics was not unique to Chicago and to Friedman and Stigler. Keynesians were hostile to the revival of the Quantity Theory, but they *failed* to undermine the revival. In contrast, the Chicago School were (at least for a while) *successfully* hostile to monopolistic competition, which according to Bishop (1964, 36) had become widely accepted and was opposed only by "some determined pockets of resistance". At an AEA symposium on Monopolistic Competition, the postwar Chicago tactics were perceived to have been counterproductive to the Chicago cause (Baumol 1964, 44; Bain 1964; Markham 1964; Steiner 1964). These, or similar perceptions, may have persuaded Friedman and Stigler to decline the invitation to be drawn into empirical testing. This paper argues that the *tactics* of this hostility were consistent with Stigler's understanding of the sociology of economic knowledge.^{xix}

Stigler argued that "the chief work of economic theorists should for the present still be in the theory of perfect competition"; he was also concerned that formalist virtuosity and "a mad scramble for originality [by] our younger theorists", had led to a "poor use of the received doctrines". He (1954a, 9; 1939, 481) wrote disparagingly of "the tones of a stuffy formalist"; and he sought to "temper [the] over-enthusiastic devotees of the cult of correlations". An "indefinite period" (1955a, 300) could be devoted to the new analytical techniques associated with imperfect competition, which had become "almost the dominant concern of economic theorists ... a distracting fad".^{xx} Monopolistic competition lent itself to one branch of the

formalist revolution (mathematical investigations), and with the advance of computing power, Friedman successfully integrated the quantity theory into the other branch (econometrics). But monopolistic competition was denied the momentum that would have resulted from Stigler and Friedman accepting the challenge offered by Chamberlin and Archibald.

Stigler warned that "intellectual and political influences are extraordinarily subtle and difficult to trace or measure"; "all short answers are wrong".^{xxi} He (1988c, 9) concluded that Keynes sought to be, not a dentist, but "a brain surgeon who operated on ideologies"; and this analogy could be applied to Stigler and Friedman who sought to influence the process of knowledge construction and destruction. This essay examines the Chicago counter-revolution as a product of their "superior understanding" not so much of the behaviour of economic variables, but of the sociology of professional economic knowledge: "our ratio of objective scientific work to policy rationalisation is high only in comparison to other social sciences"; "We wish to be scientists ... We wish also to be important ... A curious tension" (Stigler 1975, 316-7; 1976a, 352-3). Friedman and Stigler spent time discussing the nature of influence and how opinions were to be changed (Stigler (1980, 353; 1988c, 11; McCloskey 1994, 341-2).^{xxii} This paper assumes that they behaved *as if* they were self-conscious counter-revolutionaries.

3. Stigler's 'model' of the sociology of economic knowledge

3.1 Theories are trumps

Stigler (1957b, 9; 1954b, 103) noted that economists have a tendency to "float on the tide of theory". He reflected that empirical generalisations "fail to achieve the continuity and the widespread influence of the formal theories".^{xxiii} He was also aware that for theorists, statistically derived relationships could be "frankensteins over which he has little or no control". Galbraith, for example, looked at the same empirical studies as himself but derived opposite inferences (Stigler 1939, 470; 1949a, 96).

With respect to conclusions that contained policy implications, Stigler (1951, 127-8) sought to elevate received theory over empirical analysis.^{xxiv} The reason for this confidence in orthodoxy was that it was "our *most tested* and reliable instrument for relating policies to effects" [emphasis added] (1964a, 421). Received theory, presumably, operated with a considerable advantage.^{xxv} The idea that a new theory "is presumed innocent until shown guilty ... is the exact opposite of the presumption I would use" (1982b, 204; 1978, 191).

Not all theorists were to be trusted;^{xxvi} unorthodox approaches were sometimes denied the label 'theory'. Galbraith's notion of countervailing power was not a theory but a "dogma" and rested on "allegations" of economic life; in the 1930s it would "have attracted no one". Galbraith's policy implications should be "left untouched" because to quarrel with those implications would

imply "that the theory had reached the stage of relevance to policy ... Galbraith cannot persuade us that we should turn our economic problems over to Santa" (1954a, 9-10, 14). Likewise, monopolistic competition had "failed to initiate a fundamentally new direction of economic theorising" (1988b, 93).

3.2 The crucial importance of internal developments

Stigler criticised the attempt to explain how economic ideas gained currency by reference to environmental factors; objecting to Wesley Mitchell's downgrading of "intellectual stunt[s] [to] a secondary rather than a primary force". Stigler concluded that the "dominant influence upon the working range of economic theorists is the set of internal values and pressures of the discipline. The subjects for study are posed by the unfolding course of scientific developments" (1965b [1960], 18, 22, 27-8; see also Sowell 1987, 498). Friedman found Mitchell's course on the history of economic thought "dull" and his view that "theories had no life of their own" to be "in sharp contrast to the history of thought as developed many years later by George Stigler". Friedman shared Stigler's views about the importance of "the internal logic of the subject ... The contrast between Chicago and Columbia was sharp" (Friedman and Friedman 1998, 44).

3.3 The elite and the masses

It was Stigler's ability to set the agenda (positively and negatively) which underpinned his influence among economists (McCann and Perlman 1993, 997); he believed that any assessment of a scholar's achievements should include "not only what he wrought but also what he prevented" (Stigler 1990a, 12). Stigler (1969c, 227; 1951, 126; 1982a, 60) clearly distinguished between "major scientific entrepreneurs" and the rest, some of whom could only employ "an inferior mind", and some of whom were "*ersatz* economists". They entered the market as demanders, not suppliers, of ideas; and conference participants reminded Stigler (1977, 441) of travelling salesman exchanging stale jokes. He believed that economists were analogous to the purchasers of second hand automobiles and he wondered why some ideas "wouldn't run far or carry many passengers" (1986 [1982], 134-5, [1979], 340). Part of the explanation involved a quality differential: "which socialist propagandist has been as logically lucid as Friedman?"

Stigler (1978, 201) concluded in his study of 'The Literature of Economics' that two thirds of the articles surveyed were virtually worthless.^{xxvii} There were commonly only about six really first class scholars in any field (1963b, 37); a small minority in the profession had "superb instincts" with regard to the pursuit of ideas. The 2 or 3 percent of the profession who were "active and ambitious" were also "reform[ers] of economic science" (1988d, 95). Occasionally, "economists-missionaries" successfully ventured into the territory of "apprehensive and hostile natives" (1984, 304); but academic consensus (which could be unreliable) was achieved not by a professional "plebiscite", but only by an elite group within the profession (1991, 44; 1985, 1).

Science was *defined* as the consensus interpretations that emerged from this process.^{xxviii} In contrast, for the "mass" of scholars in any discipline, risk aversion, and a desire to preserve already acquired human capital, created a bias in favour of scientific conservatism.

Given this structure of the sociology of economic knowledge, the "most irresistible" of all the weapons of scholarship was "infinite repetition"; a "form of the classical Chinese torture" (1965b, 4; 1984, 311; 1986 [1979], 339). Stigler (1965b [1959], 286) was impressed by the Fabian Society's "effectiveness in shifting opinion. If enough able and determined men - and the number in the Fabian group was almost unbelievably small - denounce and denounce again a deficiency, that deficiency becomes grave".

3.4 The technique of the huckster

Stigler displayed an understanding of "the technique of the huckster" by which change is effected amongst economists. He believed that intellectuals suffered from "romantic wishfulness" in their understanding about influence; a Darwinian "survivor technique" was useful: "if I wish to know whether a tiger or a panther is the stronger animal, I put them in the same cage and return after a few hours"; "Great economists are those who influence the profession as a whole, and this they can do if their doctrines do not involve too great a change from the views and the knowledge of the rank and file of the science. It is simply impossible for men to apprehend and to adopt wholly unfamiliar ideas ... New ideas are even harder to sell than new products ... One must put on the best face possible and much is possible. Wares must be shouted - the human mind is not a divining rod that quivers over truth. The techniques of persuasion also in the realm of ideas are generally repetition, inflated claims, and disproportionate emphases, and they have preceded and accompanied the adoption on a large scale of almost every new idea in economic theory ... It is possible by mere skill of presentation to create a fad, but a deep and lasting impression on the science will be achieved only if the idea meets the more durable standards of the science. Among these standards is truth, but of course it is not the only one" (Stigler 1975, 318-9; 1988b, 99, 108; 1968a, 72-4; 1955b, 294-6).^{xxix} The price mechanism operated here, as elsewhere: "It is perfectly reasonable for individuals to purchase intellectual integrity as well as meat and wine - always provided that the price is not too high" (Stigler 1988e, 8).

Stigler bemoaned the "attitudes of professional economists ... when economists agree that a movement is inevitable, it is not" (1950a, 30, 34). He argued that the knowledge production industry was a competitive industry in which "confused" participants would "watch their customers vanish, their best employees migrate, their assets dissipate". A monopolist was "overwhelmingly dominated by forces over which he has negligible control" (1963b, 42).

Stigler clearly had an ability to provoke his audience (Samuelson 1962, 9),^{xxx} as illustrated by his Harvard lecture, 'The Politics of Political Economists', on the "mysterious intellectual osmosis" that economists are subjected to. Radicals, he argued, tended to be anti-empirical, but scientifically trained economists tended to be politically conservative because of their exposure to price theory: "We shall no doubt continue to bend before a strong [anti-conservative] wind, but I consider it a remarkable effect of our professional discipline that we shall not be contributing to the wind" (1959a, 528-5).^{xxxii}

Later, Stigler (1965b, 48-9) pondered about the role of "fashion" in economics and the "truly remarkable" process by which Keynesian economics diverted attention away from more traditional research agendas. He then echoed his Harvard sentiments: economics had become highly respectable but was "lacking in promise in basic influence on policy in the future. I do not know whether it is an occasion for pride or for regret that the economist is using Marquis of Queensbury arguments in an arena where emotional brass knuckles continue in fashion".

Stigler was impatient with alternative perspectives, and from the late 1970s he "governed" the ethics of conversation at Chicago. Criticisms of his work, even those which appeared in major journals, often did not command his attention or respect,^{xxxiii} and according to one of his Chicago colleagues, exchanges with Stigler were "likely to be terminated by a positivist edict and a sneer" (McCloskey 1994, 14).

3.5 *The fox hunts of controversy*

Stigler (1978, 200-201, 185) made numerous other perceptive comments about the nature of controversy in economics: "if controversy is active almost every proposition seems open to debate, and the course of controversy shifts as rapidly as the situs of a fox hunt - indeed, a series of simultaneous and intersecting fox hunts". The fundamentally pervasive concept of competition had long been treated with "the kind of casualness with which one treats of the intuitively obvious". But Frank Knight's *Risk, Uncertainty and Profit*, with its comprehensive and "meticulous" discussion of perfect competition was counterproductive for the Chicago School: it "did most to drive home to economists generally the austere nature of the rigorously defined concept and so prepared the way for the widespread reaction against it in the 1930s". Stigler (1956a, 270) reflected on Alfred Marshall's opposition to this ongoing attempt to attach "more restrictive" conditions to the concept of competition. According to Stigler, Marshall's opposition was motivated by sociology of knowledge considerations: he sought to avoid introducing complications that might lessen the appeal of competition ("He wished to retain for competition its traditional claim as the great engine of progress ...").

The "more persuasive" defence of the concept of perfect competition is that it has "defeated its

newer rivals in the decisive area: the day-to-day work of the economic theorist" (1957b, 1, 11, 17). Price theory was scientifically more mature, and therefore stronger, than macroeconomics because of the homogeneity of doctoral citation practices - with the exception of the "exclusive appeal of monopolistic competition to Harvard" (Stigler and Friedland 1975, 503; 1979). "Scientific literature is to a considerable degree controversial literature. New ideas are sold very much the way new automobiles are sold: by exaggerating their superiority over the older models ... Scientific innovation proceeds more by disparagement of rivals than by excessive self-praise, perhaps because it appears more modest. The role of controversy is indeed to stimulate interest and animosity ... The sterility of the early Walrasian system arose because it was ignored by most economists and adopted by a few but criticised by almost none. Milton Friedman's work was bound to be spread rapidly in the science and to achieve a wide scope and high rigour because of his wondrous gift of eliciting the probing attention of eminent contemporaries" (1969c, 222). Friedman was "a masterful peddler ... intellectuals sell their wares to customers, and they - not the customers - do most of the adapting" (1975, 321, 315).^{xxxiii}

3.6 Neglect - the highway to oblivion

Chicago has inscribed on the front of its Social Science Research Building an approximation to Kelvin's dictum "When you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind" (McCloskey 1986, 7; Stigler 1988c, 12). Knight (1960, 166, n18) thought that this statement "very largely means in practice, If you cannot measure, measure anyhow", and Viner "rejoined that our knowledge after measurement would still be meagre and unsatisfactory" (cited by Stigler 1982c, 23). Stigler (1992, 462) welcomed "formidable" empirical challenges to economists: "Ease of solution ... is not the best guide in the selection of agenda for scientific research".

