Johnson's Conversion from Keynesianism at Chicago

Russell S. Boyer*
Department of Economics
University of Western Ontario

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Abstract: “Johnson’s Conversion from Keynesianism at Chicago,” Russell S. Boyer, September 12, 2010

Johnson arrived at Chicago in 1959 identifying himself as a Keynesian, but during his period there he began to speak in derogatory terms about Keynes and about Keynesians. This paper analyzes the role that Friedman and Mundell played in this conversion. Our argument is that Johnson moved towards “the monetarist position,” but he denied that this was due to Friedman’s influence. Mundell’s thinking followed a similar path, both in his economics and in his assertion of independence from their distinguished colleague. The effect of these claims is to hide the fact that Friedman’s impact on their work was much greater than has generally been recognized.

1. Introduction

Harry Johnson's thinking concerning macroeconomic topics evolved markedly during the years 1959-1977, while he was at the University of Chicago. His move from Keynesianism towards "the monetarist position"[1] over this period has been noted by numerous observers, including such well-known economists as Bhagwati (1977) and Polak (2002).

What has not received sufficient attention is Johnson's (1977) claim that this evolution was independent of any influence from his distinguished colleague, Milton Friedman. Such a claim, although supported explicitly by Laidler (1984) and in a nuanced way by Leeson (2000) and Tobin (1979), runs counter to conventional thinking, because the usual view is that it was precisely such influence which caused Johnson dramatically to switch his allegiance between these rival camps in macroeconomic thought.

We argue here that the impact of Friedman's thinking on the work of both Johnson and Mundell was far greater than has generally been recognized. The basic themes of both the Optimum Currency Areas Argument, and the Monetary Approach to the Balance of Payments can be found in Friedman's celebrated essay, "The Case for Flexible Exchange Rates." (Friedman 1953) Since neither Johnson nor Mundell has acknowledged Friedman's contributions in these areas, his influence on their work has not received much attention. Given the extreme assumptions which typically go into the research which falls into these categories (zero capital mobility, purchasing power parity, full employment, and no sterilization), these rubrics, rather than being progressive research agendas,[2] are more usefully seen as having the effect of denying Friedman the credit that he is due for these ideas.

The Johnson-Mundell view is that in the aftermath of the Keynesian revolution, they restored monetary elements to a prominent position in open economy macro analysis.[3] The bulk of their work was done at the pre-eminent center for research in money and banking at the time, the Department of Economics at the University of Chicago, which was dominated by the world's foremost monetarist, Friedman. To claim, as Johnson (1977) and Mundell (1967b) do, that this work was carried out without influence from their distinguished colleague, is problematic, and thus the focus of the present paper.

2. Johnson's Transition from Keynesianism to the Monetarist Position

[1] Johnson (1972a, 5,7) defined "the monetarist position" as encompassing the general conclusions at which one arrives by applying "…commonplace… monetary theory…" This reasonable set of precepts is distinguished from "…monetarism…, the particular formulation of the monetarist position embodied in the past works of Milton Friedman…" (Johnson (1972a, 66)). As we show below, monetarism was rejected repeatedly, and in ever more strident terms, as Johnson reworked this material over a five-year period. He felt that monetarism was on the decline, whereas monetarist research by Mundell, although controversial, held out substantial promise (Johnson (1972b, 336)). Laidler's (1984, 603) claim that Johnson at times labeled himself "a 'monetarist'"appears to be of dubious validity.

[2] See Chipman (1980, 141-42) and Hahn (1977) for a similar point of view.

[3] See the narratives which appear in Frenkel and Johnson (1976) and in Obstfeld (2001).

