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The Debate Widens - Introduction

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Keynes, Chicago and Friedman

Chapter 13: The Debate Widens

Robert Leeson

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Shortly after Don Patinkin's initial assault on Milton Friedman, Thomas Humphrey (chapter 14 [1971], 12) highlighted the importance of the contributions ("overlooked by both Patinkin and Friedman") made to the quantity theory between 1930-50 by four non-Chicagoan economists: Carl Synder,ⁱ Lionel Edie,ⁱⁱ Lauchlin Currie and Clark Warburton. There are similarities between Friedman's version of the Chicago monetary tradition and Currie's *Supply and Control of Money in the United States* (1934). Also, Currie's (1962 [1934]) essay on 'The Failure of Monetary Policy to Prevent the Depression of 1929-32' interpreted the Great Depression as a Great Contraction in a manner which foreshadowed the later work by Friedman and Anna Schwartz (1963). Humphrey commented that "oddly enough, however, [Lloyd] Mints and Friedman do not seem to be aware of the extent to which their criticisms were anticipated by Currie, for they cite him infrequently". In the exchange that followed two further names were added to the list of overlooked quantity theorists: Arthur Margetⁱⁱⁱ and James Angell (Patinkin chapter 16 [1974], 28; Humphrey chapter 17 [1973], 462).^{iv} Both Patinkin and Humphrey expressed curiosity about these omissions. Currie (chapter 15 [1972]) provides an additional perspective on Humphrey's contribution in a note that is published here for the first time.

It seems unlikely that Friedman was unaware of Currie's April 1934 *Journal of Political Economy* (*JPE*) essay since it was reprinted, at Harry Johnson's insistence, in 1962 as one of the *JPE Landmarks in Political Economy*; the selection criteria of which included "impact upon professional economists".^v The editor of that volume noted that the load "borne by ... Milton Friedman" in the production of that volume had been "disproportionately heavy" (Hamilton 1962, ix-x). There are several references to Warburton's "important papers" in the Friedman and Schwartz (1963, 301, n2, 359, n71) *Monetary History*, but Currie's name is not listed in their bibliography. Indeed, Currie (correspondence 29 November 1972) informed Patinkin that he had been informed by Harry Johnson that Friedman had "nothing to do with the selection of my paper in the *Landmarks of Political Economy*".^{vi}

This introductory chapter contrasts Friedman's endorsement of Warburton as a pioneer monetarist and his initial reluctance to mention Currie and Angell in this context. It also examines some previously unpublished archival evidence highlighting the intensity of some of the passions engendered by this debate.

15.1 Warburton

Without specifically referring to his own work, Warburton (1963, 77) noted that the conclusions derived by Friedman and Schwartz (1963) "should come as no surprise to anyone who has surveyed the history of business-fluctuation theory". The "most

continuous” component of the existing literature was to attribute “a dominant causal” role to “misbehavior of the monetary and banking system”. Warburton (1952, 292-3) reported that “the most remarkable theory feature of contemporary business-fluctuation theory is the unanimity with which economists have ignored the timing and amplitude of changes in the quantity of money in the United States relative to changes in population, output, consumer spending, prices and employment”. The first three ‘villains’ in Warburton’s list were John Maynard Keynes, Alvin Hansen and Frank Knight. Mints and Henry Simons apparently approved of Warburton’s work. Mints recalled that (presumably in the 1930s) “I remember distinctly (I think!) that after a talk by Warburton at the University [of Chicago] both Simons and I were greatly pleased by what Warburton had said. I even gained the impression that Warburton was grateful for our approval” (cited by Patinkin chapter 46 [1981/1979], 284-5).

Warburton was a monetarist while Friedman was still a wartime Keynesian.^{vii} Friedman and Warburton contributed to the same wartime debate on the Inflationary Gap in the *American Economic Review (AER)*. Friedman criticized Walter Salant’s (1941) essay on ‘The Inflationary Gap: Meaning and Significance for Policy Making’. But when his *AER* critique was reprinted, Friedman (1953, 253, n2, 251, n) was obliged to add a footnote: “The next seven paragraphs and the subsequent material inclosed in brackets are additions to the article as originally published. As I trust the new material makes clear, the omission from that version of monetary effects is a serious error which is not excused but may perhaps be explained by the prevailing temper of the times”. This was “a serious error of omission”.

Warburton continued the debate with ‘Measuring the Inflationary Gap’ (1943) and ‘Monetary Expansion and the Inflationary Gap’ (1944). Warburton (1944, 303, 318, 323, 325-6) argued that the first ‘gap’ that required filling was “the gap between those economists who approach the problem of price inflation from analysis of the use of income and those who approach it from monetary theory and the analysis of monetary statistics ... Monetary theory has also long had an answer to the next question in the exploration of the causes of price fluctuations: How much pressure on prices is exerted by monetary expansion or monetary contraction? ... This commonsense view is an application to money of the general economic principle known as the law of supply and demand. The refinement of this view which has long been made by monetary theory is often spoken of as the quantity theory of money”. Warburton believed that his statistical analysis demonstrated that “These facts are in perfect accord with the quantity theory of money, defined as the belief that changes in the price level are primarily due to changes in the volume of money relative to the need for money ... Modern economists who observe the facts around them have no ... reason to disavow this old-fashioned theory”. For Warburton the “cause of the great depression of the early 1930s was monetary deficiency” and “the greater part of the amplitude of business fluctuations could be eliminated by ... monetary policy”.

Although Warburton was not a full member of the Chicago School, several of his articles appeared in Chicago journals.^{viii} The *JPE* published Warburton’s ‘The Trend of Savings’ (1935), ‘The Volume of Money and the Price Level Between the World Wars’ (1945),

‘Quantity and Frequency of Use of Money in the United States, 1919-45’ (1946), ‘Volume of Savings, Quantity of Money and Business Instability’ (1947) and ‘Money and Business Fluctuations in the Schumpeterian System’ (1953). Also, the *Journal of Business of the University of Chicago* published Warburton’s ‘Monetary Policy and Business Forecasting’ (1949) and his ‘Misplaced Emphasis in Contemporary Business-Fluctuation Theory’ (1952 [1946]). This later essay – “his best known and most important contribution” - was reprinted in the *AEA Readings in Monetary Theory* co-edited by Mints (Lutz and Mints 1952). According to Michael Bordo and Anna Schwartz (1987 [1979], 236-7) this essay provided evidence that Warburton “anticipated the Keynesian-monetarist debate of the 1960s by a decade or more”. Warburton’s (1952 [1946], 284, 317) purpose was to point out that “a far more potent force of economic instability in recent years, namely, erratic variations in the quantity of money, has been ignored” in Keynesian analysis. There would be no progress in business cycle theory without the “elimination of its misplaced emphasis on savings-investment relationships and a re-examination of the significance of changes in the money supply”.