Stigler (1959a, 529) and Friedman were prepared to find proxies for all sorts of variables, believing that the empirical economics was the only way to obtain a reliable feel for the way an economic system functioned. The literature of the public utility specialists was defective because it had failed to measure the effects of regulation; but Stigler and Friedland (1962, 3, 11) were prepared to ask "impertinent" empirical questions to remedy this defect. Stigler (1965d, 62-3, 68, 57) reflected on the "truly remarkable ... degree of indolence or lack of curiosity" which economists had exhibited by not empirically examining the regulatory experience. Economists had "found imperfections, because this is an imperfect world", and they had advocated state intervention without investigating the consequences of intervention. Asking new empirical questions could lead to "very striking" results. Economists had neglected the questions that Stigler had formulated: they had "not studied the past". This neglect had led to policy consequences that relied upon an exaggerated respect for the competence of the state and a diminished faith in market solutions.

But Stigler and Friedman declined to provide fuel for the locomotive of the monopolistic competition revolution of which they disapproved. This decision may have been influenced by Stigler's sociological perceptions: "Once an idea is widely accepted, it is guaranteed a measure of immortality. Its decline in popularity is more often due to changing interests than to contrary evidence, no matter how powerful that evidence may be ... Even to be demolished is better for one's self-esteem than to be ignored: It requires some ability to excite and especially to outrage one's fellow professionals ... Neglect is the highway to oblivion" (Stigler 1988a, 67, 75, 112, 157, 159, 97, 162, 166, 213, 216; Stigler and Kindahl 1973, 721, n6). Stigler's assault on the Giffen paradox was designed to push the discussion of it "deeper into footnotes" (1947b, 156); to become boring was a preliminary to the dissipation of professional interest (1962b, 1).^{xxxiv}

Stigler (1978, 200-1) also reflected that the textbooks of a discipline "play a powerfully conservative role in the transmission of doctrine ... adverse empirical evidence is not a decisive factor ... to understand the rate of decline of a theory". In 1942, Stigler published half a textbook (*The Theory of Competitive Price*); and four years later he published the full version including material on monopolistic competition (*The Theory of Price*). This text was enormously influential; later imitators tended to be both sequence and content-takers. Just as the "beginning student of physics does not demur at the absence of friction", so the student of economics is exposed first of all to perfect competition: "competition is a better single assumption, even on the basis of realism, than monopoly" (Stigler 1946, 23). According to Chamberlin (1946, 416), early reviewers objected that the "natural and easy way" would have been to begin with the individual firm and monopolistic competition. Stigler was prompted to adopt this sequence because to reverse it would have meant "not merely a reversal of order, but a revolution in the author's thinking". From a pedagogical perspective, perfect competition and monopoly possess a simplicity and an accepted theoretical framework that cannot be found in the later chapters on oligopoly and monopolistic competition.

Stigler (1949a, 104) was alarmed about the nature of knowledge construction and destruction in economics: "ours has become a flabby science".^{xxxv} Means' theory of administered pricing had at first incited a lot of controversy and empirical analysis; later it displayed "growing anaemia; it is fair to say that economists abandoned the close study of the subject, less because its lack of scientific import was established than because it became boring" (1968a [1962], 235). The ideas of those who found a clash between traditional price theory and observed behaviour often had a "Phoenix property"; but "It takes a locomotive of sorts to keep an idea moving in science"; "The reworking of predecessors' ideas will seldom lead to very fresh work, and after a while this source seems to become boring - at least these economists tend to leave economic theory after a while"; "A concept without enemies ... is also a concept without informed friends" (1982a, 52;

1951, 126).^{xxxvi}

4. The Emergence of the Chicago School of Anti Monopolistic Competition

Paul Samuelson (1967, 108, n5, 113, 116, 138), reflecting on "Chamberlin's imperishable vision", predicted that "reality will falsify *many* of the important qualitative and quantitative *predictions* of the competitive model ... Chamberlin, Sraffa, Robinson and their contemporaries have led economists into a new land from which their critics will never evict us ... Chicago economists can shout until they are blue in the face that there is no elegant alternative to the theory of perfect competition ... it is significant that Marshall's remaining defenders among theorists tend to be those satisfied with perfect competition as an approximation to reality" [emphases in text].

In contrast, Stigler (1955b, 301) described the 1930s as exhibiting "an excess of originality". His thesis supervisor, Frank Knight, stated that "If there is anything I can't stand it's a Keynesian and a believer in monopolistic competition" (Samuelson 1983, 7).^{xxxvii} But Friedman and Stigler did not share Knight's fatalistic despair about the Chicago project for social and economic transformation;^{xxxviii} neither did they share his lack of interest in empirical or policy-orientated economics or his belief in the inherent contradiction between thought and action (Stigler 1982a, 167; Buchanan 1991; Kern 1987, 640; Patinkin 1973, 796, 804-5). Both attended his seminars on the sociologist Max Weber; he was their dominant dinner table subject of conversation (Wallis 1993, 775). According to Shils, Knight "offered a great deal to a sociologist" (1981, 181, 184; see also Schweitzer 1975). This sociological perceptiveness involved doubt about the outcomes of rational debate: "Frank Knight's First Law of Talk" was that "Cheaper talk drives out of circulation that which is less cheap" (Knight, cited by Patinkin 1973, 807).

In the 1940s, what later became known as the Chicago School faced danger on two fronts. With the loss of Paul Douglas and Henry Schultz, and with Knight's diminished interest in economics, there was a shortage of intellectual leaders. There was also a struggle for academic dominance and institutional control at Chicago between the Friedman group and the Cowles Commission; a struggle that was resolved in 1955 with the departure of Cowles to Yale (Reder 1982, 4; 1987, 415). "The Chicago School of Anti-Monopolistic Competition" was first explicitly defined and described by Chamberlin (1957, 296); only then did economists begin to refer to Chicago as a School (Stigler 1988a, 150).^{xxxix} The modern Chicago School began to take shape; culminating in the famous 1960 'Coase versus Pigou' evening at Aaron Director's house which was "the most exciting intellectual event" of Stigler's life. The evening ended up with "no votes for Pigou" and effectively partitioned economics into two epochs: A.C. and B.C. ("Before Coase"). According to Stigler (1983, 221; 1988a, 75, 148-169; 1992, 456; 1972b, 11)

the previous epoch had confused "all economists ... from at least 1890 until 1961".^{xi} The realisation that Pigou had previously obtained such a "hold", even in places like Chicago, generated both enthusiasm and sociological reflectiveness. It also laid out a detailed research agenda for the Chicago School and the *Journal of Law and Economics* (Coase 1995, 242-4).^{xli} The potency of the Coase conversion evening may have been intensified for Stigler given the 'mistakenly' Pigovian role that Coase allocated to the second edition of his *Theory of Price* (1952).^{xlii}

Knight had not provided a Chicago agenda of research (Stigler 1973b, 520); only in the late 1950s was Friedman able to fully engage his Keynesian opponents. This was a fight - undertaken with strategic considerations in mind - against the "conditioned reflex[es]" of "entrenched Keynesianism" (Friedman 1968, 5, n2). In 1958, Friedman was joined at Chicago by Stigler, who, intellectually, became "more vertically integrated" (Rosen 1993, 814). Lester Telser concluded that "Perhaps what [Stigler] learnt from Friedman was focus".^{xliii} This enhanced focus and the connecting up of the various components of his expertise (microeconomics, controversy, and sociology of economic knowledge) produced an extraordinarily creative phase, for which he would later be awarded a Nobel Prize (Stigler 1961b, 1962a). Harry Johnson's arrival in 1959 also strengthened the Chicago School, which was, according to Bhagwati (1977, 225-6) at the time "very Friedmanesque ... The seminars seemed to oscillate between proving that elasticities were large with markets therefore stable, and formulating competitive hypotheses for apparently imperfectly-competitive industries and coming up with high enough R^2 s. Econometrics was the handmaiden of ideology: things looked imperfect to the naked eye, especially to that of Chamberlin and Joan Robinson, but they were 'really' not so and the world was 'as if' competitive ... market imperfections were 'demonstrated' to be negligible and the imperfections rather of government intervention were the subject of active research".^{xliv}

In terms of *microeconomic* controversy, both Cambridges had powerful alternative theories to the Chicago belief that consumers direct the production process (Stigler 1976a, 347). Chamberlin (1933) subtitled his book *A Reorientation of the Theory of Value*, and he devoted his career to the proposition that the firm is the essential structural unit of the economy (1957, 44). Joan Robinson (1933, v; 1934, 105) attempted to develop Sraffa's "pregnant suggestion" that the whole theory of value should be reconstructed around the concept of monopoly, thus "emancipating economic analysis from the tyranny of the assumption of perfect competition". These were self-consciously revolutionary attempts to construct a general parameter theory (Schneider 1967, 144), which could be combined with general equilibrium models (Chamberlin 1957, 68; Jaffe 1934, 27; Triffin 1940). Together with Keynes' work, the monopolistic competition revolution captivated the economics profession both between the wars and in the

immediate post-war period (Keynes JMK XXIX [1938], 175-6; Nichol 1934; Galbraith 1948; Bain 1948; Bishop 1964). In the Preface to his seventh edition, Chamberlin (1956) noted that his bibliographic supplement had expanded by 806 titles in the eight years since the sixth edition. This was more than the entire period prior to 1948: "important evidence" that monopolistic competition was acquiring the mantle of generality.

Traditional economic theory had created a presumption against social interference in the market place; but imperfect competition questioned the generally accepted conclusions of economic theory (Morrison 1934, 30; Harrod 1934, 463; J. Robinson 1952, 925-6). Although Chamberlin was personally quite conservative, monopolistic competition was associated with the weakening of competitive ideology, and its supporters tended to take their stand on a variety of positions that were antithetical to Chicago economics (Fellner 1967, 12).^{xlv} The immediate post-war period had witnessed "frequent and heavy bombardment[s]" on monopolistic competition from Chicago (Markham 1964, 53). These tactics were perceived to have been counter-productive to the Chicago cause: "It must be emphasised here that even the writings of those who would minimise the contribution of monopolistic competition, by their very number and vociferousness combine to constitute an encomiast, as it were, praising by strong (and frequent) damns" (Baumol 1964, 44).

Economic theory had long accepted the coexistence of perfect and imperfect competition, and Stigler (1948, 915) concluded that the latter was "a minor variant" from competition. According to Arnold Harberger, the behaviour of the U.S. manufacturing sector could be accurately analysed through the model of perfect competition; the per capita welfare losses associated with monopoly amounted to no more than \$2 (1954; see also Friedman 1965, 55). But Machlup (1939, 231, 236) concluded that the "increased dose of realism that was won for economic theory" by monopolistic competition had led economists to *emphasise* product differentiation and the frequent occurrence of oligopoly. The new theories had "clear practical value" for policy makers.^{xlvi} These microeconomic disputes were ferociously debated: one of the discussants of the Galbraith-Stigler AEA session on 'Fundamental Characteristics of the American Economy' concluded that the author of the notion of countervailing power was "one of the most effective enemies of both capitalism and democracy" (McCord Wright 1954, 30).

The cultures of Chicago and the two Cambridges appear to leave an indelible imprint on their respective students, and on the subject matter of their doctoral dissertations (*American Economic Review* 1960, 864-91). Colander and Klammer's (1987) survey revealed that only 6 per cent of Chicago students thought that price rigidities were very important (compared to 38 per cent at Harvard); and 84 per cent of Chicago students (but only 7 per cent of MIT students) agreed that inflation was primarily a monetary phenomenon.^{xlvii} Stigler concluded that

Friedman's "persistent and skilful use of price theory in dealing with economic questions ... was thoroughly drilled into graduate students" at Chicago (cited by Rose Friedman 1977, 24). Stigler (1969c, 221) also complained of the "block to proper reading" that MIT students exhibited towards Friedman's work; and a student trained using either the 1966 edition of Stigler's *The Theory of Price*, or Friedman's *Price Theory* (1962) would remain unaware of the monopolistic competition revolution, with the exception of a footnote referring to Joan Robinson and the effect of price discrimination on output (Stigler 1966, 214, n12).^{xlviii} Friedman and Stigler concluded that monopolistic competition "possesses none of the attributes that would make it a truly useful general theory ... It is therefore incompetent to deal with a host of important problems". Its contribution has been limited to "enriching the vocabulary available for describing industrial experience" (Friedman 1953, 38-9); of which "something like 98 per cent of the general [industrial organisation] literature is concerned, explicitly or implicitly with the question of monopoly" (Stigler 1959a, 530). Monopolistic competition appears only in a single paragraph (plus the Appendix) in *The Organisation of Industry*; a paragraph which concludes that "It has not been found useful in the analysis of concrete economic problems" (Stigler 1968a, 13; in contrast, see Scherer 1970).

Stigler's (1937, 707-8, 710, 713) second appearance in the professional literature, however, was an essay on 'A Generalisation of the Theory of Imperfect Competition' in which he suggested a framework for research in the "imperfections of competition in agriculture". The essay examined the realism of the assumptions of perfect competition, concluding that one could "add much realism" by eliminating some of these assumptions; the non-economic motives for land ownership by farmers "seem exceptionally amenable to empirical investigation".