Johnson arrived at the University of Chicago in 1959 as part of the "minority group," a set of members of the Department which acted as a counterweight to Friedman's dominant position in it. Johnson's strongly-held doctrinal preferences at that time have been remarked on by commentators of all ideological stripes, including Johnson himself (as is attested by the typescript, titled "A Keynesian Looks at Chicago," (Johnson 1960)[4] of a talk delivered to a cocktail party thrown by the Political Economy Club at 8:30pm, October 28, 1960, on the campus of the University of Chicago). So, for example, Bhagwati and Frenkel (1987, 357) note that Johnson was "…invited as a 'Keynesian' economist..." to take up the chair at the University of Chicago. This was an appointment which had been offered to (and briefly accepted, before being rejected, by) Samuelson, a move which aroused strong animosity from Friedman and Stigler (Hammond and Hammond (2005, 46)).

It appears that in Chicago's hiring at the time, personality characteristics played as big a role as policy adherences. The appointment rejected by Samuelson was accepted by the next economist to whom it was offered: Metzler. Despite the fact that he was just as staunch a Keynesian as was Samuelson, his hiring met little resistance. Tragically Metzler's full productivity was short-lived as an operation on a brain tumor in 1950 substantially diminished his mental capacity.

Johnson was hired in 1958, after Friedman campaigned for him starting in 1953. They had met first at Cambridge University in that year, and Johnson's warm welcome to Friedman contrasted with the frosty reception which everyone else extended to him.[5] Johnson and Friedman appear to have had cordial relations until at least 1957, when Friedman asked Johnson to contribute a book to the Cambridge Economic Handbooks series of which he was a co-editor (along with Claude Guillebaud). After this point, Harberger seems to have been Johnson's main contact and champion in the Department, and relations with Friedman became more problematic.

Moggridge (2008, 138) argues that Johnson took his "…first large step away from orthodox Cambridge Keynesianism…" (Laidler (1984, 598)) while he was still at Cambridge, when he cited with approval some quantity theory results. Friedman's year-long visit to Cambridge (1953-1954) or the quarter which Johnson spent at Northwestern University (April-June 1955) would have been obvious occasions on which Friedman could have exerted influence on Johnson. For example, in the spring of 1955 Johnson attended workshops at the University of Chicago on Thursday evenings. Nonetheless, Johnson continued to identify himself as a Keynesian at least until October 1960.

Johnson appears to have delighted in attacking authority figures, as is exemplified by his scalding assessments of Meade's (1951) monumental books on international trade and policy (Johnson (1951)). In writing these reviews as a twenty-eight-year old, Johnson had created quite

[4]Roughly a quarter of this typescript is excerpted in Moggridge (2008, 205-07). These pages include, in addition, paraphrasing of the rest of the paper, consideration of its contents, as well as an analysis of the reactions of various members of the Department and the Business School to this episode.
[5] This discussion follows the coverage of these matters by Moggridge (2008, 123-25).

a stir among the pillars of the establishment in the UK. Moggridge (2008, 139-45) deals with this episode and remarks both on the strains it caused there, and on the belated commentary which Mundell writes about it.[6]

After Johnson had accepted Chicago's offer, and before he had arrived on campus, he was ready to show Friedman that he would not defer to him. In an *Economic Journal* article, written jointly with Bhagwati, who claims that the relevant portion "was almost wholly Harry's contribution" (Bhagwati (1977, 224)), Johnson criticized a proof by Egon Sohmen supporting a claim by Friedman about stability in the foreign exchange market. Harry writes "…these sentences illustrate the extent to which *non sequitur* and *ignoratio elenchi* can be accepted as valid argument if the conclusion purportedly reached is of sufficient propaganda value." (Bhagwati and Johnson (1960, 93))

Friedman responded by telling Johnson "that he ought to be thoroughly ashamed of himself and that he owes you [Sohmen] an apology…" (Friedman 1960) Johnson had been well aware that his paper would cause a strong reaction. He had written to Harrod, concerning publication dates, that "I should be sorry to see it delayed as late as June 1960 for one particular reason which is that Bhagwati and I will both be at the University of Chicago for this coming year and the last part of the article is a pretty hard-hitting criticism of some strongly-held Chicago views. I should like the article to appear before he has left Chicago." (quoted in Moggridge (2008, 204))

Another opportunity for Johnson to criticize Friedman arose only five months later, when Johnson was asked to give a talk at a cocktail party thrown by the Political Economy Club at the University of Chicago, whose typescript is cited above. This document serves as an illustration both of the nature of the relationship between Johnson and Friedman at this point, as well as of Johnson's macroeconomic thinking. The presentation was viewed by the "minority group" (which included at the time Metzler, Downs, Johnson, and Harberger) as an attack on the current thinking among the majority of the members of the Department.[7] Therefore it was directed at Friedman and Stigler, and their followers. In contrast, the names mentioned in it are: Keynes, Veblen, Knight, Marx, Lange, and Metzler.