Warburton (6 January 1968) wrote to Friedman to congratulate him on his AEA Presidential Address: “it was a very well-developed exposition of the view *both of us* [emphasis added] have reached from our studies of the factual record and the reasoning and hypotheses of our predecessors. I still find it amazing that the factual data available and the views of earlier economists on the role of money were so largely ignored during the ‘Keynesian revolution’ ... your eloquent statement of *our case* [emphasis added] before today’s professional audience, young and old, will bear fruits I am sure”.^{ix} Friedman (20 January 1966) informed Charles Golembe of the American Bankers Association that Warburton “is now, we are all glad to say, no longer so lonely in his intellectual companionship”.^x

From 1934 until his retirement in 1965, Warburton (1981, 293) was employed at the Federal Deposit Insurance Commission (FDIC) which was not, he recalled, an “academic center” (it was established in 1933 to prevent another collapse of the banking system).^{xi} In July 1951, Friedman wrote to Warburton urging him to write a “substantial monograph” so as to maximise the impact of his work. Warburton thought it was unwise for him to do so. Warburton (6 January 1968) recalled to Friedman that in 1955 John Black, the AEA president had invited him to participate in an AEA panel discussion on ‘The Monetary Role in Balanced Economic Growth’. Since 1953, Warburton had “been prohibited by FDIC from publishing articles or participating in public discussions on monetary problems or policies”. His employers led him to believe that his “job would be at stake” if he accepted Black’s invitation: “With real regret, I declined, deciding to play by the rules of the ‘game’ in which I was involved, but hoping to extract myself or that a new breeze would blow soon in Washington. The ban was in effect for several years” (see also Yeager 1981, 280; Cargill 1979, 445; Bordo and Schwartz 1987 [1979], 235; Steindl 1995, 160, n5, 159, n1).^{xii}

15.2 Currie

Unlike Warburton, Friedman did not initially embrace Currie as an intellectual predecessor.^{xiii} In his study of the causes of the Depression, Hans Neisser (1936, 116,

n14) interpreted Currie and Ralph Hawtrey as having placed the “responsibility entirely upon the credit policy of the Federal Reserve System”.^{xiv} In the 1930s, Currie’s work was well known to Chicago economists (Laidler chapter 33 [1993], 1089; Samuelson 1996, 150; Bach 1940, 76, n1; Reeve 1943). Jacob Viner befriended Currie whilst visiting Harvard in November 1932.^{xv} At Harvard in 1933, Erik Lundberg (1994 [1934], 62) learnt that the depression had been caused by “a lack of money rather than an abundance ... It was certainly stimulating though a sad bankruptcy in economic theory, to now learn the exact opposite of what I had learnt before ... Professor Currie was the most eager advocate of this theory”.^{xvi}

When Viner was asked by Henry Morgenthau, President Roosevelt’s Treasury Secretary to assemble a ‘monetary Brain Trust’, among his early recruits were Currie and Harry Dexter White (Sandilands 1990, 56-7; Rees 1973, 40; Laidler and Sandilands chapter 41 [2002]). The 1935 banking bill was described by James Warburg (1969, 2944) as “Curried Keynes”.^{xvii} When Herbert Stein (1995, 216) arrived in Washington in 1938 he regarded Currie as “the most important economist in Washington”. In the 1930s, Currie attempted to recruit both Friedman and Arthur F. Burns to the Federal Reserve (Currie 1978, 547; Steindl 1995, 73, n14; correspondence from Friedman 13 September 2000).

In his *The Quantity Theory of Money: A Critical Study of its Historical Development and Interpretation and a Restatement*, Hugo Hegeland (1969 [1951], 3) observed that “the interpretations of the quantity theory shows almost as many variations as the number of its adherents”.^{xviii} But Friedman (chapter 2 [1956], 15-7) expropriated the quantity theory for monetarist purposes: “the question arises what it means to say someone is or is not a ‘quantity theorist’”. Friedman then went on to delineate the set of “deep and fundamental” demarcation rules which had to be accepted before one could be treated as a member of the quantity theorist ‘crusade’.^{xix} In particular, Friedman asserted that the quantity theorist “accepts the empirical hypothesis that the demand for money is highly stable – more stable than functions such as the consumption function that are offered as alternative key relations”.

Roger Sandilands (1990, 38, 96, 55-6, 156-7) speculated that underpinning Friedman’s neglect of Currie might have been the view that “one cannot be a bona fide monetarist if one is also a New Dealer”. Currie recalled that “in the early days, the New Deal was in the nature of a crusade” (see also Green 1981, 231-2). Currie became the victim of another crusade. In testimony to the 1948 House Committee on Un-American Activities, the self-confessed Soviet agent, Elizabeth Bentley, stated that in 1935 she “met Communists, both in Columbia and downtown, and gradually my ideas began to change”. Although she had never met him, she named Currie as someone who had passed information to a wartime spy ring that reported to her during Currie’s tenure as adviser to President Roosevelt (Carr 1952, 90-1, 241). In 1948, Currie and his old Harvard friend Harry Dexter White were obliged to appear before the House Committee on Un-American Activities to defend themselves against her allegations (White died of a heart attack immediately after these proceedings). No charges were ever brought against Currie, and in 1949 he was chosen to head a World Bank mission to Colombia and later to help administer President Harry Truman’s “Point Four” development programme there

(Sandilands, 1990, 156). During the McCarthy era, however, an unjustified cloud of suspicion hung heavy over Currie and he chose to settle permanently in Colombia where he developed a highly successful career as a top-level presidential adviser and development economist, noted for his sustained work on monetary theory and policy that paralleled his 1930s work in the United States.

The *Chicago Tribune* was a primary vehicle for the dissemination of these jaundiced opinions about Currie. Friedman (1983, 178) calculated that by 1934 “close to a majority” of faculty and students within the social sciences at the University of Chicago were “either members of the Communist party or very close to it”. The *Chicago Tribune* fanned anti-communist flames and the Illinois State Senate established a committee to investigate subversive influences in the educational system (Schlesinger 1960, 604, 607, 529, 88, 94; Stigler 1988, 157; Ickes 1953, 368, 376).^{xx} In February 1950, Senator Joseph McCarthy’s “first important blow” against supposed communists in the State Department was reported the following day in only two newspapers, one of which was the *Chicago Tribune* (Buckley and Bozell 1954, 160, n, 50-1). Later, McCarthy received financial backing from Colonel Robert McCormick, the publisher of the *Chicago Tribune* (Revere 1959, 115).