5. Chamberlin and Archibald versus Chicago

5.1 Chamberlin's Challenge

The Theory of Monopolistic Competition began as a doctoral dissertation (submitted in 1927) and went through eight editions (1933-1962). According to Stigler (1988a, 96), Chamberlin (1899-1967) was "possessed" by his theory.^{xlix} Alfred Sherrard (1951, 126, 142) thought that Chamberlin's contribution had so far proven more "illusory" than revolutionary: "Another wing, or perhaps a porch, has been added to economic theory; but the structure retains its original form ... But as deliberate product differentiation, advertising and salesmanship take the centre of the stage, economic theory in the traditional sense must depart. A revolution in analysis is called for - a new set of questions, a new philosophical foundation". Wallis (1949, 559) suggested that monopolistic competition theories should be tested to see how well they could "organise, predict or rationalise observed advertising behaviour".

Chamberlin devoted less of his career to the empirical investigation of his favoured market

structure than to his acute sense of product differentiation; insisting that every Harvard PhD. candidate understood the precise differences between imperfect competition and monopolistic competition (Johnson and Johnson 1978, 153; see, for example, Chamberlin 1937).¹ But in *Towards a More General Theory of Value*, Chamberlin made a case for the revolutionary generality of his approach, citing Tinbergen's explanation for the unsatisfactory response of economists: "there is a lag of measurement behind theory, which forces the users of applied economic theory to stick to the older and simpler theories" (Tinbergen cited by Chamberlin 1957, 9, n10). The "right wing orthodoxy" of the Chicago School who "cling desperately to perfect competition" could offer only a "jumble of reasons ... a cloud of dust" to defend the status quo: "mere tricks to bolster up what is at bottom an emotional position ... surely better sticks than this could be found ... people who live in *ad hoc* houses should be more indulgent" (1957, 13-5, 17, 26, 300, 305). Chamberlin sought to overcome this "heavy" legacy by reformulating his theory to assist the process of measurement (1957, 24, 43, 70-91; 1957 [1948], 226-249). He (1957, 305-6) concluded that his hypothesis had come into existence following the 'classical' scientific process outlined in 'The Methodology of Positive Economics'. He had edited the proceeding of an International Economic Association conference on *Monopoly and Competition* (1954b), contributing an essay on the difficulties of 'Measuring the Degree of Monopoly and Competition', which included a suggested classification of the concept of monopolistic competition (1954a, 265).

5.2 Stigler and the importance of testing theories

Stigler (1963c, 109) concluded that economics would have "suffered grievously" had he been research 'dictator' in the 1940s.^{li} Chamberlin (1957, 140, 299) also took an interest in this evolution of Stigler's position. As a graduate student at Chicago, Stigler organised Saturday morning seminars on monopolistic competition in the belief that Chamberlin's work would revolutionise economics (Wallis 1993). About one third of the first edition of Stigler's *The Theory of Price*^{liii} was devoted to a discussion of monopolistic competition (1946, v, 195-302, 312-4).^{liii} Referring to "dissatisfaction with the neoclassical theory of competition", Stigler complained that economics was in an "unsatisfactory state". There were, he argued, two obstacles before perfect competition could render to monopolistic competition the mantle of generality. The first was variety, that is, the infinite number of conceivable departures from perfect competition. But this was not insurmountable: "it would usually be possible to establish sensible classifications which would reduce the problem to much more manageable proportions, and in fact this has been done in good part". The second was "ignorance ... primarily an ignorance of relevant assumptions ... the more urgent need is for factual knowledge. Until this knowledge becomes available - and this is a slow, accretionary process - the detailed content of the theory of imperfect competition will remain in large degree intuitive" (1946, 197-8, v). Stigler (1944, 81) objected to the "formalism" associated with the theory: "Real progress in the

theory now requires a shift in emphasis from mathematical analysis ... to detailed studies of individual market structures".

Friedman dominated the thoughts and practices of the profession as a methodologist, several years before his thoughts on money attracted similar attention. For a decade he was closely involved with methodology, planning at one stage to write a book on the subject. In November 1947 he informed Stigler that "I have gotten involved for various irrelevant reasons in a number of discussions of scientific methodology related to the kind of thing you are talking about"; this interest apparently waned after 1957 (Hammond 1991b, 3, 34; Stigler 1994). His methodology of positive economics was formulated as a "reaction" to Robinson and Chamberlin, and a "reply" to a series of empirical studies which questioned the relevance of orthodox price theory (Moss 1984; Friedman 1993, 770).^{liv} Keynesian economics and monopolistic competition share a common concern for the 'accuracy' of assumptions; the early drafts of his methodological essays reveal Friedman's interest in rebutting the assault on microeconomic orthodoxy.^{lv}

Stigler provided one of the first written expositions of this methodology of positive economics in a lecture at LSE on 'Monopolistic Competition in Retrospect',^{lvi} concluding that the dispute between monopolistic competition and the theory of competition was "a question of fact, and must be resolved by empirical tests of the implications of the two theories (*a task the supporters of the theory of monopolistic competition have not yet undertaken*) [emphasis added]" (1949b, 22-5, 13, 45; 1949a, 102-4). Stigler (1955c, 13-14) stated that the dispute between full cost pricing and imperfect competition must "first be settled ... presumably by recourse to empirical tests".

In the preface to the second edition of his textbook, Stigler (1952, v) sought to "emphasize ... how the theory is tested by evidence" and to "incite the student to attempt to test parts of the theory for himself ... I can assure him that some parts of the theory in this book are unquestionably wrong - and I should like to know which they are". Stigler (1955a, 300) also stated that "One can test empirically a crude theory as well as a refined one". In a scathing article, entitled 'Alice in Fundland', Stigler (1967b, 12) ridiculed the "Untrained Minds" of the Securities and Exchange Commission for lacking "the humility to subject its policies to empirical test". He commended the ongoing examination of "untested hypotheses of which we have too many" (1959b, viii). Later, in his Nobel Lecture (1986 [1982], 147), he reflected that such explorations were sometimes little more than tests of the "intellectual fertility" of those doing the testing. In a lecture at Chicago, he also reflected on the difficulties of achieving "objective discussion" about empirical work which stirred up strong emotional or moral implications; and he had been previously disappointed with the professional response to his tests of theories of which he disapproved (1984, 306). In 1947, he "tested" Sweezy's demand

theory;^{lvii} but despite his adverse findings, for four decades the kinked demand curve continued to attract the attention of a large proportion of prominent economists (Stigler 1978; 1982b).

Joan Robinson (1952, 925-6) reflected on the lineage of Galbraith's *American Capitalism: The Concept of Countervailing Power*: "imperfect competition was essentially a debunking movement" which left the defenders of orthodoxy "in an exposed position". Her primary motive for writing about imperfect competition was to demonstrate that wages were not equal to the value of the marginal product of labour and that therefore labour was exploited under capitalism (Robinson 1969, xii; Bishop 1964, 37). She was also curious about how Galbraith's audience (different from her own) would respond. Galbraith (1954, 2) painted an unattractive picture of the "men who resist any tampering with the rigidly idealised world of our ancestors ... the self-designated protectors of our political morality".^{lviii} Galbraith emphasised the importance of accurate assumptions and offered the prospect of reformulating economics along unorthodox lines.^{lix}

Sympathetic reviewers found in Galbraith's book an invigorating questioning of "classic premises"; he had "certainly opened a door which will not be closed for a very long time" (Berle 1953, 81, 84). The methodology of positive economics emphasised the output, not the input side of economic analysis, and in a famous attempt at door closing, Stigler (1954a, 12) "tested" Galbraith's notion of countervailing power.^{lx} He concluded that the tests were "not flattering to the dogma"; but Galbraith's star continued to rise. Indeed, Galbraith was given an additional platform by the 1959 Kefauver Senate *Hearings on Administered Prices*,^{lxi} and immediately prior to Archibald's challenge, Rudledge Vining, writing in the *American Economic Review*, reported the response that Joan Robinson was curious about: Galbraith's work had "extraordinary public appeal". Vining (a non-econometric economic statistician) described Galbraith's work as outlining an agenda for statisticians: "a call to competent students and analysts to direct their attention to the essential descriptive and analytical tasks" (1959, 119).

Stigler (1949b, 23) noted that "the discrepancies between pronouncements and practice are notorious in the field of methodology". But with respect to the testing of hypotheses, "we can be reasonably certain of what is right practice ... Occasionally we lack the data necessary to carry out the tests but lack the slight imagination necessary to rework the theory so it bears on available information and the enormous energy necessary to ferret out the information ... I do not wish to condone our impatience with laborious research, and I hope it will soon disappear" (1951, 126-7). Stigler (1943, 532) welcomed Simon Kuznets' pioneering contribution to the "new and ... difficult" field of national income measurement.^{lxii} He also attempted to test aspects of public choice theory related to size of legislatures, whilst admitting that his empirical analysis was "long on problems and short on solutions" (Stigler 1976b, 31).

Qualitative conclusions from the theory of consumer behaviour needed to "tested empirically". Although the bridge between empirical demand curves and their theoretical counterparts would never be bridged, fruitful progress could still be made by statistically estimating those curves (1939, 472, 476, 481). During his stay at the NBER, Stigler completed three major empirical studies of *Domestic Servants in the United States* (1947c), *Trends in Output and Employment* (1947d) and *Employment and Compensation in Education* (1950b). His own extensive empirical work led him to believe that "the classical theory of the market has direct applicability to empirical market determinations ... the applications are manageable in both their data requirements and their methodology" (Stigler and Sherwin 1985, 584).

For Stigler (1949b, 54) the purpose of economics was to generate predictions: "The sole test of a usefulness of an economic theory is the concordance between its predictions and the observable course of events". He argued that there was never likely to be any evidence for the thesis that competition has steadily declined. It was therefore "an obstacle to clear thinking on social policy" which needed to be abandoned.

Stigler and Friedland (1983, 258) concluded that the Berle and Means assault on the assumptions of traditional analysis received an "astonishingly uncritical" reception; but traditional analysts continued to work in "complete disregard of *The Modern Corporation*". One explanation for the apparent absence of any discernible influence upon traditional economic theory may lie in the observation that "the practice of testing economic theories was still extremely uncommon in the 1930s. Certainly no one dreamt of subjecting Chamberlin's *Monopolistic Competition* or Keynes's *Treatise* to a formal empirical test". A series of tests had, however, been proposed by Chamberlin (1957), Archibald (1961) and Bishop (1964, 41-3). But the Chicago opponents of monopolistic competition declined to be drawn into the testing procedures.

5.3 Archibald's Challenge and Chicago's Response

The LSE Methodology, Measurement and Testing (M²T) Staff Seminar Series not only 'tested' the Phillips Curve, but sought to 'test' *all* economic theory. Invoking the names of both Popper and Friedman (they could have added Stigler), the M²T prospectus (1957) stated that "we agree with Friedman (although we doubt if he accepts the basic Popperian position that you advance knowledge through refutation, and must therefore *try* to refute)" [emphasis in text] (see also Klappholz and Agassi 1959, 67-8). Their faith in the potency of econometric estimation and the methodology of positive economics was matched only by Friedman's and Stigler's faith in the potency of perfect competition.

Popper did not participate in the M²T meetings, but his student, Josef Agassi, did. Agassi (perceived by the M²T group to be a 'proxy' for Popper's views) and Kurt Klappholtz castigated Friedman's dismissal of monopolistic competition; his criticisms rested mainly on "impatient methodology and is conservative and discouraging rather than stimulating". If the theory was untestable at that time then Friedman should assist in the process of making it testable (Klappholtz and Agassi 1959, 66-9). Archibald (1959a, 61-2) doubted if the theory of the firm had ever been tested. Friedman, he argued, had not followed the guidelines of his own "revolutionary" methodology: "Is it not precisely those hypotheses which are wrapped in the cotton wool and authority of 'tradition and folklore' that most urgently require testing ... [Friedman's] purport is to encourage complacency and to discourage that sceptical re-examination of the allegedly obvious that is the prerequisite of progress".

Five months of weekly M²T meetings (February, March 1959, January, October, November 1961) were taken up on the theme of "Testing Some Theories of the Firm". The minutes record contributions from an impressive array of young economists, including Richard Lipsey and Kelvin Lancaster who had recently (1956) made a provocative contribution to welfare economics in the form of 'the General Theory of Second Best' which had severely weakened (if not completely undermined) the welfare presumption in favour of perfect competition (but see Friedman 1965, 51).^{lxiii} Also present were Chris Archibald, Maurice Peston, Charles Holt, Bernard Corry, Lucian Foldes, Thomas Meyer, Josef Agassi, George Borts, Edward Mishan and Richard Quandt. Archibald (18, 22 February, 4 March 1959) was searching for generality: "Our analysis applies to all cases". He objected to the Hall and Hitch Oxford studies on the grounds that "Their methodology is bad. They are not open to independent refutation"; and to Baumol's contribution to the theory of the firm on the grounds that "His data is personal to him". Agassi retorted: "Then we can ignore his data". Archibald (1960, 210, 213) dissected Jerome Stein's essay on 'The Predictive Accuracy of the Marginal Productivity Theory of Wages' and found it to "lack discriminatory power". There was, therefore, "not much cause for congratulation or confidence" in the marginal productivity approach. Archibald then proposed a test of the marginal productivity approach that would be "the same for perfect and imperfect competition".