Further salient episodes in the relationship between Johnson and Friedman include the sniping portions of Johnson's (1965) review of Friedman and Schwartz (1963), and Johnson's Ely Lecture in December 1970 (Johnson 1971). In the latter publication Johnson questions the validity of Friedman's characterization of the development of monetary theory at the University of Chicago during the inter-war period. Specifically, Johnson reiterates many of the objections

[6] Compare Mundell (1968a, 113) with the earlier version of this paper in Adler (1967), where the reviews of Meade's books are not mentioned.
[7] See the letter from Downs in Johnson's Papers, Regenstein Library, University of Chicago (Box 13, Writing 1960, November 3, 1960).

which Patinkin (1969) had raised about Friedman's narrative.[8] Friedman's first reaction to this lecture was that it was libelous.[9]

These fragile personal relationships hide the fact that in his thinking Johnson was moving ever closer to Friedman's position. This was something that Friedman noticed, and about which he wrote "Harry was originally a very strong Keynesian who was converted to monetarism."[10] Thus in 1971 Johnson described the Monetary Approach to the Balance of Payments, for which he took substantial credit, as being a "monetarist approach;" that is, an application to open economy macro of insights derived from the monetarist position. By using the adjective "monetarist" Johnson is acknowledging what is quite apparent about this framework. Namely, that it is consistent with Friedman's thinking on macroeconomic topics. As becomes clear from Johnson's (1977) re-jigging of his nomenclature a few years later, he is treading very close to Friedman's turf, but is unwilling to acknowledge the debt that he owes to him.

Similarly, in a closed economy analysis, Johnson (1969a, 30) "covers much of the same ground" as Friedman's Optimum Quantity of Money Argument. Nonetheless, Johnson does not cite this essay in the body of his own paper because it "…was submitted for publication…[before he]…came across Milton Friedman's…" work. Since this paper (Friedman 1969) was available in manuscript form to graduate students in September 1967 (when it was used as a text for Friedman's teaching of Economics 330), it is quite surprising that Johnson had not become acquainted with it earlier.[11]

While the basic summary of the evolution of Johnson's thinking about macroeconomics during this period is quite simple and widely recognized, there are some dissenters. For example Tobin (1979, 454) says of these years that "The influence of Chicago on Johnson's thought is...hard to assess." More definite is Laidler's (1984, 593) assertion that "The evolution of Harry Johnson's macroeconomics had very little indeed to do with the fact that he held a chair at the University of Chicago." He comes to this conclusion looking at a narrow category of publications. This is in line with the commission which he had received from the *Journal of Political Economy*, which asked him to confine his attention to Johnson's closed economy research. Looking just at Johnson's published work, Laidler focuses on his views of the effectiveness of monetary policy, as captured by the Phillips Curve relationship. It is on the basis of this evidence that Laidler

[8] The relationship between Johnson's and Patinkin's work in this episode is transparent. Johnson is voicing, in stronger terms, the substantive points which were entirely due to Patinkin. We argue below that a similar relationship existed between Mundell and Johnson's work, but it is less apparent that Johnson is acting as spokesman for Mundell's somewhat derogatory view of Friedman's contributions. See Dornbusch (2001) for instances of such a view.
[9] See Moggridge (2008, 347).
[10] Letter from Friedman (1995), dated April 19, quoted in Leeson (2000, 746).
[11] Hynes (2001, 629) appears to accept Johnson's claim without question.