The *Chicago Tribune* produced what Herbert Simon (1991, 121) regarded as a “thick stream of bile” in its battle to save what it regarded as the American way of life against the New Deal. According to Rexford Tugwell (1972, 169), the *Chicago Tribune* continued to print stories that were “straight Hoover. It might have been culled from the Memoirs”. In 1961, the *Chicago Tribune* described Currie as a Soviet spy “who is planning how the dollars provided by a country which has stripped him of citizenship are to be employed in Colombia. It will be surprising if President Kennedy doesn’t find out he has made an alliance for Communist progress in that country” (cited by Stormer 1964, 72).^{xxi} In reality, Currie was not involved in Kennedy’s Alliance for Progress. Indeed, in 1961, he traveled to Washington to enlist the support of White House adviser Walt Rostow for an alternative national urbanization programme for Colombia (Sandilands, 1990, 157).^{xxii}

In the introduction to the second edition of Currie’s *Supply and Control of Money in the United States*, Karl Brunner (1968) highlighted the many unacknowledged similarities between Currie’s work in the 1930s and subsequent monetary analysis. In 1966, as Brunner was preparing the reprint, Friedman informed him that “Currie is a fugitive from justice somewhere in South America” (cited by Sandilands 1990, 157; correspondence from Sandilands 15 March 2002). Citing Currie’s (1934) book, Anna Schwartz (1981, 6) explained that she and Friedman “did, of course, have at hand” a variety of measures of monetary variables for the period of the Great Depression. But Friedman did not initially acknowledge Currie’s contribution. Currie (29 November 1972) wrote to Patinkin: “I know that both Karl Brunner and Lowell Harriss wrote to Friedman over his lack of mention of my work in his book but I never heard that he replied. I suspect that he disapproves of me strongly”.^{xxiii} But Friedman made reparations: Currie (14 November 1979) wrote to Albert Lepawsky explaining that in the 1970s Friedman sent him a copy of *Monetary Statistics of the United States* (Friedman and Schwartz 1970) inscribed ‘to a

pioneer in the field'.^{xxiv} In addition, he has now issued a specific “mea culpa” (Laidler chapter 33 [1993], 1077, n12).

15.3 Angell

In launching the monetarist counter-revolution, Friedman characterised Chicago as the early intellectual home of the modern quantity theory. He spent his second graduate year (1933-4) at Columbia: “the early intellectual home of the New Deal” (Adolf Berle cited by Schlesinger 1960, 393). At Columbia, Friedman was taught by Angell, who served on the front line of Roosevelt’s brains trust as a monetary expert and speechwriter (Moley 1939, 15, 18, 22; Berle 1973, 51, 32, 45, 50-1).^{xxv} Angell was one of the *Men Around the President* (Alsop and Kintner 1939, 21).

The policy conclusions in Angell’s *The Behaviour of Money: Exploratory Studies* (1936) appear to have a monetarist ring about them (Lee and Wellington 1984; Milgate and Levy 1987; Steindl 1995). Angell (1936, 144-5, 161-4, 61, 159) sought to provide the groundwork to “test” the relevant parts of existing theories. He concluded that the circular velocity had been very stable between 1909-30, but had fallen after 1930. Thus the large increase in national income prior to 1930 “must have been chiefly associated with increases in the money stock”. Although Angell doubted that there was a close relation between the quantity of deposits and the level of prices, he opposed the counter-cyclical manipulation of the money supply. He concluded that “the most effective procedure is to stabilise the quantity of money itself” allowing it to change “only gradually and evenly”. Although he was wary of dogmatic conclusions about causality, he suggested that an increase in business activity would cause “a subsequent expansion of (particularly) deposits, which our type of banking system permits and usually encourages, will in turn support or even induce a further increase in business activity. A rising spiral of mutually aggravating actions and reactions may thus be set up, which may persist for a considerable time”.^{xxvi}

Angell (1933a, 225, 207) used the quantity theory to advance the proposition that the principal cause of unemployment was “excessive variations in the volume of bank credit”. Angell also prefaced his analysis with a statement about his preference for “planned economies ... With these proposals I have great sympathy, and I think the adoption of almost any one of them would be an improvement over our present forms of business organisation”. Angell (1933b, 56, 70) also explained that “a solution through explicit socialisation or collectivisation may be debarred here by hypothesis” because “intelligent systems of planning” were too far in advance of American opinion: “In a society where laws and the prevailing social and economic philosophy make deliberate control easier than it is now in the United States, this type of planning and the monetary measures outlined above would presumably go hand-in-hand”.

In his *Memoirs*, Friedman recalled that he thought that he had attended Angell’s course on international economics, which he found to be “most valuable” (Friedman and Friedman 1998, 44, 46).^{xxvii} In a letter to Patinkin (19 July 1972) Friedman explained that the reason he had subsequently paid “little or no attention” to Angell was “very simple: his theoretical work in my opinion was very pedestrian and superficial. Let me

say that I was a graduate student Columbia in the year 1933-34 and took Angell's course. I started to dictate that I took his course in monetary analysis, but I'm not sure whether it was that which I took or his course in international theory which he also gave. In any event I recall very well that I was unenthusiastic to put it mildly – contemptuous would be a better word – at the theoretical level of the analysis. The same thing goes for Angell's [1936] book. I believe the theoretical analysis in it adds very little to our understanding. It is a highly mechanical approach. On the other hand Angell was responsible for a good deal of empirical work. He was very much a pioneer in that area".^{xxviii}

Friedman's first recollection was more accurate than his second: in spring semester 1934 he attended Angell's "Economics 128 Currency and Credit".^{xxix} Thirty of Friedman's forty-six pages of lecture notes from Angell's course relate to Keynes' *Treatise*. This section of the course began with "Keynes: 'Treatise on Money' great 'tour de force' says Angell". Friedman may well have been "contemptuous" because he had already been thoroughly exposed to the *Treatise* in Chicago the previous year in Mints' Economics 330 (see chapter 54 below). Perhaps there were other factors, too. George Tavlas (chapter 34 [1997], 173) recounts the information (provided to him by Peter Kenen and Benjamin Cohen) that by the 1950s, Angell devoted much of his teaching to defending the *General Theory* against the Keynesian Neoclassical Synthesis.^{xxx}

15.3 Stigler

In addition to the Patinkin-Humphrey exchange there were eight other occasions in which pairs of economists debated the validity of Friedman's claim. The original pair were Friedman (chapter 2 [1956]) and Patinkin (chapter 5 [1969/1981]). A second bout was arranged between Friedman (chapter 7 [1972/1974]) and Patinkin (chapter 6 [1972/1974]) in a *JPE* symposium. In the third and fourth bouts George Tavlas engaged J. Ronnie Davis and Tom Cate in the *Southern Economic Journal* (chapter 27 [1979]; chapter 26 [1979]; chapter 29 [1981]; chapter 28 [1981]). The fifth bout occurred in the *JMCB* in February 1986 between Michael Parkin (chapter 18 [1986]) and Patinkin (chapter 19 [1986]).