The M²T economists were evangelical about reconstructing economic theory so as to make it *all* econometrically testable: "There is a small supply of hypotheses and they are probably all wrong ... The hypothesis must be improved to make sense of the data" (Archibald, 11 February, 4 March 1959). This process of creative theoretical destruction appears to have involved a deliberate attempt to be fair to those theories that appeared to have been refuted. Marginal productivity, for example, although apparently refuted, could be defended by alibis: "it seems it might be worth trying to check these alibis" (Archibald 1960, 213). Their evangelical zeal to

generate testable implications was fuelled by Friedman's methodology of positive economics; they also sought to apply this method to areas in which Friedman had made seminal contributions.^{lxiv}

On 25 January 1961, the M²T economists met to discuss Archibald's paper on "Chamberlin versus Chicago". Borts opened the discussion by asking: "Archibald's objection is that the competitive model is false ... What form will new general theory take?" Lipsey asked "What set of observations would refute Chicago?"^{lxv} Archibald (1961, 21, 2-5, 7) complained that there was a glaring inconsistency between the Chicago tendency to "cling" to perfect competition and the "'new' or 'Chicago' methodology". When it came to monopolistic competition, the Chicago economists were engineers, not scientists, whose constructions had led them to neglect the "enquiring, scientific question[s]". Archibald outlined an *agenda* for a wide reaching theoretical and empirical investigation: "we require more facts, not for their own sake, but in order to put into the theory sufficient content for it to yield significant predictions ... the new methodology provides a solution to our difficulties". Archibald was *not an advocate* of the theory of monopolistic competition, which he found (at the stage it had then reached) to be disappointingly empty.^{lxvi}

Stigler (1962e, xvi) defined the "fundamental characteristic" of scholarship as "the discovery of relevant questions [rather] than the giving of conclusive answers"; and Archibald was clearly attempting to *engage* the opponents of monopolistic competition by appealing to their own method of research: "this justification should commend itself to Chicago methodologists". Stigler (1983b, 416) wrote of Jevons being "ridiculed into silence"; and Archibald's twenty seven page essay elicited what must be among the shortest replies in the history of economic disputation: "there is a real doubt that any communication between Archibald and me is possible" (Friedman 1963, 66). Stigler (1946, v) had previously referred to the "greater (or at least more explicit) empirical content ... [of] the theory of imperfect competition". But now Stigler (1963a, 63) merely applauded Archibald's "attack on Chamberlin", and ridiculed his discussion of Chicago as a "detour ... The methodological discussion is a detour on the detour". In reply, Archibald (1963, 69) could only repeat his complaint about the methodological "inconsistency in Friedman's and Stigler's dismissal of monopolistic competition on apparently *a priori* grounds". They were, he stated, guilty of "a shocking piece of obscurantism, and an indefensible attempt to close discussion". This response echoed Chamberlin's (1957, 139) complaint about the "set of preposterous misinterpretations so standardised that there could be no questioning the fact of their common intellectual origins".

The Friedman-Stigler response could be interpreted in theological terms (as the invisible hand is sometimes interpreted): in the 'moral' struggle between the malevolent forces of monopoly and

its nemesis, the virtuous forces of competition, there was no sympathy for the proposition that most, if not all, productive agents exhibited a co-habiting mixture of the two. Certainly, in reflecting on 'Perfect Competition: Historically Contemplated', Stigler (1957b) used theological language: 'The Critics of Private Enterprise' "emphasised the evil tendencies which they believed flowed from [the] workings" of competition. In contrast, "The vitality of the competitive concept in its normative role has been remarkable ... That the concept of perfect competition has served these various need as well as it has is providential". The Stigler-Friedman methodology of positive economics was influenced by the Popperian demarcation line between science and non-science; they appeared to be relocating monopolistic competition on the non-science side of the demarcation line, whilst emphasising the "providential" location of perfect competition on the science side. For Friedman (1953, 9), "great confidence is attached to [a hypothesis] if it has survived many opportunities for contradiction"; perfect competition survived the opportunity for contradiction offered by Chamberlin and Archibald.

Stigler did not neglect monopolistic competition with his Chicago students at this time; he "eviscerated" the concept and "conduct[ed] a Demolition Derby ... shattering theories with gusto". This training led his students to find only "meaninglessness" from advocates of alternative concepts (Sowell 1993, 787).^{lxvii} ^{lxviii} As macroeconomic paradigmatic challengers, Friedman and Stigler were only too willing to set up the "fox hunts" of controversy. Indeed, according to Stigler (1962c, 1; 1965a, 16) the statistical evaluation of economic relationships was the only distinctive trait of modern economics; in his AEA Presidential Address he reiterated that the Keynesian revolution was one of several "minor revisions compared to the vast implications of the growing insistence on quantification ... comparable to the displacement of archers by canons ... I am convinced that economics is finally at the threshold of its golden age - nay, we already have one foot through the door ... the age of quantification is now full upon us". Stigler (1982c, 13, 22-3) repeated this assessment at "the high noon of capitalism ... This is the age of quantification ... An inconvenient *a priori* argument can always be eroded or blunted by challenging its exclusion or inclusion of some assumption, but economists find it difficult to resist well-established *empirical* [emphasis in text] findings".

Stigler "wore his passion for measurement on his sleeve" (Demsetz 1993, 795);^{lxix} he also noted the "unfortunate paradox that the more meaningful a concept is for analytical purposes, the less useful it usually is for empirical investigations" (1939, 470). Stigler (1949a, 103) ran (possibly) the first Keynesian-Monetarist statistical race, finding for the latter a correlation coefficient of 0.904, and for the former, 0.395. The testing procedure by which alternative ideas won or lost status involved "trial by combat" - an art in which Stigler and Friedman were highly skilled; but their stated views on statistical trial by combat were rather different from those of their opponents (Stigler 1988a, 116; 1988c, 12).^{lxx}

6. Concluding Remarks

The 1959 AEA conference program had been drawn up by the AEA President (Arthur Burns) and Stigler. The conference opened with a joint session with the Econometric Society in which Samuelson and Solow (1960) outlined the empirical American Phillips Curve trade-off that would unwittingly contribute to the Keynesian demise. The conference ended with a sociological paper from Stigler (1965b [1960], 17) which sought to explain the "areas of active work and the lines of attack" in the economics profession. The writing of history requires an organising framework: these two implicit 'manifestoes' illuminate much of the succeeding quarter of a century. This paper has specifically examined the Chicago revolution in the light of one of these implicit 'manifestos'.

As Chicago revolutionaries, Stigler and Friedman complemented each other, and it is natural to seek a partial explanation for the success of the Chicago revolution in their combined talents (Friedman 1993, 772; Sowell 1993, 788). Friedman contributed to the attack on the 'accuracy of assumptions' method of the Keynesians and the advocates of monopolistic competition, and also to the direct assault on the Keynesian hegemony. Stigler brought to the partnership a masterful understanding of the nature of knowledge construction and destruction in the economics profession (and area in which Friedman was a highly perceptive amateur). This Chicago (but Fabian, that is gradualist) intellectual revolution was launched on a profession whose historical sub-discipline had been "permanently declining in professional esteem"; "a nearly moribund subject in the United States" (Stigler 1988a, 28; 1982a [1972], 85).^{lxxi} For Stigler (1961b, 213) "knowledge *is* power. And yet it occupies a slum dwelling in the town of economics" [emphasis in text].

In his definition of competition, Stigler (1968b, 181) thought that it would be shocking if competitors refused to compete.^{lxxii} But Friedman and Stigler declined to engage in the 'regression race' over monopolistic competition offered by Chamberlin and Archibald; although they had successfully engaged their macroeconomic opponents in such a race. Economists are *Free to Choose* their research priorities and the current revival of interest in theoretical models of monopolistic competition has yet to generate an impressive empirical counterpart. Besides, Chamberlin, Archibald *et al.* could conceivably have undertaken the 'race' themselves.^{lxxiii} But Stigler (1971, 1974) contributed much to the analysis of asymmetric free or cheap rider situations where agents, in the absence of coercion, considered carefully who would appropriate the benefits before taking part in joint ventures.^{lxxiv} Their response to Archibald and Chamberlin was the optimal allocation of Chicago energies, whilst constraining the vitality of monopolistic competition explanations. From the mid-1950s to the mid-1980s, *Testing Monetarism* and estimating money demand functions became major preoccupations for

economists (Desai 1981; Laidler 1977; Stein 1982); testing or investigating monopolistic competition never qualified for such attention during that period. Simultaneously, faith in competition rose; while faith in Keynesian economics fell.^{lxxv}

The defenders of microeconomic orthodoxy believed that the advocates of monopolistic competition had defined terms which "evades the issue, introducing fuzziness and undefinable terms into the abstract model where they have no place, and serves only to make the theory analytically meaningless" (Friedman 1953, 38). The defenders of Keynesian macroeconomic orthodoxy made similar accusations about the Chicago challengers. Stigler's understanding of the sociology of economic knowledge led him to predict that some commonly accepted theories would have a "low vitality" (1949a, 104); a theory could "limp along for a century, collecting large pieces of good reasoning, and small chunks of empirical evidence but never achieving scientific prosperity"; the academic poverty of a theory could be caused by its concepts having "eluded confident measurement" (1968a, 71). Stigler and Friedman had reasons for not wishing to contribute to the process by which some theories (the ones they disapproved of) acquired confident measurement.

Stigler (1969c, 227) recognised that a fertile school requires an opposition with which to argue; and Stigler and Friedman were supremely skilful in both engaging in - and avoiding - ongoing controversy with their policy opponents: "a scholar is an evangelist seeking to convert his learned brethren to the new enlightenment he is preaching" (Stigler 1988a, 211). As macroeconomists they were reformers, or revolutionaries; as microeconomists they were conservatives: "Unlike the reformers, who seek to convert, the conservatives seek to defeat ... the successful conservative must also be an innovator" (Stigler 1975, 320-1). As the paradigmatic challenger, Friedman brilliantly exploited the Keynesian faith in macroeconomic estimation; a faith he did not share (Leeson 1998). But monopolistic competition was an upstart that had failed to reconstruct microeconomics around the concept of monopoly. Competition was still "the main dish" (Stigler 1968a, 5); and microeconomic revolutionaries still had to overcome the "heavy" legacy of pure competition (Chamberlin 1957, 140). Stigler (1965d, 76) commented on the inherently asymmetric nature of this type of competition: "If you support the majority view, a cliché is often enough to support the position. It is when you are with the minority that a real burden of evidence is put upon your arguments". Despite its revolutionary potential, Stigler (1976a, 347; 1968a [1962], 251) thought that monopolistic competition remained a species of "Cambridge eccentricity", which involved "irrelevant numbers" and "dark suspicions". Friedman and Stigler displayed tactical astuteness in declining the invitation to participate in the "Chamberlin versus Chicago" competition.^{lxxvi} The Chicago counter-revolution was, in part, the product of superior sociological perceptiveness.

Many Old Keynesians were taken aback by the success of the Chicago revolution of the 1960s and 1970s. Robert Solow (1965, 146) concluded that "I think that most economists feel that short-run macroeconomic theory is pretty well in hand ... The basic outlines of the dominant theory have not changed in years. All that is left is the trivial job of filling in the empty boxes, and that will not take more than fifty years of concentrated effort at a maximum". The prospect of losing their hegemonic position was not, apparently, taken seriously: "*Après moi, la sociologie*" (Solow 1967, 119). But a considerable volume of "sociologie" of economic knowledge *preceded* the successful Chicago intellectual revolution; much of it authored by George Stigler.

One of Stigler's obituaries closed with Milton Friedman's assessment of the attitude of his friend and collaborator: "let the chips fall where they may, my task is to be objective, accurate and interesting" (cited by McCann and Perlman 1993, 1012). But Stigler was also acutely interested in sociological questions (Rosenberg 1993, 841).^{lxxvii} Combined with this sociological perceptiveness was an almost "irrational ... sense of loyalty ... Much of his work centred around saving the damsel in distress, neoclassicalism, from her attackers" (Friedland 1993, 780; Demsetz 1993, 794; Yordon 1992; Freedman 1995).^{lxxviii} Chamberlin (1947, 416) complained that Stigler possessed a "'faithful until death' attitude towards perfect competition". Stigler (1951, 126) acknowledged this tendency: "The admonition to keep one's mind open and skeptical, for example, is fatal".^{lxxix} In the 1950s, Stigler (1956a, 278-9) concluded that there was "no prima facie contradiction of the classical view of the positive relationship between competition and progress or, indeed, as much support for the contrary view as the devil usually provides for clever heresies". But by the late 1960s, confidence in the working of competition was "at a low level ... [and] the majority of economists have lost much of their faith". According to Stigler (1967c, 356, 361) "The only effective challenge to generous opportunism is a trenchant ideology, and that is precisely what we no longer have".^{lxxx}

Stigler (1946, 3) opened the first edition of his *Theory of Price* textbook with the assessment that "The important purpose of a scientific law is to permit prediction, and prediction is in turn sought because it permits control over phenomena". Stigler and Friedman also sought influence and control over economists' research agenda. It is worth asking why they were (at least temporarily) successful in this regard. Beneath the veil of economics lie some fascinating processes of knowledge construction and destruction that follow laws and tendencies as examinable as the laws and tendencies of economic behaviour.