concludes that Johnson's macroeconomic thinking did not evolve due to influences at the University of Chicago.[12]

Tobin (1979, 453) has noted that "…Harry had troubles when, as in monetary theory, his interests intersected Friedman's," but then goes on to say that "In international economics he was able to carve out a rewarding independent role." Specifically, "His 'monetary theory of the balance of payments' commanded his attention in his last years… Harry regarded the new theory as his most important recent work…" (Tobin (1979, 456))

The import of Laidler's interpretation is unmistakable. Contrary to the view of the economists quoted above, his claim is the Friedman had a negligible impact on Johnson's thinking in terms of monetary economics. This is a claim of which Johnson would have very much approved, because it suggests that he was doing independent work, even if much of it was "well known at Chicago." (Laidler (1984, 592) This is the interpretation which Johnson and Mundell put forth, with ever greater conviction.

3. Friedman's Work and His Interaction with Johnson and Mundell

Friedman "clearly identified the revival of interest in money with himself." (Leeson (2000, 747)) This is, of course, the chief lesson that comes from the book he edited in 1956, *Studies in the Quantity Theory of Money* (Friedman 1956), which focuses on the macro behavior of closed economies. But his interest in the open economy, as well, is apparent from two of his major works, namely his celebrated essay (1953), and his *Monetary History* co-authored by Anna Schwartz (1963). These publications have a completely different view from Metzler (1948, 212), who describes the minor role assigned to monetary elements in balance of payments theories at the time, as the Keynesian Revolution reached the zenith of its influence.

Friedman (1953, 171) lays out in a logical order first the nature of the Humean price-specie-flow mechanism, and then the effects of recent central bank actions such that the process was thwarted, starting approximately thirty years before his essay appeared in print. He writes, "Traditionally, of course, monetary reserves have not been used as the primary method of adjusting to changes in external conditions but as a shock absorber pending changes in internal prices and incomes. A deficit has been met out of monetary reserves in the first instance, but the proceeds or even a multiple of the proceeds have been, as it were, impounded; that is, the stock of money has been allowed or made to decrease as a result of the decline of monetary reserves, with a consequent rise in interest rates and downward pressure on internal prices….Since the end

[12] Johnson's contribution to the Monetary Approach to the Balance of Payments, "which he viewed as the crowning achievement of his career" (Bhagwait and Frenkel (1977, 355)) is covered in less than half a page out of a total of 147 pages, which the *Journal of Political Economy* published in an issue memorializing Johnson in August 1984. In that issue there is no discussion of Johnson's research on open economy macro matters. Since this was the largest component of the research which he undertook while at the University of Chicago this omission is conspicuous.

of the First World War, nations have become increasingly unwilling to use reserves in this way and to allow the effect to be transmitted directly and immediately to internal monetary conditions and prices…Especially after the Great Depression completed the elevation of full employment to the primary goal of economic policy, nations have been unwilling to allow deficits to exert any deflationary effect."[13] This discussion is pure Humean thought, but it is also the Monetary Approach to the Balance of Payments, as we document more fully in Section 9 below.

Since Johnson and Mundell were working in this area (Johnson joined Friedman's Money and Banking Workshop soon after arriving at Chicago, and ran the workshop during Friedman's sabbatical year (1962-63))[14], the question is, What was the relationship between these two bodies of work, and did they communicate concerning issues in which they had a common interest? Was one group consistently responding to claims by the other? Was there an attempt to find common ground between the two groups? The appearance is that, to the contrary, Johnson's thinking was not influenced by his association with Friedman (Laidler (1984, 593). As Dornbusch (2001, 22) has noted, there were at least two quite distinct cohorts at this point, with little interaction between them. But appearances can be deceiving, and we argue below that Friedman's thinking had a major impact on the work done by Johnson and that by Mundell.