The Parkin-Patinkin exchange illustrates some of the cross cutting cleavages generated by the dispute over the Chicago tradition. Parkin and David Laidler are fellow monetarists who estimated the 'Demand for Money in the United Kingdom, 1956-1967' (Laidler and Parkin 1970). Their establishment of the Manchester University Inflation Workshop in July 1971 contributed to the monetarist explanation of inflation (Congdon 1978, 20). They joined the University of Western Ontario, where Patinkin held a visiting appointment and where Johnson was a regular visitor. Simultaneously, Parkin (1986) implicitly used the Chicago "sticky price" oral tradition located by Patinkin and Stanley Fischer (chapter 5 [1969/1981]) to analyse the output-inflation trade-off. This type of research became an important component of the New Keynesian response to the New Classical third generation Chicago School version of Simons' "rules party" (Fischer 1977; Taylor 1979; Mankiw 1985; Akerlof and Yellen 1985).

Parkin (chapter 18 [1986], 106-7) interpreted Patinkin (chapter 44 [1974], 4) as having defined Keynesian monetary theory as being primarily concerned with “details concerning the demand for money function ... a direct outgrowth of the *Treatise*”. Parkin noted that between 1917-30, A.C. Pigou, R.G. Hawtrey, Edward Cannan, Lionel Edie and Cyril James all appeared to recognize that the quantity theory could be formulated in terms of the demand for money and that “such a formulation was explicitly a portfolio analysis – an analysis of substitution between money and goods”. Moreover, Simons, Mints and Knight appeared also to be in this tradition. Thus “Friedman’s identification of the quantity theory as being first and foremost a theory of the demand for money is entirely in line with the writings of all the major quantity theorists of the 1930s”.

Parkin examined Patinkin’s method of historical research and found that he has fallen into the “dangerous pitfall” which he cautioned others about: that of “reading things out of context”. In particular, Patinkin “misquoted and misunderstood” Friedman’s reasoning. In reply, Patinkin (chapter 19 [1986]) argued that Parkin had not distinguished between a writer’s “basic analytical framework” and occasional references to other frameworks: “Parkin is repeatedly guilty of the basic methodological fallacy of reading meaning into texts”. Moreover, Parkin had not, as purported, written a review of *Essays On and In the Chicago Tradition*, but instead after seventeen years provided a “belated criticism” of “my 1969 article” (chapter 5 [1981/1969]).^{xxxii}

Following Patinkin and Parkin, the next combatants were Laidler (chapter 33 [1993]) and George Tavlas (chapter 34 [1997]) in the *JPE*. Following the commonly accepted rules of scholarly engagement, the editors of the *JPE* offered Laidler a brief right of reply. This constraint (a two page rejoinder) was, however, ill suited to the nature of the evidence in dispute and so a seventh bout was arranged in the *Journal of Economic Studies* (Laidler chapters 36 and 38 [1998]; Tavlas chapter 37 [1998]).^{xxxiii} This was followed by a further exchange of letters in the *Journal of Economic Perspectives* (chapters 39 and 40 [1999]). Patinkin was Friedman’s major critic; now Tavlas emerged to defend Friedman’s assertion.^{xxxiii} It seems unlikely that Laidler’s further contribution with Sandilands (chapters 41 [2002] and 42 [1932]) will pass without a rejoinder.

Yet there is an exception to this sequence of seven pairs: when the *JPE* published Frank Steindl’s (chapter 21 [1990]) essay on ‘The “Oral Tradition” at Chicago in the 1930s’ without Patinkin’s brief rejoinder, which is published here for the first time (chapter 22 [1991]). Stigler was the *JPE* editor dealing with both Steindl’s essay and Patinkin’s rejoinder. The archival evidence cast some light on the passions engendered by the dispute over the authentic Chicago lineage.^{xxxiv}

Steindl (chapter 21 [1990], 430-1) briefly summarised Patinkin’s accusations and then posed the question: “Was there an oral tradition of the quantity theory at Chicago or not? One important piece of evidence in support of such a tradition is Henry Simons’ enthusiastic review (chapter 20 [1935]) of Lauchlin Currie’s *Supply and Control of Money in the United States* (1934)”. Steindl continued: “the quantity theory is an essential component of Currie’s analysis ... The existence of a Chicago oral tradition can

be seen in Simons' four page review, which begins 'This book should have a significant and salutary effect, both on professional opinion and on college teaching. It ... expounds clearly a set of views which, while firmly established in the 'oral tradition' of some schools [leaving little doubt that Chicago is one of them], are [*sic*] meagerly represented in the accessible literature''.

The plurality of the "schools" mentioned by Simons suggests a lack of uniqueness.^{xxxv} Indeed, Currie (1934, ix) explained that "most of the subject matter" of his book had "been taught [by Currie] in the first half of the money and banking courses at Harvard for some years". Nevertheless, Steindl's evidence was relevant to the debate and deserved to be published. Tavlas (chapter 35 [1998], 215) regarded it as a significant, possibly even a clinching contribution. Patinkin's (chapter 22 [1991]) response was that Steindl's evidence failed to support Friedman's propositions about the *specific* contents of the oral tradition.

According to Patinkin (chapter 5 [1969/1981], 249) the dispute over the oral tradition did not revolve around fallible memories since he, unlike his rival, had the "concrete evidence" of lecture notes. Patinkin also took his stand not only on the basis of his superior mastery of the relevant historical literature. Patinkin (chapter 16 [1973/1981], 283) referred to Simons' 1935 review of Currie's *Supply and Control of Money in the United States*; he left no doubt that he was claiming to have exhaustively searched the Simons literature. But Steindl appeared to have resolved the Patinkin-Friedman dispute in favour of Friedman, using evidence that Patinkin must have been aware of. This must have been especially stinging for Patinkin (chapter 6 [1972/1974], 112) given that his accusation against Friedman was that he had "ignored the detailed evidence which has been adduced against the views he expresses" and had "indulged in casual empiricism in the attempt to support his doctrinal interpretations". Friedman (chapter 7 [1972/1974], 177) believed he had refuted Patinkin's claims, and in return accused him of "careless textual interpretation".

Stigler had a "mordant" sense of humour (McCann and Perlman 1993, 994, 1012); and Patinkin believed that Stigler was attempting to ensure that Friedman got the last laugh with respect to the Chicago monetary tradition. Patinkin (to Stigler, 31 January 1990) was cross with Stigler for not reviewing Thomas K. Rymes' (1988) *Keynes's Lectures, 1932-35: Notes of a Representative Student*; but he was livid when the *JPE* published Steindl's note. In his reply, Patinkin (chapter 22 [1991]) stated that Steindl's note completely misrepresented the debate. He had never denied the existence of a Chicago monetary oral tradition; it was Friedman's interpretation of that tradition that was the subject at issue.^{xxxvi} In correspondence, Patinkin (18 June 1991) reminded Stigler of sentiments that he (Patinkin) had expressed in an earlier letter, about the "fallacious nature" and the "demonstrably false statements" contained in Steindl's essay. Stigler had replied to Patinkin, apparently sympathetically, bemoaning the "shocking ... lack of care of people in reading and reporting on the literature".^{xxxvii} It seems likely that Patinkin interpreted Stigler's letter as an indication that his rejoinder to Steindl would be sympathetically considered. But Stigler, who apparently accepted Steindl's note without subjecting it to the usual refereeing process, unceremoniously rejected Patinkin's reply.