NOTES

i. This paper was planned around 1990, after having read Neil DeMarchi's account of Popper and the M2T economists. Unfortunately, by the time I got round to writing it, George Stigler and Chris Archibald were dead, and so I never benefited from their thoughts.

ii.. Together with W. Allen Wallis, they were the "three musketeers" of the postwar Chicago School (Leube 1986, xiii).

iii.. Chamberlin (1957, 93, 148) wrote that "It is possible that the economy should be made 'more competitive'; it is also quite possible that it should be made 'more monopolistic'". He also referred to the "fallacious ideal of perfect competition".

iv.. In 1982, after winning the Nobel prize, Stigler (1988a, 137) held an impromptu press conference at the White House and was "removed from the platform in a manner reminiscent of vaudeville days" after mentioning the word "depression". The unemployment associated with the monetary disinflation of the early Reagan years had rendered this word "obscene". Despite Stigler's lack of tact or discretion, Robert Barro's (1991) concluded that he acquired an influence which "exceeded that of any economist who has spent much time in Washington".

v.. Stigler (1961a, 330) noted that "the study of how economic theory influences views on policy by non-economists is still an essentially unstudied subject".

vi.. Melvin Reder (correspondence 1 June 1997) is doubtful that this presentation would have been successful: "I doubt that he would have owned up to it, even if he had been made aware of it. George was very sceptical of 'big think' in all its forms and very likely would have tried hard to puncture such a theory had it been presented to him".

vii.. Chamberlin (1957, 300) juxtaposed two quotes from Stigler and Friedman (emphasising the repetitive use of the word 'meaninglessness') and concluded that "The idea is virtually the same, and I am unfortunately unable to identify its origins as between the two writers cited".

viii.. Johnson arrived at Chicago in 1959, shortly after Stigler's return. According to Shils (1977, 87), Johnson found the atmosphere uncongenial and considered leaving shortly after arriving. One of the first articles he published at Chicago was entitled 'The Consumer and Madison Avenue'; but the title "epitomizes the pervasive and subtly corrupting influence of the hucksters on the American way of life" and had been chosen for him by "a senior professor of economics who is presumably more expert than I in the mass marketing of economic wares" (1960, 3).

ix.. McCloskey (correspondence 26 September 1997) also reflected that "I was nearly the last student of Chamberlain at Harvard. It was a strange experience to go from an environment in which one sneered at Stigler to one in which one sneered at Chamberlin".

x. There is evidence in Stigler's writings to support these concerns. Stigler (1967d), for example, in criticising Galbraith's framework as a "poor vehicle to carry us to an understanding of our times" also stated that he was guilty of making a "nighttime leap over the ocean of motive".

xi.. "Here was a Chicago theory that didn't even annoy socialists!" (Stigler 1986 [1982], 144).

xii.. Stigler (1987a, 52) reflected that "a great nation can survive and prosper no matter how misinformed its political leaders".

xiii.. Whilst advising Presidential candidate Barry Goldwater, Friedman noted (on leave at Columbia) a chink in the Old Keynesian armour: "I was appalled at what I found. There was an unbelievable degree of intellectual homogeneity, of acceptance of a standard set of views complete with cliché answers to every objection, of smug self-satisfaction at belonging to an ingroup. The closest similar experience I have ever had was at Cambridge, England, and even that was a distant second. The homogeneity and provincialism of the New York intellectual community made them pushovers in discussions about Goldwater's views. They had cliché answers but only to their self-created straw men. To exaggerate only slightly, they had never talked to anyone who really believed, and had thought deeply about, views drastically different from their own. As a result, when they heard real arguments they had no answers, only amazement ..." (1974, 16; 1967; see also Stigler 1973a).

xiv.. Rose Friedman (1976a, 30; 1976b, 23) described Stigler as "one of our dearest friends". It was Stigler who persuaded the Friedmans to come to the University of Minnesota in 1945.

xv.. Friedman was President of the Society during its twenty fifth anniversary and believed that it should disband having accomplished its major purpose (Rose Friedman 1976 (9), 26).

xvi.. "George was and is a delight and a treasure as a friend and as an intellectual influence ... Few economists have germinated so many new ideas and so profoundly influenced the course of economic research" (Friedman 1986, 84).

xvii.. Friedman (1976, 235) wrote: "let there be an unanticipated decline in aggregate demand, so that employers are willing to hire fewer workers at each real wage rate as perceived by them. Workers searching for jobs will find fewer offers that, on the basis of their unchanged anticipations, are attractive enough to compensate them for giving up the search. The average time between jobs will lengthen, and so will recorded unemployment. As the less attractive employment situation becomes more widely known, job-seekers will revise their anticipation about opportunities, become less choosy, and recorded unemployment will decline towards its natural level". Stigler's (1962a, 94, 104) 'Information in the Labor Market' is concerned, in large part, with the problem of how job seekers "acquire information on the wage rate ... a highly rewarding area for future research".

xviii.. Rosen (1993, 813-4) concluded that Stigler's unrelenting opposition to monopolistic competition hindered the development of the economics of product differentiation.

xix.. Keynes did not utilise an imperfect competition framework in the *General Theory* for tactical reasons: he apparently informed Gardiner Means that he "wanted to beat the buggers at their own game" (cited by Marris 1992, 1241, n13).

xx. At least one (recently arrived) Chicago economist concluded that economists were no longer "concerned with matters of vital importance to the affluence society"; and had turned

away "from intellectual interest into professional competence" as expressed in mathematical and econometric techniques (Johnson 1960, 119-120).

xxi.. Stigler (1970, 426; 1957a, 159; 1982a, vii, 86) cautioned that "biography distorts rather than illuminates the understanding of scientific work", although he also thought that Rogin's history of economic theory intentionally added little, but unintentionally shed "an illuminating light" on a species of American late-New Deal radical thought.

xxii.. Sherwin Rosen (correspondence, 23 April 1997) recalled that the question of influence came up in conversations with Stigler "rather often, but in a rather oblique casual way and not particularly in a formal way". Deirdre McCloskey (correspondence, 2 June 1997) recalled that "they were self-conscious counter-revolutionaries. They talked about it all the time".

xxiii.. Empirical workers "do not give [their] generalisations the abstract and systematic formulation that characterises conventional economic theory. As a result, the generalisations seem closely bound to the specific empirical researches on which they are based, and they lend themselves much less to cumulative refinement and elaboration and to widening areas of application".

xxiv.. He argued that "received theory deserves more respect and quantitative materials less respect than are commonly accorded them".

xxv.. "Unless an author explicitly sets out to refute a theory, one should characterise his attitude towards that theory as favourable, or at worst neutral, if he actually refers to the theory. For he is reviving its currency and advertising its existence" (1978, 196).

xxvi.. "The theorist is a dangerous person to let loose on economic reform" (1980, 349).

xxvii.. He stated that "two thirds *at a minimum* made no positive contribution to received knowledge on oligopoly behaviour: they contain neither a new fact nor a new idea [emphasis in text]". Likewise, papers that were critical of Reaganomics contained 94 percent malice and about 2 percent knowledge (1988d, 91).

xxviii.. The "scientific content of a man's work is the content as interpreted by his contemporaries" (1990b, 765).

xxix.. Stigler (1949a, 95) also noted that: "Tactically this is perhaps Lord Keynes's greatest contribution: his contributions need not be itemised".

xxx.. On 12 February 1992, Samuelson wrote to Patinkin (just after Stigler's death): "Often I would write a paper really with George Stigler in mind. Almost never would he vouchsafe a reaction. Still we shall miss him". Patinkin replied (6 March 1992): "I'm glad that we had the same experience with George Stigler. In my case it was even worse ..." (Patinkin Papers, Duke University).

xxxi.. Whatever Stigler's motives, he succeeded in provoking his audience; Robert Solow thought that the contents of his Harvard lecture were "untrue" (1970, 98; see also Coats 1960 and Rothbard 1960). Robert Solow (correspondence 23 April 1997) points out that his friendship with Stigler transcended any disagreements they had over economics; Stigler was

"never an ideologue" (Solow cited by Passell 1991).

xxxii.. For example, Stigler (1989, 659-660) declined to deal in detail with a "discursive and imprecise" *History of Political Economy* review of his essay on Smith and public choice.

xxxiii.. "The strong hostility of most intellectuals towards Madison Avenue is possibly due to the rivalry between the two groups. Could the failure of Madison Avenue to reciprocate this overt dislike be an instance of the professional's indifference to the amateur?" (1975, 317).

xxxiv.. Stigler (1988a, 108-9; 1962b, 1; 1964b, 20; 1964c, 83) was only "provoked to attack" Means' theory of price determination - after it became integral to macroeconomic fox hunts of controversy via the Kefauver Committee on administered pricing and inflation; and when it seemed there may be "No End to Means". The few remaining economists with an interest in administered prices were left "[without] a subject".

xxxv.. "The fact, however, is indisputable that economists generally abandoned a traditional doctrine [the quantity theory of money] in important part because of a question of fact which they decided by casual observation ... A single contrary observation causes consternation, for the whole edifice may tumble - indeed it will tumble if a few prominent economists capitulate".

xxxvi.. He also reflected that "a formal position" was easier to overcome than an opposition that was "inarticulate and unrationalised" (1966b, 278).

xxxvii.. Knight had a functional, almost tactics-driven approach to economic theory and its history: "to contribute to the understanding of how by consensus based upon rational discussion we can fashion liberal society in which individual freedom is preserved and a satisfactory economic performance achieved" (Stigler 1987b, 57-8). In an unpublished lecture, Knight stated that "truth in society is like strychnine in the individual body, medicinal in special conditions and minute doses; otherwise and in general a deadly poison ..." (cited by Stigler 1987b, 59).

xxxviii.. Knight advised his students that "You can be with the majority or you can be in the right" (Patinkin 1973, 798). Rose Friedman (1976, 30) who was Knight's research assistant (1934-6) recalled that Knight's scepticism "unfortunately discouraged some of his students from making the contribution that they might otherwise have made".

xxxix.. Stigler (1962d, 71) thought the title invited a "slovenly stereotype"; it was also geographically inaccurate in that Friedman, he rather provocatively claimed, was the leader of the "Berkeley-Cambridge axis".

xl.. Stigler has been accused of misinterpreting the Coase Theorem; Coase stated this in conversation, and hinted at it in his writing (correspondence from McCloskey 3 June 1997; McCloskey 1997). Stigler (1966a, 113) wrote that "the Coase theorem thus asserts that under perfect competition private and social costs will be equal ... this procedure obviously leads to the correct social results"; it was a "remarkable proposition to us older economists who have believed the opposite for a generation". Coase (1988, 14; 1993, 239-240, 249) migrated to the United States in 1951, in part because of his admiration for Stigler and Knight. He distinguished between the "Coase Theorem" (with inverted commas) as "formalised by Stigler

... It would not seem worthwhile to spend much time investigating the properties of such a world", and the Coase Theorem (without inverted commas), a "preliminary to the development of an analytical system capable of tackling the problems posed by the real world of transactions costs".

xli.. This agenda (law and economics, the economics of the state, regulation etc) increasingly dominated research at Chicago and "George was as much product of this atmosphere as producer" (correspondence from Reder, 1 June 1997).

xlii.. "The traditional approach has tended to obscure the nature of the choice that has to be made ... the suggested courses of action are inappropriate" (Coase 1960, 1-2).

xliii.. Referring to "focus", Telser continued: "I believe this is the main lesson I learned from Friedman" (correspondence 29 April 1997).

xliv.. Bhagwati's obituary of Johnson was not well received at Chicago; it was perceived to be self-serving and inaccurate (correspondence from McCloskey 2 June 1997). Friedman (correspondence, 5 February 1995) took great exception to aspects of it. He also recalled that "So far as I know, the seminars were never testing imperfect competition yet there is an element of truth in what Bhagwati said. That is that most of the empirical work that was going on was undertaken to see whether the simple assumption of competitive hypotheses for apparently imperfectly competitive industries led to results that were contradicted or not contradicted by the available evidence. However, that did not arise from any attempt to test imperfect competition but, much more fundamentally, to see how much you could explain by the competitive approach and how much was left that would have to be explained by monopoly. There were also quite a number of articles during that period which offered explanations relying on monopolistic competition positions for many observed phenomena such as retail price fixing, tie-in arrangements, and the like". Lester Telser (correspondence 28 April 1997) also informed me of the empirical work on issues raised by monopolistic competition by members of the industrial organisation group at Chicago (e.g. Telser 1962a, 1962b and 1971; Demsetz 1962). Telser also points out that Stigler's *Theory of Price* (1966, 32, n18) refers to one of these articles (Telser 1962b).