Friedman never cited any of Johnson or Mundell's monetary research, in part because his publications in this area preceded theirs by many years. Similarly, Johnson made comments on Friedman's closed economy analysis, but never seemed to be aware of what was contained in the open economy work. For example, in characterizing the differences between his own work and that of Friedman, Johnson makes the claim (Nobay and Johnson (1977, 480)) that monetarism assumes that the money supply is exogenous, whereas in an open economy under fixed exchange rates it is endogenous. This claim does not take into account that Friedman and Schwartz (1963, 89) had made precisely this point in the context of the differences between fixed and flexible exchange rates.

It would be remarkable if the money/macro analysis that Johnson and Mundell engaged in at the pre-eminent center for research in money and banking at the time, the Department of Economics at the University of Chicago, was done without any influence from the dominant figure there, who himself was recognized as the most important monetary economist alive. Nonetheless, this is a claim that Johnson and Mundell make, because they refuse to be viewed as "lesser lights" at the University (Johnson (1977, 4)). They say that it was just an accident of geography that they happened to be at the same institution as Friedman. Even if it is true that a mere coincidence had brought them into such proximity, isn't it still likely that Friedman's insights could have helped them in their research?

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[13] A footnote at this point in Friedman's essay spells out the automaticity of the effects of this mechanism in the Humean case. "Under a pure gold standard, these effects follow automatically, since any international claims not settled otherwise are settled by gold, which in the case of a deficit, is bodily extracted from the monetary stock…"
[14] Hynes (2001, 625) says about Johnson's participation in the workshop "…he would make only an occasional comment. This was not a particularly supportive environment…"

Friedman's views on monetary mechanisms in both closed and open economy settings were formed in the late forties and early fifties, long before Johnson and Mundell arrived at the University of Chicago. Therefore Friedman did not have much to gain from such communication. In contrast, the generally-held view is that Johnson and Mundell's ideas were evolving very rapidly towards Friedman's position. Perhaps they did not engage in discussions with him because this would have made clear that little of their work in this area was original. In order for their work to be seen as having been done independently of Friedman, at the very least they needed to limit their interactions with him.

4. Johnson's Varying Views of Friedman's Influence on His Work

Johnson's assertion that he had learned little from his interaction with Friedman is laid out in his later accounts (Nobay and Johnson (1977), and Johnson (1977)). The specific claims concerning the Monetary Approach to the Balance of Payments are: the approach was developed long before Mundell and he had arrived at the University of Chicago; and most of the work done after their arrival was carried out when they were not present at that institution, with the London School of Economics being mentioned specifically.[15]

These claims to independence are rendered implausible by two incontrovertible facts: first, in earlier incarnations of his narrative, Johnson (1972a) had conceded that their view is very monetarist, being consistent with the quantity theory of money; second, the timing of his conversion from Keynesianism coincided with his move to Chicago. We shall deal below with further evidence which contradicts Johnson's view. The essential point to note at this juncture is that Johnson changed his nomenclature concerning his discussion of the Monetary Approach to the Balance of Payments in order to assert independence. A re-writing of the substance of his memoirs was undertaken by Mundell, and it had much the same effect.

That the bulk of the work on the monetary approach was carried out in Chicago can be seen from the list of contributors to the Frenkel and Johnson (1976) volume. All the authors in that book were affiliated at least for a time with the University of Chicago. Furthermore, five years earlier, Johnson (1972a, 83-84) had stated categorically that the Monetary Approach to the Balance of Payments "has been very largely the work of my colleague R.A. Mundell and our students at the University of Chicago…" The footnote attached to this sentence in the text points to Chapters 8 and 9 of *International Economics* (Mundell 1968a) as being the key contributions to the monetary approach. According to the extended introductory footnote in the latter chapter, the material in it dates from "the spring of 1965…[and]…the Frank Graham Memorial Lecture (given at Princeton in April 1966…)…" (Mundell (1968a, 134). With these dates this work was definitely done after Mundell had arrived at the University of Chicago. In contrast, the

[15] This specific claim is contradicted by Johnson (1976, 263) writing only a year earlier: "…[the content of this]…chapter draws heavily on the ongoing work of the University of Chicago Workshop in International Economics…"

confusion concerning the dating of the drafting of the material in Chapter 8 becomes an important part of our analysis below.