Friedman (correspondence to the author, 19 September 1997) recalls that he “was very appreciative of Steindl’s piece”. Patinkin was certainly not and was outraged by Stigler’s behaviour. He insisted that Stigler reconsider the matter with his three co-editors. But Stigler (6 August 1991) replied that he and his fellow *JPE* editors regarded Patinkin’s rejoinder as “simply too unimportant to be published”.^{xxxviii}

The Patinkin-Stigler relationship was different from the Patinkin-Friedman relationship. In particular, Stigler, unlike Friedman, was an historian of economic thought. In this context Stigler (3 July 1972) advised Patinkin that in his research on Chicago monetary history he was “as mistaken as you can be”. Stigler appeared to be pulling rank in his efforts to discourage Patinkin: “As a historian of economics I have come more and more to view science as a social institution and process, not as intellectual acrobatics by individual good and bad guys. I commend that viewpoint to you”.^{xxxix}

Patinkin spent autumn 1972 at Chicago where he pursued his interest in what he regarded as the authentic Chicago tradition.^{xi} He could have used the opportunity to launch a parallel attack on Stigler’s version of the Chicago tradition. “The Chicago School of Anti-Monopolistic Competition” was first explicitly defined and described by Edward Chamberlin (1957, 296). According to Stigler (1988, 150), only then did economists begin to refer to Chicago as a “School”.^{xii} Chamberlin (1957, 13-5, 17, 26, 300, 305, 24, 43, 70-91, 226-249, 305-6) sought to direct microeconomists *Towards a More General Theory of Value*. He felt himself to be confronted by the “right wing orthodoxy” of the Chicago School who “cling desperately to perfect competition” and 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”. Chamberlin sought to overcome this “heavy” legacy by reformulating his theory to assist the process of measurement. He concluded that his hypothesis had come into existence following the ‘classical’ scientific process outlined in Friedman’s (1953) ‘Methodology of Positive Economics’.

With respect to this second generation Chicago School methodology, Patinkin accused Friedman and Stigler of inconsistency, if not hypocrisy. In particular, Stigler’s approach to empirical work “sent shudders” through him (cited by Leeson [1998] chapter 11). In 1963, Friedman and Stigler stood accused on identical grounds by refusing to debate the empirical merits of the theory of monopolistic competition. Patinkin apparently took a keen interest in this exchange between Christopher Archibald (1961; 1963) and Friedman (1963) and Stigler (1963). Stigler (1963, 63) ridiculed Archibald’s discussion of Chicago as a “detour ... The methodological discussion is a detour on the detour”. In ‘Reply to Chicago’ Archibald (1963, 69) repeated 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”.^{xlii}

Patinkin and Stigler presented differing assessments about the extent to which monopolistic competition had been discussed in Chicago. Citing this exchange between

Chicago and its critics, and using his 1942 Economics 301 class notes, Patinkin (1981 [1973], 31) concluded that Knight had discussed monopolistic competition and had referred his students to the relevant readings “to an extent greater than one might infer from some of the things that have been written on the attitude of the Chicago school to this theory”.^{xliii} This contradicted Stigler’s view, which was that Knight (his PhD supervisor) “devoted even less time than compliments to Chamberlin so students had to read the book on their own” (Stigler and Friedland 1975, 497, n23).^{xliiv} But for whatever reason, this skirmish with Stigler did not escalate as had his private skirmish with Friedman.

Both Patinkin and Stigler were stern ‘gate-keepers’ of their respective disciplines. With respect to the Keynesian literature, Patinkin had a well-deserved reputation for rejecting an astonishingly high proportion of essays that he was asked to referee. Between 1976-95, Patinkin wrote 69 referees reports for the *History of Political Economy*; 56 he rejected outright, 3 he accepted outright (Saunders 2001).^{xliv} Referring to “committee meetings and journal editing”, Stigler (1969, 229) argued that it was both “true, and necessary to their survival” that “learned bodies are each run by a self perpetuating inner clique”. Citing *The Devil’s Dictionary*, Stigler defined an incumbent as “A person of the liveliest interest to the outcumbents”. Referring to the dispute over the oral tradition, Stigler (1988, 153-4) pointed out that Friedman used the quantity theory “as a powerful weapon to attack the Keynesian theory” and was “quite talented in outraging his intellectual opponents”. Stigler outraged Patinkin by refusing to publish his rejoinder to Steindl.

On another occasion Stigler (2 April 1979) had advised Patinkin: “Why be so polite?”^{xlvi} Certainly, Stigler could not be accused of handling Patinkin with excessive tact over his rejoinder to Steindl. In a letter to all four editors, Patinkin maintained his rage about Steindl’s “egregious misinterpretation of this disagreement”. It was, he explained, troubling to see the *JPE* “with which for obvious reasons I feel a close connection - fail to fulfill its obligations as a scholarly journal”. Patinkin’s letter was dated 19 September 1991, but Stigler’s death (1 December 1991) did not diminish his anger. Samuelson (12 February 1992) tried to console him with similar stories about Stigler’s behaviour. To which Patinkin (6 March 1992) replied that “In my case, it was even worse ...”.^{xlvii}

Patinkin proceeded to question Stigler’s credentials as an historian.^{xlviii} He informed Laidler (correspondence 9 October 1994) that Friedman’s attempt to defend himself against the criticisms contained in the original *JMCB* essay “by expounding at length on the Chicago advocacy in the 1930s of an activist monetary and [sic] policy” was irrelevant because the point was “never in question”. Patinkin continued: “Unfortunately – for we would expect that a leading historian of thought would base his views on a careful reading of texts – Stigler in his autobiography attempts to defend Friedman with the same irrelevant argument”.^{xlix}

However, despite the relatively high heat-to-light ratio, the debate over the Chicago tradition revealed a vibrant sub-culture in the economics profession. Patinkin, Johnson (and Stigler) were pre-eminent theorists, fully aware of the important role that history can

play in the analysis of contemporary issues (Laidler 2002).¹ The formalist revolution has tended to leech this “dynamic framework” out of economic analysis (Leeson 2001). A quarter of a century ago, Lionel Robbins (1976, 39) himself a pre-eminent theorist, denigrated “the extraordinary provincialism in time of much contemporary professional literature” – a trend which has continued. As a result many contemporary theorists (many of whom are historically untrained) typically deny their students an opportunity to acquire historical erudition by denigrating the subject and by deleting history of thought courses (Rosen 1993, 811).ⁱⁱ History continues to be used by theorists as a potent rhetorical weapon – a rhetorical flourish sustained by an intellectual monopoly (Laidler 2002). Moreover, the formalist revolution has begun to devour its own children: as the demand to study economics falls, so the supply of academic positions dries up.