xlvi.. "Chamberlin pointed the way to a revolutionary change in price theory ... The major defence of Chamberlin's principal contribution rests on the methodologically sound proposition that an economic theory can only be as good as its assumptions ... [the Chicago] stand is transparently ludicrous" (Bain 1967, 150-3); Chamberlin provided an "inspiring vision of realistic economics" (Kuenne 1967, v); "it has to be recognised that a general abandonment of the assumption of perfect competition, a universal adoption of the assumption of monopoly, must have very destructive consequences for economic theory" (Hicks 1946, 83). Bronfenbrenner (1950, 82-3) offered a "compromise" by attempting to integrate monopolistic competition into economic theory "without disturbing the fundamentals of neoclassical economic thought".

xlvii.. Edward Mason, Gardiner Means and others at Harvard pioneered *The Market Concentration Doctrine* (Demsetz 1973) which is associated with the development of industrial organisation as a subdivision of economics (Phillips and Stevenson 1974).

xlvi.. There is some homogeneity behind these disagreements: only 3 per cent of graduate students believed that a successful economist required a thorough knowledge of the economy ("very important"), and 68 per cent thought it to be unimportant.

xlvi.. First year graduate students at Columbia would find no reference to Chamberlin or Robinson in Gary Becker's (1971) price theory course, outside the reading list (which contains twenty references to Friedman and Stigler). In the recommended reading list for his Economics 300 A and B (September 1948), Friedman included Robinson (1933, chapter 2) and Chamberlin (1933, chapter 3, sections 1, 4, 5 and 6 and chapter 5).

xlix. One reviewer claimed that he had "unduly idealised" limited competition, just as the older generation had previously done with perfect competition (Nichol 1934, 337).

l. Chamberlin (1962, 5, n4, 226, n1) was sensitive about timing with respect to the reconstruction of value theory. Sraffa's famous essay was published in 1926; but Chamberlin stated that his thesis (submitted on April 1, 1927) was at the time of Sraffa's article "virtually complete". Likewise, Hotelling's 'Stability in Competition' appeared in March 1929, several months before Chamberlin's essay on duopoly (November 1929), and Chamberlin felt obliged to explain why he had not referred to it.

li.. Stigler was "deeply impressed by the fact that if any one person had dictated the lines of research [in economics] fifty years ago, we would be much behind where we are today. It is worse than that. If I had this power twenty years ago, our discipline would have suffered grievously".

lii.. His *Theory of Competitive Price* (1942, v) was written with the intention of including this material at a later date.

liii.. Stigler (1940, 364) complained about monographs which paid no attention to market forms "other than perfect competition and simple monopoly".

liv.. "George and I carried on an intensive correspondence while he was exiled to Brown and Columbia. On perusing the surviving records recently, I was struck by his contribution to my methodology article, in the course of exchanges between us about an article that he was writing on imperfect competition" (Friedman 1993, 770).

lv.. The Friedman-Stigler correspondence shows a concern about the struggle for influence: "I keep feeling that you arouse skepticism and opposition by stopping where you do ... [elaboration] will create sympathy for and receptiveness to your thesis and make the paper much more influential" (letters from Stigler to Friedman, cited by Hammond 1991b, 12, 23). Friedman's methodology was constructed in opposition to the desire for descriptive 'realism' and the 'assumption questioning' tendency of both monopolistic competition and Keynesian macroeconomics. One rhetorical device employed by Friedman (1953, 3) was to invoke the authority of Keynes (John Neville, *The Scope and Method of Political Economy*) in the opening sentence of his famous methodological essay. Ten years earlier, Stigler (1943, 358, n7) noted the potential potency of this rhetorical device: "I did not realise how neglected this excellent book had become in America until, when I recently referred to it, several friends expressed surprise that I did not know Keynes' middle name was Maynard!"

lvi.. "A good deal of support for this theory stems from the mistaken demand for correspondence between 'reality' and premises". Chamberlin, a "true revolutionary", and his disciples had assigned a "fundamentally mistaken role ... to general theory". The Walrasian theory of general equilibrium had "proven to be relatively uninformative"; formalism, in its mathematical guise, "should not be made a puppet of a scientific oligarchy". This had been the "period of the clever gadget and the plausible surmise ... the triumph of statistics over history as the source of empirical knowledge", when hasty statistical studies falsely concluded that the consumption function was more stable than the velocity of money.

lvii.. "My appraisal of the theory in 1947 was stimulated more by a growing interest in the empirical testing of theories than by the intrinsic interest of the kinked demand curve ... The main task of the article was a test of the empirical fruitfulness of the theory".

lviii.. In 1995, Milton Friedman very kindly invited me to visit him at the Hoover Institution, but the day before I arrived he suffered a stroke and he was only able to spend an afternoon with me. While he was in hospital I spoke to a number of his colleagues. One Senior Fellow expressed his admiration for both Friedman and Stigler but was surprised when they both walked out of a seminar given by a newly recruited junior economist who had expressed support for some aspects of Galbraith's work. The junior economist was promptly removed from the Hoover Institution. Likewise, Galbraith (1978, 150-1) did not forget his adversaries. The Hoover Institution economists who advocated the "romance" of deregulation were "trying to recapture the world of Herbert Hoover - it's a very worthy ideal, intellectual archaeology as it were. Once ageing and righteous scholars of conservative mood dreamt of going to Heaven. Now it's to the American Enterprise Institute". Galbraith (1981, 31) was told that Friedman opposed his nomination as AEA President: "he offered as his clinching evidence that Veblen had never been president". Mark Perlman (correspondence 6 June 1997) was present when Friedman was chairman of the nomination committee: "Friedman can be a perfect gentleman, and he presented [Galbraith's] nomination in good grace. All present knew what pain it cost him, but we all admired his composure".

lix.. "With the decline of independent market behaviour - or perhaps more accurately its decay as a plausible assumption - a gap has been left in our explanation of the operative mechanics by which the economy is governed".

lx.. Stigler (1943, 357) attributed responsibility for the decline in the prestige of economists to Galbraith's Office of Price Administration, where he had briefly been employed (Telser 1996, 2). Later, he attributed the "weakened ... status" of economists to their high profile involvement in the government apparatus (1967c, 360). Later still, Stigler (1987a, 56) referred to Galbraith's "impressive record for error".

lxi. Senator Kefauver (1965 [1963], 211-2, 180-1), the Chairman of the Senate Subcommittee on Antitrust and Monopoly, suggested that "to give us a new focus for our thinking" a "new term" was required; replacing 'private enterprise' with 'collective enterprises' or 'private socialism' or 'private economic government'. Kefauver also noted the availability of a "plethora" of government statistics with which to pursue these investigations: "There has been something of a tendency to gather statistics which *might* be valuable in the examination of a 'controversial' topic" [emphasis in text].

Ixii.. Stigler (1943, 529) thought that the "chief policy applications" of national income figures were made to monetary analyses.

Ixiii.. Friedman (1965, 51) stated that "there is no general theory of the second best. There is the proposition that there may exist a theory of the second best".

Ixiv.. Friedman and John Savage (1948, 282) in their 'Utility Analysis of Choices Involving Risk', proposed "a crude empirical test by bringing together a few broad observations"; but Archibald (1959b, 437) proposed "a crucial test" which Friedman and Savage had "apparently overlooked".

Ixv.. This was comment 20 in Stigler's (1977, 442) Conference Handbook: "What empirical findings would contradict your theory?"

Ixvi.. "[S]ignificant implications cannot be obtained without more information than is usually assumed or readily available ... When we turn to Chamberlin's defence we again fail to find any statement of predictions".

Ixvii.. Sowell was specifically referring to Stigler's ex-Columbia colleague, J.M. Clark's concept of workable competition, which Stigler (1964b, 20) regarded as an academic employment-creating research agenda.

Ixviii.. Lester Telser (1964, 562, n20, 558) found Archibald's challenge "interesting" but deficient with respect to the identification problem; he found little empirical support for an inverse association between advertising and competition, "despite some plausible theorising to the contrary".

Ixix.. Rosenberg (1993, 837) concluded that "no previous scholar had ever examined the development of the discipline with anything like the same insistence that intellectual progress had to be measured in terms of its ability to generate empirically refutable implications".

Ixx.. Stigler (1988a, 116) wrote: "The computer has made it easy to fish for results. If the statistical analysis doesn't come out 'right' the first or the twentieth time, one can drop a year from the data, add a new variable to explain contradictions, take the logarithm of another variable, and so on until, lo, the desired answer appears - all in just a few minutes". Econometricians were the "most quarrelsome class of economists" (1988c, 12).

Ixxi.. Stigler contributed to this development: in 1972, he successfully proposed that the history of thought requirement be dropped at Chicago. Most other economics departments later followed suit (Rosen 1993, 811). At the same meeting Stigler unsuccessfully proposed that the economic history requirement also be dropped (correspondence from McCloskey 2 June 1997).

Ixxii.. "[W]e would be shocked if two teams called off the event".

Ixxiii.. Archibald and Rosenbluth (1975, 588) outlined some of "the implications of an analytically tractable definition of a monopolistically competitive group".

Ixxiv.. In another context, Stigler (1943, 357) deplored the constraint that some approaches to

economics imposed: "The familiar admonition not to argue over differences in taste leads not only to dull conversation but also to bad sociology". With respect to "the economics of scholarly advice ... I have always thought that revealed preference is the only reliable guide to what a scholar believes to be fruitful research problems: If he doesn't work on them, he provides no reason for us to do so" (Stigler 1981, 76).

lxxv.. According to the Director of the Hoover Institution (Campbell 1986, x), this renewed faith in competition and deregulation was due to Stigler's Copernican-like role.

lxxvi.. As Reder put it: "Why should Friedman or Stigler have entered into a controversy which could, at best, only serve to highlight a possible deficiency of laissez-faire and (in their view) exaggerate its importance" (correspondence 1 June 1997).

lxxvii.. Rosenberg (1993, 841) described some of Stigler's questions as "whether there were laws or regularities shaping the growth of knowledge itself ... How does the research agenda of any science get to be determined? ... How do scientists persuade one another? To put it in its boldest possible form: What are the underlying laws governing the evolution of a science?"

lxxviii.. Stigler (1986 [1981], 333) referred to "my friend, the competitive economy". Deviations from neoclassical perspectives produced, in James Meade's work, "bastard demand curves" (Stigler 1966c, 479). During the War, he passionately objected to (finding it "especially hard to excuse") a book that remained silent about political questions: "Is the purpose of the study of economics the production of impartial pages? Has the democratic citizen no moral responsibility to state and strive for that order of things which he believes is best?" (Stigler 1943c, 78).

lxxix.. "Not only do we not know how to teach students how to invent theories, but much of the advice is surely wrong. The admonition to keep one's mind open and sceptical, for example, is fatal. Every useful hypothesis will soon encounter facts that are at least superficially adverse, and one must love one's own creations and cling stubbornly to them until the contradictory evidence is overwhelming. On the whole economists fully meet this recommendation".

lxxx. Some of his critics found his testing procedures to be defective: "a triumph of ideology over scholarship" (Friend and Herman 1964, 382).

BIBLIOGRAPHY

American Enterprise Institute. 1988. Ideas, Their Origins and Their Consequences: Lectures to Commemorate the Life of G. Warren Nutter. Washington: American Enterprise Institute.

Archibald, C.G. 1959a. The State of Economic Science. British Journal of the Philosophy of Science 10, May: 58-69.

___ 1959b. Utility, Risk and Linearity. Journal of Political Economy October, LXVII.5: 437-450.

___ 1960. Testing Marginal Productivity Theory. Review of Economic Studies 27: 210-213.

___ 1961. Chamberlin versus Chicago. Review of Economics and Statistics October: 2-28.

_____ 1963. Reply to Chicago. Review of Economics and Statistics 30: 68-71.

Archibald, G.C. and Rosenbluth, G. 1975. The 'New' Theory of Consumer Demand and Monopolistic Competition. Quarterly Journal of Economics LXXXIX: 569-590.

Bain, J. 1948. Price and Production Policies. In Ellis ed.

___ 1964. The Impact on Industrial Organisation. American Economic Association Papers and Proceedings May: 28-32.

___ 1967. Chamberlin's Impact on Microeconomic Theory. In Kuenne ed.

Barro, R. 1991. An Unregulated Economist. Wall Street Journal Monday December 9.

Baumol, W. J. 1964. Monopolistic Competition and Welfare Economics. American Economic Association Papers and Proceedings May: 44-52.

Becker, G. 1971. Economic Theory. Alfred Knopf: New York.

___ 1993. George Joseph Stigler: January 17, 1911-December 1, 1991. Journal of Political Economy 101.5, October: 761-767.

Berle, A. A. Jr. 1953. American Capitalism. Review of Economics and Statistics February: 81-84.

Bhagwati, J. N. 1977. Harry G. Johnson. Journal of International Economics 7: 221-9.