As we have already noted, Johnson had been thinking along quantity theory lines starting in 1956. As an example of this, in a publication in the late fifties (Johnson (1958, 157)), he writes that "…a balance-of-payments deficit implies *either* dishoarding by residents, *or* credit creation by the monetary authorities—either an increase in V, or the maintenance of M. Further…a deficit associated with increasing velocity of circulation will tend to be self-correcting…."

Johnson's claim to independence from Friedman is reflected in the fact that his work is rarely cited by Johnson, even in places where it would be apt.[16] For example, Johnson (1972a, 80) omits Friedman's name from those who were familiar with the notion of the inflation rate's being *given* in the case of small countries on fixed exchange rates.[17] But in fact Friedman makes quite clear in his essay that one of his reasons for espousing flexible exchange rates is the primacy of internal policy which he endorses. (Friedman (1953, 180)) Also, he notes that the rate of inflation in a fixed rate system is determined by those countries which have the most expansionary policies, with the other countries needing to move in step because of the "harmonization of monetary and fiscal policy" which is required in such an exchange rate regime. (Friedman (1953, 198-200))

Throughout his 1971 de Vries Lecture Johnson used the expression "monetarist," an approach which at this time he heartily embraced. While using this expression he gave little credit to Friedman for the application of this framework, including him, in contrast, in "a new and eccentric vocal minority espousing a new theory called monetarism." (1972a, 51) Instead, Johnson made the ironic comment (1972a, 68) that "The Quantity Theory of Money: A Restatement" (in Friedman (1956, 3-21)) is "a much clearer and more enlightening statement of Keynes' essential contribution to the development of monetary theory than the statements of Keynes and his followers." Clearly Johnson was trying to wrest away from Friedman some of the credit given to him for the development of these ideas for both closed and open economy analysis. Leeson (2000, 757) has noted that Johnson's purpose was bifurcated. He wished to propagate "…international monetarism while denigrating Friedman's domestic version." What Leeson does not mention is that Johnson's international application is the same as the view that is presented on pages 164-67 and 170-73 of Friedman's (1953) essay, which amounts to an extended account of the Humean view of the mechanism of international adjustment.

By applying the term "monetarist," in light of the general readership's association of Friedman with this term, Johnson was merely confirming his own conversion to this way of thinking,

[16] Johnson's (1972a, 84) uses an unusual expression concerning the motivation for the Monetary Approach to the Balance of Payments. Namely, it "…harks consciously back to David Hume's analysis…" These words suggest that there was only a select group of individuals with whom Johnson was willing to engage on this subject.
[17] Instead, Johnson at this point lists the names of numerous economists, in alphabetical order of the names of their (unlisted) native countries.

rather than garnering any credit for himself in connection with purported original thoughts on the subject. So Johnson's strategy here backfired. As a result of this failure to erode Friedman's reputation, Johnson in his subsequent publications lashes out at the idea of monetarism. Johnson asserted that "…the evolution of the monetary approach to the balance of payments had…proceeded independently of the debates between monetarists and Keynesians." (Nobay and Johnson (1977, 480))

In his Horowitz Lecture, Johnson distinguished carefully between "monetarism" and his own work. "It is important to emphasize…[that]…the development of the monetary approach to the balance of payments has been confused in many so-called minds with something called 'monetarism,'… [which is embraced by]…those who emphasize the necessity of proper monetary policy for the stabilization of the economy, led but by no means dragooned by Milton Friedman." This discussion goes on to point out that many readers are "confused" and "unthinking" about the connection between monetarism and the monetary approach to the balance of payments, and as a result the two are viewed as intertwined because of "guilt by association." (Johnson (1977, 3-4)) These same sentiments are presented in Johnson (1975, 221), where they are voiced in the strongest terms: "There has been a noticeable tendency to dismiss the new approach as merely an international economics application of an eccentric and intellectually ludicrous point of view of a contemporary lunatic fringe referred to as monetarism."