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NOTES

i Frank Knight would not have appreciated Carl Snyder's (1927) *Business cycles and business measurements: studies in quantitative economics*, with its emphasis on empirical economics and its frontispiece quotation from Lord Kelvin extolling the empirical approach. When in 1929 the University of Chicago Social Science Research Building was dedicated, the Committee of Symbolism chose as the building's inscription another quotation supposedly from Kelvin: "When you cannot measure your knowledge is meagre and unsatisfactory" (Cate 1956, 426-7). In his *JPE* analysis of 'Truth in Economics' Knight (1940, 18, n10, 30, n17) stated that Kelvin's dictum was a perversion of the social sciences: "misleading and pernicious ... In this field, the Kelvin dictum very largely means in practice 'if you cannot measure, measure anyhow!' ... To call averaging estimates, or guesses, measurement seems to merely embezzling a word for its prestige value". In contrast, Paul Douglas (1930, 4) used Kelvin's dictum to "remind ... objectors" of the importance of empirical work. For Douglas (1934, xii) this was an antidote to the "sterile shadow boxing which has characterised so much of dialectical economics". This dispute was part of the ongoing warfare between Douglas and Knight (and his disciples) that characterized Chicago in the 1930s (Leeson forthcoming).

ii Fisher (1933, 124) associated the idea that the supply of money should grow at 3% per year (in line with the growth of trade) with Snyder, Edie and James Harvey Rogers.

iii Friedman was at the University of Minnesota in the year before his return to Chicago; as was Stigler from 1938-46, with a three-year break (Stigler 1988, 39-40; Friedman and Friedman 1998, 148-9). Marget (1938), one of their Minnesota colleagues, published a two-volume 'monetarist' *Theory of Prices: a Re-examination of the Central Problems of Monetary Theory*. In the *AER* review of 'Monetary Theory at the Textbook Level' Marget (1942, 781, 787) noted that it "can hardly escape even those who will not go beyond the titles of the chapters" that the authors of the textbooks under review had "unashamedly accepted ... the '*equation of exchange*' [emphasis in original] as an organising device" and were "unrepentant users of the Fisherine equations as their formal framework". The two books reviewed by Marget were Harold Reed's (1942) *Money, Currency and Banking* and George Halm's (1942) *Monetary Theory: A Modern Treatment of the Essentials of Money and Banking*. Friedman used Reed's (1942) text for his Economics 230 course at Chicago and a number of Reed's judgments "align remarkably closely" with the some of Friedman's macroeconomic judgments (Hammond 1999, 462-3). Referring to the word "money" Reed (1950, 210) explained that he "agree[d] with Mr Homer Jones that the word should be dropped from scientific terminology". Currie (1933, 79) suggested that "the continued use of the term 'credit' appears to be an obstacle both to the advancement of monetary science and its application

to current problems”. One alternative was to “drop the word entirely on the grounds that its ambiguity renders it unsuitable for scientific purposes”.

^{iv} Angell’s father, James R. Angell, was President of Yale University, and was described by William O. Douglas (1974, 164) as “broad-gauged, tolerant and fastidious when it came to academic freedom”. Angell and Nicholas Murray Butler, the President of Columbia, received unfavourable comment in President Roosevelt’s ([1937] 1950, 650) private correspondence for “howling their heads off” against the new income and inheritance taxes which they believed would dry up the flow of new contributions to the universities. After the First World War, Angell Sr. as Acting President of Chicago, recruited the twenty-four year old Robert Hutchins (the future President of Chicago) as University Secretary (Reagan 1982, 221; *Time* 1967, 183). James W. Angell was born in Chicago, graduated from Harvard, and spent 1919-20 as Viner’s teaching assistant at Chicago (Patinkin chapter 5 [1981], 267, 280, n). Angell taught at Columbia from 1924, as Full Professor, 1931-66, and Emeritus Professor after 1966 (American Men of Science 1968). He was vice president of the American Economic Association (1940) and was part of the US delegation to the Bretton Woods conference.

^v The *JPE* also published Currie’s (1933) ‘The Treatment of Credit in Contemporary Monetary Theory’.

^{vi} Don Patinkin Papers, Box 44.

^{vii} In ‘The Spending Tax as a Wartime Fiscal Measure’ Friedman (1943, 51, n3) explained that his analysis offered “no judgment ... on the critical issue separating the “Keynesians” and “anti Keynesians”.

^{viii} He obtained his doctorate on *The Economic Results of Prohibition* from Columbia (Warburton 1932).

^{ix} Milton Friedman Papers, Correspondence, Warburton file.

^x Milton Friedman Papers, Correspondence, Golembe file. In conversation with Warburton, Leland Yeager (1981, 280, 284) reflected on his “puzzlement” over the relative lack of influence that Warburton achieved. Academic “gamesmanship” was advanced as a potential explanatory factor.

^{xi} In the 1930s both Friedman’s wife and one of his mentors (Homer Jones) were employed in the Division of Research and Statistics within the FDIC (Friedman and Friedman 1998, 65).

^{xii} Milton Friedman Papers, Correspondence, Warburton file.

^{xiii} In contrast, Arthur Kemp (1979, 11), the Treasurer of the Mt Pelerin Society (1969-79), characterised Currie as “a classic figure among the early members of the Chicago

School”. Kemp does not explain where this information is derived from, but presumably he had recently been in close contact with Stigler, the 1976-8 President of the Mt Pelerin Society (Hartwell 1995, 78). At a 1977 seminar at the University of Toronto, John Scadding, the seminar chairman and a Chicago graduate, asked Currie what was the price of money. Currie replied “the price level”. “Congratulations!” shot back the chairman, “you pass the test” (correspondence from Sandilands 15 March 2002).

^{xiv} Neisser was Professor of Monetary Theory, at the Wharton School of Finance and Commerce, University of Pennsylvania.

^{xv} Friedman (chapter 7 [1974], 167) was “amazed” to discover how “precisely” Viner’s account of the Depression “foreshadows” the interpretation contained in his *Monetary History* (Friedman and Schwartz 1963).

^{xvi} Currie’s *Supply and Control of Money in the United States* (1934) was described in a Chicago Public Policy Pamphlet as “An extremely valuable analysis ... carefully and logically presented” (Whittlesey 1935, 25).

^{xvii} Warburg (1969, 2944) described the bill as “Curried Keynes” a “large half-cooked lump of J. Maynard Keynes ... liberally seasoned with a sauce prepared by Prof. Laughlin [sic] Currie” (see also Egbert 1967, 122).

^{xviii} Hegeland (1969 [1951], x, 96) spent two years in the United States researching his 1951 history, but reported no mention of a Chicago quantity theory tradition.