Bishop, R. L. 1964. The Theory of Imperfect Competition After Thirty Years: The Impact on General Theory. American Economic Review 54, May: 33-43.

Breit, W. and Spencer, R.G. 1986. eds. Lives of the Laureates. Cambridge, Mass.: MIT Press. Reprinted 1988, 2 ed 1990. 3 ed 1995.

Bronfenbrenner, M. 1950. Imperfect Competition on a Long-Run Basis. Journal of Business of

the University of Chicago April: 81-93.

Brown, E. C. and Solow, R. 1983. eds. Paul Samuelson and Economic Theory. New York: McGraw Hill.

Buchanan, J. M. 1991. Frank H. Knight. In Shils ed.

Campbell, W. G. 1986. Foreword. In Stigler.

Chamberlin, E. H. 1933. The Theory of Monopolistic Competition. Harvard: Harvard UP. 1 ed.

___ 1937. Monopolistic or Imperfect Competition? Quarterly Journal of Economics August: 557-580.

___ 1946. Discussion. American Economic Association Papers and Proceedings May: 139-142.

___ 1947. Review of Stigler's *Theory of Price*. American Economic Review June: 414-418.

___ 1954a. Measuring the Degree of Monopoly and Competition. In Chamberlin ed.

___ 1954b. ed. Monopoly and Competition and Their Regulation. London: Macmillan.

___ 1957. Towards a More General Theory of Value. New York: OUP.

___ 1962. The Theory of Monopolistic Competition. Harvard: Harvard UP. 8 ed.

Coase, R. 1960. The Problem of Social Cost. Journal of Law and Economics 15.2: 1-44.

___ 1988. The Firm, the Market and the Law. Chicago: University of Chicago Press.

___ 1993. Law and Economics at Chicago. Journal of Law and Economics XXXVI, April: 239-254.

___ 1995. Ronald H. Coase. In Breit and Spencer eds.

Coats, A. W. 1960. The Politics of Political Economists: Comment. Quarterly Journal of Economics November: 666-9.

Colander, D. and Klammer, A. 1987. The Making of an Economist. Economic Perspectives 1.2, Fall: 95-111.

Demsetz, H. 1962. The Effect of Consumer Experience on Brand Loyalty and the Structure of Market Demand. *Econometrica* 30.1, January: 22-33.

___ 1973. The Market Concentration Doctrine. Washington: AEI-Hoover Policy Studies.

___ 1993. George J. Stigler: Midcentury Neoclassicalist with a Passion to Quantify. Journal of Political Economy 101.5, October: 793-808.

Desai, M. 1981. Testing Monetarism. London: Frances Pinter.

Eatwell J. Milgate M. and Newman P. 1987. eds. The New Palgrave Dictionary of Economics. London: Macmillan.

Ellis, H. S. 1948. ed. A Survey of Contemporary Economics. American Economic Association, Illinois: Richard D. Irwin. Volume 1.

Feiwel, G. R. 1982. ed. Samuelson and Neoclassical Economics. Boston: Kluwer.

Fellner, W. 1967. The Adaptability and Lasting Contribution of the Chamberlin Contribution. In Kuenne ed.

Feyerabend, P. 1975. Against Method. London: New Left Books.

Freedman, C. 1995. The Economist as Mythmaker - Stigler's Kinky Transformation. Journal of Economic Issues XXIX.1, March: 175-209.

Friedland, C. 1993. On Stigler and Stiglerisms. Journal of Political Economy 101.5, October: 780-783.

Friedman, M. 1953. Essays in Positive Economics. Chicago: University of Chicago Press.

___ 1962. Price Theory: A Provisional Text. Chicago: Aldine.

___ 1963. More on Archibald versus Chicago. Review of Economics and Statistics 30: 65-7.

___ 1965. Panel Discussion. In Ketchum and Strunk eds.

___ 1967. Value Judgements in Economics. In Hook ed.

___ 1968. Dollars and Deficits. Prentice Hall: New Jersey.

___ 1974. Schools at Chicago. University of Chicago Magazine :11-16.

___ 1976. Price Theory. Chicago: Aldine.

___ 1986. In Breit and Spencer eds.

___ 1993. George Stigler: A Personal Reminiscence. Journal of Political Economy 101.5, October: 768-773.

Friedman, M and Friedman, R. 1982. Capitalism and Freedom. Chicago: University of Chicago Press.

Friedman, M. and Savage, J. L. 1948. The Utility Analysis of Choices Involving Risk. Journal of Political Economy LVI.4, August: 279-304

Friedman, M. and Schwartz, A. 1963. A Monetary History of the United States. Princeton

University Press: Princeton, New Jersey.

Friedman, R. 1976a. Milton Friedman: Husband and Colleague (1). Oriental Economist May: 28-32.

Friedman, R. 1976b. Milton Friedman: Husband and Colleague (3). Oriental Economist July: 18-23.

Friedman, R. 1977. Milton Friedman: Husband and Colleague (12). Oriental Economist August: 20-24.

Friend, I and Herman, E. S. 1964. The S.E.C. Through a Glass Darkly. Journal of Business 37, October: 382-405.

Fromm, G. 1981. ed. Studies in Public Regulation. Cambridge, Mass: MIT Press.

Galbraith, J. K. 1948. Monopoly and the Concentration of Economic Power. In Ellis ed.

___ 1978. In Galbraith and Salinger.

Galbraith, J. K. and Salinger, N. 1978. Almost Everyone's Guide to Economics. New York: Bantam.

Gort, M. 1962. Diversification and Integration in American Industry. Princeton University Press: Princeton.

Hammond, D. 1991a. Frank Knight's Antipositivism. History of Political Economy 23.3: 359-381.

___ 1991b. Early Drafts of Friedman's Methodological Essay. Mimeo.

Harberger, A. 1954. Monopoly and Resource Allocation. American Economic Review May:77-87.

Harrod, R. 1934. Doctrines of Imperfect Competition. Quarterly Journal of Economics: 442-470.

Hicks, J. R. 1946. Value and Capital: An Inquiry into Some Fundamental Principles of Economic Theory. Oxford: Clarendon.

Hook, S.S. 1967. ed. Human Values and Economic Policy. New York: New York U.P.

Hotelling, H. 1929. Stability in Competition. Economic Journal 41, March:

Jaffe, W. 1934. Imperfect Competition. American Economic Review March: 27-30.

Johnson, E. and Johnson, H.G. 1977. The Shadow of Keynes. Oxford: Basil Blackwell.

Johnson, H. G. 1960. The Consumer and Madison Avenue. Current Economic Comment August: 3-10.

___ 1971. The Keynesian Revolution and the Monetarist Counter-Revolution. American Economic Review 61.2, May: 91-106.

___ 1972. Inflation and the Monetarist Controversy. Amsterdam: North Holland.

___ 1976. The American Tradition in Economics. Journal of Economics and Business 16, Summer: 17-26.

Kefauver, E. 1965. In A Few Hands: Monopoly Power in America. England: Penguin.

Ketchum, M.D. and Kendall, L.T. 1965. eds. Readings in Financial Institutions. Boston: Houghton Mifflin.

Ketchum, M.D. and Strunk, N. 1965. Conference on Savings and Residential Housing. Chicago: The US Savings and Loan League.

Keynes, J. M. 1936. The General Theory of Employment, Interest and Money. London: Macmillan.

___ 1971-89. The Collected Writings of John Maynard Keynes. London: Macmillan. Edited by D. Moggridge.

Kern, W. S. 1987. Frank Knight's Three Commandments. History of Political Economy 19:4: 639-646.

Kitch, E. W. 1983. ed. The Fire of Truth: A Remembrance of Law and Economics at Chicago, 1932-1970. Journal of Economics and Law April, XXVI: 163-234.

Klappholz, K. and Agassi, J. 1959. Methodological Prescriptions in Economics. Economica February 60-74.

Knight, F. 1960. Intelligence and Democratic Action. Cambridge: Harvard UP.

Kuenne, R.E. 1967. ed. Monopolistic Competition Theory: Studies in Impact: Essays in Honour of E.H. Chamberlain. New York: John Wiley.

___ 1987. Edward Hastings Chamberlin. In Eatwell et al eds.

Laidler, D. 1977. The Demand for Money: Theories and Evidence. New York: Harper and Row. 2 ed.

Leeson, R. 1998. 'The Ghosts I Called I Can't Get Rid of Now': The Keynes-Tinbergen Friedman-Phillips Critique of Keynesian Macroeconometrics. History of Political Economy 30.1: 51-94.

Leube, K. R. 1986. George J. Stigler: A Biographical Introduction. In Stigler.

Machlup, F. 1939. Evaluation of the Practical Significance of the Theory of Monopolistic Competition. American Economic Review XXIX.2, June: 227-236.

Markham, J. 1964. Discussion. American Economic Association Papers and Proceedings May: 53-55.

Marris, R. 1992. R. H. Kahn's Fellowship Dissertation: A Missing Link in the History of Economic Thought. Economic Journal 102, September: 1235-1243.

McCann, C. R. and Perlman, M. 1993. On Thinking about George Stigler. Economic Journal 103.419: 994-1014.

McCord Wright, D. 1954. Discussion. American Economic Association Papers and Proceedings May: 26-30.

McIvor, R. C. 1983. A Note on the University of Chicago's Academic Scribblers. Journal of Political Economy 91.5: 888-893.

McCloskey, D. 1986. The Rhetoric of Economics. Great Britain: Wheatsheaf.

___ 1994. Knowledge and Persuasion in Economics. Cambridge: Cambridge University Press.

___ 1997. The Good Old Coase Theorem and the Good Old Chicago School. In Medema ed.

Medema, S. G. 1997. ed. Coasean Economics. Boston: Kluwer.

Morrison, L. A. 1934. Imperfect Competition. American Economic Review March: 30.

Moss, S. 1984. The History of the Theory of the Firm from Marshall to Robinson and Chamberlin: The Source of Positivism in Economics. Economica 51: 307-318.

Nelson, R. L. 1959. Merger Movements in American Industry 1895-1956. Princeton University Press: Princeton.

Nichol, A. J. 1934. Professor Chamberlin's Theory of Limited Competition. Quarterly Journal of Economics: 317-337.

Passell, P. 1991. George Joseph Stigler Dies at 80, Nobel Prize Winner in Economics. New York Times Tuesday December 3.

Patinkin, D. 1947. Multiple-Plant Firms, Cartels, and Imperfect Competition. Quarterly Journal of Economics February: 173-205.

Patinkin, D. 1969. The Chicago Tradition, the Quantity Theory and Friedman. Journal of Money Credit and Banking February, 1: 46-70.

___ 1972. Friedman on the Quantity Theory and Keynesian Economics. Journal of Political Economy: 883-905.

___ 1973. Frank Knight as Teacher. American Economic Review 63.5, December: 787-810.

Phillips, A, and Stevenson, R. E. 1974. The Historical Development of Industrial Organisation. History of Political Economy Fall: 324-342.

Potvin, G. 1969. Hearings Before the House of Representatives Select Committee on Small Business. Washington: US Government Printing Office. Vol 1.

Reder, M.W. 1982. Chicago Economics: Permanence and Change. Journal of Economic Literature XX, March: 1-38.

___1987. The Chicago School. In Eatwell *et al.*

Robinson, J. 1933. The Economics of Imperfect Competition. London: Macmillan.

___ 1934. What is Perfect Competition? Quarterly Journal of Economics :104-120.

___ 1952. Review of Galbraith's *American Capitalism*. Economic Journal : 925-8

___ 1969. The Economics of Imperfect Competition. London: Macmillan. Second ed.

Rosen, S. 1993. George J. Stigler and the Industrial Organisation of Economic Thought. Journal of Political Economy 101.5, October: 809-817.

Rosenberg, N. 1993. George Stigler: Adam Smith's Best Friend. Journal of Political Economy 101.5, October: 833-848.

Rothbard, M. N. 1960. The Politics of Political Economists: Comment. Quarterly Journal of Economics November: 659-665.

Rottenberg, S. 1980. ed. Occupational Licensure and Regulation. Washington: American Enterprise Institute for Public Policy.

Samuelson, P. 1962. Economists and the History of Ideas. American Economic Review March, L11. 1: 1-18.

___1967. In Kuenne ed.

___ 1983. Economics in a Golden Age. In Brown and Solow eds.

Samuelson, P. and Solow, R. 1960. Analytical Aspects of Anti Inflation Policy. American Economic Review May, 50: 177-94.

Scherer, F.M. 1970. Industrial Market Structure and Economic Performance. Chicago: Rand McNally.

Schneider, E. 1967. Milestones on the Way to the Theory of Monopolistic Competition. In Kuenne ed.

Schweitzer A. 1975. Frank Knight's Social Economics. History of Political Economy 7.3: 279-292.

Selden, R. 1975. ed. Capitalism and Freedom: Problems and Prospects. Charlottesville: University Press of Virginia.