Johnson's emotional derogation of those who endorse monetarism, as well as those who make the obvious connection between monetarism and the Monetary Approach to the Balance of Payment[18] (as Johnson himself, hair-splitting aside, had done just five years prior), is a dramatic breaking of the silence about Friedman's contribution on the subject. It suggests that the former silence had not been inadvertent. To have worked in monetary theory at the University of Chicago, where a monetarist was the dominant figure, and not to have mentioned his work, as part of the resurgence of interest in monetary ideas after the hiatus of the Keynesian Revolution, is very strange. It is implausible to argue that this omission was unintentional. The striking aspect of both Mundell (1967b, 1968b), and Frenkel and Johnson's (1976) discussion of the origins of the monetary analysis of an open economy is the *absence* of Friedman's name, for the open economy analysis he did in his 1953 essay and in his work with Anna Schwartz.

The effect of the previous silence and of the claims made later are the same: both argue that all credit for the Monetary Approach to the Balance of Payments belongs to Mundell and Johnson, since their work was independent of Friedman.

5. Mundell's Re-writing of His Narrative

Mundell's relationship with Friedman's essay follows a similar path. Mundell's initial publication in this area is a precise formalization of the argument in the Friedman essay: namely

[18] Among those whom Johnson is deriding as being "so-called minds" are Grubel (1976) and Whitman (1975).

"to analyze the difference between an economy with fixed exchange rates and flexible prices, and an economy with flexible exchange rates with fixed prices." (Mundell (2002, 6)) Although his conclusion at first blush appears to be that Friedman's intuition on this matter is "entirely erroneous" (1960a, 227), in fact careful consideration confirms that Mundell agreed with Friedman's view that it is only the dynamics of the two exchange rate regimes that cause them to differ. Their comparative static properties are identical, a point which Meade (1951, 190) had made earlier.

In subsequent publications, Mundell again does not do justice to what Friedman had written. So, for example, Mundell (1961b, 657, 663) wrote that Friedman was a proponent of flexible exchange rates, whose argument depended on money illusion. In fact, Friedman's position is the same as Mundell's, and Friedman had *already asked* the key question which Mundell later posed concerning choice of exchange rate regime, and had *answered* it in precisely the terms which Mundell uses. And two further works, which we consider below (1961a) and (1967b), deal with exactly the same questions as Friedman asks, such that they are best interpreted as formalizations of his argument. As he did with Fleming's (1962) work published at this time, Mundell does not cite Friedman in these publications.

Further evidence of this arises in Mundell's re-writing of the narrative of the circumstances of his "sharp move back to classical, money-oriented models, the context of the classical economy." (Brooks (1982, 139)) In an interview with John Brooks for *The New Yorker* magazine in April 1982, Mundell claimed that this "return-to-the-classics" (Mundell (2002, 13)) occurred "[a]round 1965." The paper which shows this change of heart is Mundell's (1960b) major contribution to the Monetary Approach to the Balance of Payments, which is singled out by Johnson, and appears as chapter 8 of *International Economics*.

The question then becomes, When was this paper (titled originally "International Disequilibrium and the Adjustment Process," (Mundell (1960b) (and published as Mundell (1967b)) and subsequently titled "Barter Theory and the Monetary Mechanism of Adjustment") (Mundell (1968b)) first drafted? In identifying his "return to the classics" as occurring "[a]round 1965" Mundell was using the date when he presented this paper to a World Bank Conference (in late July 1965). The date for the appearance of the original published version of this paper is 1967, in the conference volume edited by John Adler (1967).

Note that since July 1965 is the date of Mundell's first appointment as a faculty member at the University of Chicago (as Ford Visiting Professor of International Economics), it would appear that he was caving in to Friedman's point of view without so much as even engaging in an initial debate. Alternatively, it makes Mundell appear to be chameleonic. At the IMF he wrote in Keynesian terms; heading to Chicago he switched to the classical framework.