^{xix} Friedman (chapter 2 [1956], 16) stated that “The quantity theorist must sharply limit, and be prepared to specify explicitly, the variables that it is empirically important to include in this function ... the quantity theorist not only regards the demand for money function as stable; he also regards it as playing a vital role in determining variables that he regards as of great importance for the analysis of the economy as a whole ... The quantity theorist also holds that there are important factors affecting the supply of money that do not affect the demand for money”.

^{xx} The owner of the Chicago *Daily News*, Colonel Frank Knox, who had previously been general manager of Hearst’s newspapers, and was Alfred Landon’s Republican vice presidential running mate in 1936, declared that “the new deal candidate has been leading us towards Moscow”. During this 1930s “red scare”, Charles Walgreen, a prominent drug store owner, withdrew his niece from the University of Chicago believing that she had been exposed to the doctrines of free love and communism. Finally, Walgreen was persuaded to donate funds to establish the professorship at Chicago which Allen Wallis successfully offered to Stigler in 1958.

^{xxi} For a more balanced assessment, see Sandilands (2000) and Boughton and Sandilands (2002). Also see Sandilands’ online biography of Currie on American National biography On-line at www.anb.org.

^{xxii} I am most grateful to Roger Sandilands for supplying me with this information.

^{xxiii} Don Patinkin Papers, Box 44.

^{xxiv} Lauchlin Currie Papers, Correspondence, Lepawsky file.

^{xxv} “Jimmy Angell contributed ideas on prices and money” (Moley 1939, 15, 18, 22).

^{xxvi} In his review, Dennis Robertson’s (1937, 330) noted that Angell’s policy proposal was to keep “*national income* relatively stable ... by stabilising the quantity of circulating money” [emphasis in original].

^{xxvii} In the Friedman Papers there is a “Reading List in International Trade Economics 125-6 ... (Revised: 1933)”. A tick appears alongside a 1931 essay by Angell on ‘foreign exchange’ for the encyclopedia of the social sciences. However, there is no record on Friedman’s Columbia academic transcript of him attending this two-semester course. (At the bottom of his transcript three courses are listed as having been “visited”: social economics given by J.M. Clark, one in Labor given by Leo Wolman and one in theory given by R.W. Souter). Milton Friedman Papers, Box 5.

^{xxviii} Patinkin (6 August 1972) replied: “On Angell, I won’t argue with you about his theoretical abilities, (though a letter I received from Viner would seem to indicate a somewhat higher estimation). However, as I recall, Angell did emphasise one empirical finding (namely, that monetary changes followed those in prices, instead of preceding them) that I think should have been dealt with by the Chicago School”. Don Patinkin Papers, Box 32. In 1934, Angell and Currie engaged in dispute over their early separate estimates of the income velocity of money. Their estimates of the course of money’s “income velocity” in the 1920s differed widely and made for very different interpretations of Fed policy at that time. The dispute essentially revolved around their different definitions of “money”, Currie preferring a narrower, means-of-payments definition (see Sandilands, 1990, 41).

^{xxix} In Friedman’s Papers (Box 5) archives is a 10 page “Reading List in Money and Banking Economics 127-8 (J.W. Angell) Revised: 1933”. Section three of the course on “The General Theory of Money, Banking and Prices” has 72 books or articles listed for reading. Items marked “##” were described as “Required Reading, to be prepared for class-room discussion”. Among such items were Fisher’s “The Purchasing Power of Money (1911)”, Keynes’ “Monetary Reform (1924), pp. 1-95” and “Treaties [sic] on Money (2 vols., 1930)” and Angell’s “Theory of International Prices (1926), pp. 116-135, 178-186, 274-280, 308-312, 324-331”.

^{xxx} Alan D. Whitney (21 August 1976) recalled in a letter to Friedman that “Angell was a fascinating personality” (Milton Friedman Papers, Correspondence, Whitney file). While some detected misanthropic tendencies in Angell’s personality, others, such as Kenen (a

young departmental chair dealing with older and more senior colleagues) recalled that Angell was most helpful. Stigler (1988, 43) was Angell's Columbia colleague for eleven post war years but the only "oral ... legend" that he associated with Angell was his displeasure when Arthur F. Burns (then a PhD candidate) declined to answer a role-playing question about how he (as Treasury Secretary) would respond to a financial panic. Anna Schwartz sought to submit *A Monetary History of the United States* (Friedman and Schwartz 1963) as her Columbia PhD thesis. Burns, the Director of the National Bureau of Economic Research (NBER), may have contributed to the delay she experienced in having her dissertation approved by questioning whether her individual contribution to the Friedman-Schwartz NBER project was sufficient to warrant acceptance. If Angell was a difficult thesis committee member, this may have increased Friedman's contempt for Angell. On 22nd October 1958, Friedman (with a copy to Angell) wrote to Albert Hart at Columbia about "a matter that is strictly speaking on my business, namely, Anna Schwartz's thesis problem ... the report of her conversations with you and Angell suggest to me that, she must have presented the case very badly indeed ... it is incumbent on us to keep our requirements from being meaningless hurdles, and to adapt them to the particular situation ... the fundamental problem is how to satisfy formal requirements without imposing 'make-work'. If I may again cite our own experience, we have had the same problem here in the past few years in connection to other mature scholars, Homer Jones and Herbert Stein". Six years later Friedman (January 7th 1964) wrote to Barger explaining that he had tried to contact him by phone to "talk over with you the problems connected with Mrs Schwartz finally getting her degree. She tells me that there has been some question raised over using *A Monetary History* because it is in printed form and this limits the possibility of people on the committee making suggestions for change ... it would be a shame to let technicalities of any kind play an important role in the process ... I would much prefer to talk to you about this than to write about it because clearly I am very much misinformed and there is some misunderstanding about it on her part". Barger immediately (16th January 1964) wrote a sympathetic letter to Schwartz explaining that he did not know "how much trouble [the committee] may cause you about revisions" (her thesis was finally approved later in the year). Milton Friedman Papers, Correspondence, Schwartz file.

^{xxxi} Patinkin's original essay (chapter 5 [1969/1981]) was published thirteen years after Friedman's essay (chapter 2 [1956]).

^{xxxii} Sandilands suggested the *JES* as an outlet for Laidler's reply; Laidler suggested that Tavlas be invited to respond.

^{xxxiii} Patinkin (January 24, 1994) wrote to Sandilands to say how pleased he was "for more reasons than one" to see Laidler's (chapter 33 [1993]) essay in the *JPE*. Don Patinkin Papers, Box 66.

^{xxxiv} Not only was Patinkin a gatekeeper with respect to the journals, he played a similar role with respect to application for funding from research foundations. Patinkin did not support Steindl's request for financial assistance to support his monetary research.