- Sills, D. L. ed. International Encyclopaedia of the Social Sciences. New York: The Free Press.
- Shaw, G. K. 1991. ed. Economics, Culture and Education: Essays in Honour of Mark Blaug. England: Edward Elgar.
- Sherrard, A. 1951. Advertising, Product Differentiation, and the Limits of Economics. Journal of Political Economy: 126-142.
- Shils, E. 1977. Harry Johnson. Encounter November: 85-9.
- ___ 1981. Some Academics, Mainly in Chicago. The American Scholar: 179-196.
- ___ 1991. ed. Remembering the University of Chicago: Teachers, Scientists and Scholars. Chicago: University of Chicago Press.
- Solow, R. 1965. Economic Growth and Residential Housing. In Ketchum and Kendall eds.
- ___ 1967. A Rejoinder. Public Interest 9, Fall: 118-9.
- ___ 1970. Science and Ideology in Economics. Public Interest 21, Fall: 94-107.
- Sowell, T. 1987. Stigler as an historian of economic thought. In Eatwell et al eds..
- ___ 1993. A Student's Eye View of George Stigler. Journal of Political Economy 101.5, October: 785-792.
- Sraffa, P. 1926. The Law of Returns under Competitive Conditions. Economic Journal #6, December: 535-50.
- Stein, 1982. Monetarists, Keynesians and New Classical Economics. Oxford: Basil Blackwell.
- Steiner, P. 1964. Discussion. American Economic Association Papers and Proceedings May: 55-7.
- Stigler, G. 1937. A Generalisation of the Theory of Imperfect Competition. Journal of Farm Economics August: 707-717.
- ___ 1939. The Limitations of Statistical Demand Curves. Journal of the American Statistical Association 34.207, September: 469-481.
- ___ 1940. Review of Carlson's *A Study on the Pure Theory of Production*. American Economic Review June: 364-5.
- ___ 1941. Production and Distribution Theories. New York.
- ___ 1942. The Theory of Competitive Price. London: Macmillan.
- ___ 1943a. The New Welfare Economics. American Economic Review June: 355-9.
- ___ 1943b. Review of Kuznets' National Income and its Composition, 1919-1938. Journal of

Farm Economics :528-533.

___ 1943c. Review of *Fiscal Planning for Total War* by Crum et al. Journal of Political Economy 51, February: 77-8.

___ 1944. Review of Saxton's *The Economics of Price Determination*. Journal of Political Economy 52, March: 81-2.

___ 1946. The Theory of Price. London: Macmillan. First Edition.

___ 1947a. The Kinky Oligopoly Demand Curve and Rigid Prices. Journal of Political Economy LX: 342-49.

___ 1947b. Notes on the History of the Giffen Paradox. Journal of Political Economy: 152-6.

___ 1947c. Domestic Servants in the United States. New York: NBER.

___ 1947d. Trends in Output and Employment. New York: NBER.

___ 1948. Review of Bain's *Pricing, Distribution and Employment - Economics of an Enterprise System*. American Economic Review 38, December: 915-6.

___ 1949a. A Survey of Contemporary Economics. Journal of Political Economy LVII.2, April: 93-105.

___ 1949b. Five Lectures on Economic Problems. Longman: LSE.

___ 1950a. Monopoly and Oligopoly by Merger. American Economic Association Papers and Proceedings XL.2, May: 23-34.

___ 1950b. Employment and Compensation in Education. New York: NBER.

___ 1951. Discussion: Economic Theory, Statistics and Economic Practice. American Economic Association Papers and Proceedings May: 126-8.

___ 1952. The Theory of Price. New York: Macmillan. Second Edition.

___ 1954a. 1954. The Economist Plays with Blocs. American Economic Association Papers and Proceedings May: 7-14.

___ 1954b. The Early History of Empirical Studies of Consumer Behaviour. Journal of Political Economy LXII.2: 95-113.

___ 1955a. Mathematics in Economics: Further Comments. Review of Economics and Statistics 37, August: 299-300.

___ 1955b. The Nature and Role of Originality in Scientific Progress. Economica November: 293-302.

___ 1955c. Introduction. In Business Concentration and Pricing Policy. Princeton, N. J.:

Princeton University Press (for NBER).

___ 1956a. Industrial Organisation and Economic Progress. In White ed.

___ 1956b. The Statistics of Monopoly and Merger. Journal of Political Economy: 33-40.

___ 1957a. Review of Rogin's *The Meaning and Validity of Economic Theories*. American Economic Review 159-64.

___ 1957b. Perfect Competition, Historically Contemplated. Journal of Political Economy February, LXV.1: 1-17.

___ 1959a. The Politics of Political Economists. Quarterly Journal of Economics 73: 522-532.

___ 1959b. Foreword. In Nelson.

___ 1961a. Review of Grampp's *The Manchester School of Economics*. Economica August: 329-330.

___ 1961b. The Economics of Information. Journal of Political Economy June, LXIX.3: 213-225.

___ 1962a. Information in the Labour Market. Journal of Political Economy 70.1: 94-105.

___ 1962b. Administered Prices and Oligopolistic Inflation. Journal of Business January, 35.1: 1-13.

___ 1962c. Henry L. Moore and Statistical Economics. Econometrica January, 30.1: 1-21.

___ 1962d. Comment on the Chicago School. Journal of Political Economy 70, February: 70-71.

___ 1962e. Foreword. In Gort.

___ 1963a. Archibald versus Chicago. Review of Economics and Statistics 30: 63-64.

___ 1963b. The Intellectual and the Market Place. Free Press: London.

___ 1963c. Policies for Growth. In Proceedings of a Symposium on Economic Growth. Washington: American Bankers Association.

___ 1964a. Comment. Journal of Business October, 37: 414-22.

___ 1964b. Competition and Concentration: Interview. Challenge January: 18-21.

___ 1964c. Comment. Journal of Business 37: 82-3.

___ 1965a. The Economist and the State. American Economic Review 55, March: 1-18.

___ 1965b. Essays in the History of Economics. Chicago: University of Chicago.

- ___ 1965c. The Problem of the Negro. New Guard December: 11-12.
- ___ 1965d. The Formation of Economic Policy. In Current Problems in Political Economy. Paul L. Morrison Lectures in Political Economy. Depauw University: Greencastle, Indiana.
- ___ 1966a. The Theory of Price. London: Macmillan.
- ___ 1966b. Report. In Shultz and Aliber eds.
- ___ 1966c. Review of Meade's *The Stationary Economy*. Economica November: 479.
- ___ 1967a. The Foundations and Economics. In Weaver ed.
- ___ 1967b. Alice in Fundland. Barron's February 27: 8-9, 12.
- ___ 1967c. The Changing Problem of Oligopoly. Il Politico 32: 355-61.
- ___ 1967d. Galbraith's New Book: A Few Problems. Wall Street Journal 26 June.
- ___ 1968a. The Organisation of Industry. Irwin: Homewood, Illinois.
- ___ 1968b. Competition. In Sills ed.
- ___ 1969a. Hearings Before the House of Representatives Select Committee on Small Business. Washington: US Government Printing Office. Volume 1.
- ___ 1969b. Text of Report of Nixon Task Force on Productivity and Competition. Antitrust and Trade Regulation Report No. 413: Washington.
- ___ 1969c. Does Economics Have a Useful Past? History of Political Economy: 217-230.
- ___ 1970. Review of Robbins' *The Evolution of Modern Economic Theory*. Economica November: 425-6.
- ___ 1971. The Theory of Economic Regulation. Bell Journal of Economics and Management Sciences 1: 3-21.
- ___ 1972a. Economic Competition and Political Competition. Public Choice: 91-106.
- ___ 1972b. The Law and Economics of Public Policy: A Plea to the Scholars. Journal of Legal Studies VI: 1-12.
- ___ 1973a. Professors Suppressing Freedom? Compact December: 15-17.
- ___ 1973b. Frank Knight as Teacher. Journal of Political Economy 81, May/June: 518-520.
- ___ 1974. Free Riders and Collective Action: an Appendix to Theories of Economic Regulation. Bell Journal of Economics and Management Sciences 5, Autumn: 359-365.

- ___ 1975. *The Intellectual and his Society*. In Selden ed.
- ___ 1976a. Do Economists Matter? Southern Economic Journal 42.3, January: 347-354.
- ___ 1976b. The Size of Legislatures. Journal of Legal Studies 5: 17-34.
- ___ 1977. The Conference Handbook. Journal of Political Economy 85.2: 441-3.
- ___ 1978. The Literature of Economics: The Case of the Kinked Oligopoly Demand Curve. Economic Inquiry 16.2, April: 185-204.
- ___ 1980. Occupational Licensure for Economists? In Rottenberg ed.
- ___ 1981. Comment. In Fromm ed.
- ___ 1982a. The Economist as Preacher. Chicago: University of Chicago Press.
- ___ 1982b. Review of Reid's *The Kinked Demand Curve Analysis of Oligopoly*. Economic Journal 92.365, March:203-4.
- ___ 1982c. The Pleasures and Pains of Modern Capitalism. London: IEA..
- ___ 1983a. Nobel Lecture: The Process and Progress of Economics. Journal of Political Economy 91.4: 529-545.
- ___ 1983b. Review of *Papers and Correspondence of William Stanley Jevons*. Economic Journal 93.370, June: 415-7.
- ___ 1983c. In Kitch ed.
- ___ 1984. Economics - The Imperial Science. Scandinavian Journal of Economics 86.3: 303-313.
- ___ 1985. The Origins of the Sherman Act. Journal of Legal Studies XIV, January: 1-12.
- ___ 1986. The Essence of Stigler. Hoover Institution Press: Stanford, California. Edited by Kurt Leube and Thomas Gale Moore.
- ___ 1987a. Are we hissing the wrong guys? Forbes 13 July: 52-6.
- ___ 1987b. Frank Hyneman Knight. In Eatwell *et al.* eds.
- ___ 1988a. Memoirs of an Unregulated Economist. New York: Basic Books.
- ___ 1988b. In Breit and Spencer eds. Reprint of 1 ed 1986.
- ___ 1988c. The Effect of Government on Economic Efficiency. Business Economics January: 7-13.
- ___ 1988d. Economists and Public Policy. In American Enterprise Institute.

___ 1989. Adam Smith and Public Choice: A Reply to Anderson. History of Political Economy 21.4: 659-660.

___ 1990a. The Place of Marshall's *Principles* in the Development of Economics. In Whitaker ed.

___ 1990b. Ricardo or Hollander? Oxford Economic Papers 42: 765-8.

___ 1991. The Direction of Economic Research. In Shaw ed.

___ 1992. Law or Economics? Journal of Law and Economics XXXV, October: 455-468.

Stigler, G. and Friedland, C. 1962. What Can Regulators Regulate? Journal of Law and Economics V, October: 1-16.

___ 1975. The Citation Practices of Doctorates in Economics. Journal of Political Economy 83.3: 477-507.

___ 1979. The Pattern of Citation Practices in Economics. History of Political Economy 11.1: 1-20.

___ 1983. The Literature of Economics: The Case of Berle and Means. Journal of Law and Economics XXVI, June: 237-268.

Stigler, G. and Kindahl, J. K. 1973. Industrial Prices, as Administered by Dr. Means. American Economic Review 63.4, September: 717-721.

Stigler, G. and Sherwin, R. A. 1985. The Extent of the Market. Journal of Law and Economics XXVIII, October: 555-585.

Stigler, S. 1994. Some Correspondence on Methodology Between Friedman and Wilson, November-December 1946. Journal of Economic Literature XXXII, September: 1197-1203.

Sutton, J. 1989. Is Imperfect Competition Empirically Empty. In Feiwel ed.

Telser, L. G. 1962a. Advertising and Cigarettes. Journal of Political Economy: 470-499.

___ 1962b. The Demand for Branded Goods as Estimated from Consumer Panel Data. Review of Economics and Statistics August: 300-324.

___ 1964. Advertising and Competition. Journal of Political Economy LXXII.6, December: 537-562.

___ 1971. Competition, Collusion and Game Theory. London: Macmillan.

___ 1996. Stigler, George Joseph (1911-1991). Mimeo.

Tinbergen, J. 1967. Quantitative Economics, Macroeconomics, and Monopolistic Competition. In Kuenne ed.

Triffin, R. 1940. Monopolistic Competition and General Equilibrium Theory. Cambridge: Harvard UP.

Vining, R. 1959. The Affluent Society: A Review Article. American Economic Review: 112-119.

Wallis, W.A. 1949. Review of Lever's *Advertising and Economic Theory*. American Economic Review 558-9.

___1993. George J. Stigler: In Memorandum. Journal of Political Economy 101.5, October: 774-789.

Weaver, W. ed. 1967. U.S. Philanthropic Foundations: Their History, Structure, Management and Record. New York: Harper Row.

Whitaker, J. K. 1990. ed. Centenary Essays on Alfred Marshall. Cambridge: Cambridge University Press.

White, L. D. 1956. ed. The State of the Social Sciences. Chicago: University of Chicago Press.

Whyte, W. H. 1952. Is Anybody Listening? New York: Simon and Schuster.

Yordon, W. J. 1992. Stigler's Adaptable and Indivisible Plant and the Micro/Macro Schism. History of Political Economy 24.2: 456-470.