^{xxxv} Another book review casts further doubt about Simons' supposed commitment to analysing the economy through the quantity theory. Edwin Nourse (1933, ix-x) of the Brookings Institution in his Director's preface to Leo Pasvolsky's *Current Monetary Issues* explained that Pasvolsky's objective was to present "a general survey of the monetary issues which have dominated world economic discussions during the past year. After showing the divergence of opinion between various groups and nations participating in international discussions and negotiations during the course of the year, it sets forth the trend of monetary developments in the United States". But Pasvolsky confined his attention to "the vital relationship of present monetary policies to economic recovery". Pasvolsky did not mention the quantity theory and neither did Simons in his February 1934 review. Instead, Simons (1934, 53) stated that he was "impressed throughout with his fairness and competence in the analysis of issues and in the interpretation of conflicting positions".

^{xxxvi} This was the position he had maintained in a letter to Steindl (7 August 1989). Don Patinkin Papers, Box 62.

^{xxxvii} I was unable to find Stigler's letter and so I am relying on Patinkin's report of it (to Stigler 18 June 1991). Don Patinkin Papers, Box 62.

^{xxxviii} Don Patinkin Papers, Box 62.

^{xxxix} Don Patinkin Papers, Box 37.

^{xi} In the resulting essay, Patinkin (1981 [1973], 40, n19) tentatively attributed to Knight the second generation Chicago tradition that "there is no such thing as a free lunch". Knight (1935 [1932], 251-276) wrote the entry on 'Interest' in the *Encyclopaedia of the Social Sciences*; Patinkin (1968) wrote the equivalent entry in the *Encyclopaedia* alongside Friedman's *Encyclopaedia* entry on the quantity theory (chapter 4 [1968]) which was the "nominal" reason for Patinkin's initial assault (chapter 5 [1969/1981]).

^{xli} In 'Comment on the Chicago School' Stigler (1962, 71) asserted that the title invited a "slovenly stereotype".

^{xlii} For a detailed discussion of this controversy, see Leeson 2000, chapter 3.

^{xliii} Patinkin (1981, 8) also attended Oskar Lange's 1944 Economics 307 lectures on Imperfect Competition and A.J. Nicholl's course on Imperfect Competition. In an article published while he was still at the University of Chicago, Patinkin (1981 [1947], 91, n) proposed to use empirical evidence to improve the theory of imperfect competition. Moreover, Patinkin acknowledged that "I cannot overemphasize my debt to Professor Henry C. Simons, on whose Economics 201 Syllabus ... this article is so largely based". Stigler (1974, 2-3) acknowledged that Patinkin had "elaborated" upon Simons' analysis of cartels.

^{xliv} Wallis' (1993, 776) recollections supported Stigler's: the Chicago faculty of the 1930s gave Chamberlin's work "no attention: literally none". However, Albert Hart's (1936, iii, 151) Chicago dissertation (supervised by Knight, Viner, Henry Schultz and Theodore Yntema) had been "very profoundly ... influenced" by the "recent writings on 'imperfect competition' (especially those of Mrs Robinson, and Drs. Hicks, Chamberlin and Kaldor)". Hart also recalled that Simons' and Aaron Director's undergraduate teaching material had been "freely cribbed by George Stigler and others". Paul Douglas (1972, 351, n) explained that he been "strongly influenced by Joan Robinson's *The Economics of Imperfect Competition* ... After nearly forty years I still think Mrs. Robinson's book was the best in my generation".

^{xlv} Patinkin took a very jaundiced view of a paper submitted to the *JPE* by one of his opponents in the dispute over the Chicago tradition. Patinkin (11 June 1982) informed Stigler, that with respect to his role as a referee "it was a serious misallocation of my resources to have spent so much time on a paper that is superficial, unscholarly and carelessly written – and whose rejection I accordingly unhesitatingly recommend". The report sent to the author appeared to be quite neutral – but the *JPE* editors were left in no doubt about Patinkin's determination to see the paper rejected: "Despite the concluding sentence of my report, I have strong doubts whether [the author] has the necessary scholarly qualifications for carrying out the proper study ... and so I do not think you should ask him to resubmit it after revision". Don Patinkin Papers, Box 51.

^{xlvi} Don Patinkin Papers, Box 48. According to one of his Chicago colleagues, verbal disputes with Stigler were "likely to be terminated by a positivist edict and a sneer" (McCloskey 1994, 14).

^{xlvii} Don Patinkin Papers, Box 61.

^{xlviii} On other occasions Patinkin appeared to resort to ridicule when challenged. Patinkin (chapter 19 [1986], 120) criticised Parkin for being "more Friedmanian than Friedman" because Friedman had "subsequently admitted" the Keynesian influence, but "there is barely a hint of this fact in Parkin's paper". Parkin (correspondence to the author, 24 October 1997) recalled that "Patinkin seemed angered and seemed to take my critique personally. He insulted me personally in his published response. I thought about rejoining but decided not to after talking with Milton". Laidler (correspondence to the author, 4 November 1997) recalled that "Patinkin was contemptuous of Parkin's *JMCB* piece". See also Patinkin's (1978, 577) patronising "plea for common sense" to counteract the basic illogical fallacies (christened "Jacob's Principle") which he detected in David Roberts' (1978) critique of his analysis of *Keynes' Monetary Thought* (1976).

^{xlix} Don Patinkin Papers, Box 57.

¹ In a review of the AEA *Survey of Contemporary Economics*, Stigler (1949, 101, 98, 102) noted that the collection "demonstrates anew that interest in the history of thought has languished mightily". This had impoverished the "scope and depth" of the essays in

the volume and also “contemporary economics” in general. This concerned Stigler because it was “easy to show that the neglect of doctrinal history has led the contributors to exaggerate the recent advances in theory”. Also, a “second characteristic of recent economics documented by the *Survey* is the triumph of statistics over history as the source of empirical knowledge”. This was lamentable because the “historical method ... has the obvious advantage of making available a long sweep of experience unrecorded in statistics. This alone is ample reason for exploiting it: we *are* [emphasis in original] interested in the long run. The historical method has also the advantage ... of yielding information on a much wider range of problems than one can illuminate with quantitative studies”. As Chicago lost its “outcumbent” status, Stigler’s views about the role of historical studies appeared to have changed. Stigler (1988, 214-5) concluded that “I cannot be confident that it would be profitable for a young scholar to study the history of his subject”. Stigler (1969, 217-8, 230) defined “the subject matter of the history of economics to be economics which is not read to master present day economics”. In other words, the history of thought was a largely redundant subject: “one need not read in the history of economics – that is, past economics – to master present economics”. Stigler predicted that “the young economist will increasingly share the view of the more advanced formal sciences that the history of the discipline is best left to those underendowed for fully professional work at the modern level ... it remains the unfulfilled task of the historians of economics to show that their subject is worth the cost”.

^{li} In 1972, Stigler successfully proposed that the history of economic thought requirement be dropped at the University of Chicago. Most other economics departments later followed suit (Rosen 1993, 811). Deirdre McCloskey (correspondence to the author 2 June 1997) recalls that at the same meeting Stigler unsuccessfully proposed that the economic history requirement also be dropped.