Mundell's subsequent narrative of these events portrays himself as having much greater independence of mind. Starting in 1992, Mundell's memoirs claim that his return to classical economics took place in the spring of 1963; and, he asserts, that is when he presented these ideas to a seminar in the Research Department of the IMF. Apparently Marcus Fleming "really disliked" (Mundell (2002, 13)) this change of heart on Mundell's part.

This account lacks plausibility in that in the spring of 1963 Mundell was actually writing the first draft of his most-cited paper (which assumes perfect capital mobility (Mundell (1963)) which is explicitly Keynesian in its structure. He continued to turn out research in this mode for at least another eighteen months. At a minimum, this shows that he conducted research in both modes *simultaneously*. Indeed, this very paper in question, Barter Theory, says that it uses "a simple model simultaneously classical and Keynesian in spirit." (Mundell (1967b, 461)) This hardly seems consistent with a "sharp move back to classical…models."[19]

Furthermore, there is no evidence to support much of what Mundell writes here. For example, the IMF has no record of his leading such a seminar, even though they have details of three of his other presentations between May 1962 and July 1963. An early version of this paper (Mundell (1966)) (dating from approximately March 1966 when Brookings Institution was set to publish Mundell's collected works (Mundell (1967a)) includes the original drawings of its figures. These diagrams are on yellowed paper, and the typewriter which was used, in a clumsy labeling of them had a European font. Finally, the sentiments in the introductory paragraphs of this paper are repeated, almost verbatim, in Mundell's (1961a) *Kyklos* publication which appeared in 1961.[20] All this evidence points to this paper's having been written sometime in 1960, when Mundell was in Bologna.[21]

Mundell's questionable dating of the drafting of this paper has led many economists to provide their own timing of Mundell's purported sharp change in modeling strategy. Thus, Frenkel and Johnson (1976, 32) view the transition as having taken place after 1964; Obstfeld (2001, 7), as mid-sixties; and Dornbusch (2000, 203), as late sixties. All these narratives see Friedman as having had a minimal impact on Mundell's research, and are based on the idea that Mundell's Barter Theory paper was written in 1963 or after. But, in fact, all available evidence shows otherwise. Any shift in Mundell's position signaled by this paper's drafting is going to be mis-dated to the extent that the date of the drafting is misperceived. Specifically, since this paper

[19] Frenkel and Johnson (1976, 10), after repeating Mundell's claim, that Barter Theory "…introduces the connecting lines between…classical and Keynesian concepts and methods of analysis…," asserts that this same paper is "…the closest to a classic statement of the difference between the Keynesian approach and the monetary approach that can be found in the phase of Mundell's transition from one to the other approach."

[20] The title of this piece, "The International Disequilibrium System," is very similar to the original title of the Barter Theory paper.

[21] Barter Theory clearly must have preceded *Kyklos*, in terms of the timing of their drafts. The reason is that *Kyklos* asserts that the Hume adjustment process does not work. In order to make such a claim, that process first needs to be modeled and assessed. Given the vagaries of the publication outcomes, ironically Mundell's *oeuvre* has the claim that the process does not work appear in print (1961) before the article which claims that the adjustment process operates automatically (1967). This mis-ordering is corrected in Mundell's (1968a) collected works. There, not only does *Kyklos* come after Barter Theory, it is in a completely different section of the book. The re-titling of the Barter Theory paper hides the fact that it and *Kyklos* are intimately related, were written virtually simultaneously, and convey the ideas in two adjacent paragraphs which appear on the same page in Friedman (1953, 171).

was drafted before (rather than after) all of the Keynesian papers which Mundell produced at the IMF (1961-63), Frenkel and Johnson (1976, 10) provide an incorrect assessment of Mundell's impact on the Monetary Approach to the Balance of Payments. The impression one gets from their version of the events is that Mundell's work in this area was of great novelty, that it represented a different view of macroeconomic theory, and that it was done well before he arrived at Chicago. This appears to validate Mundell's claim that his contribution to the approach was accomplished independently of Friedman's influence. If, instead, the paper is seen as leftover work from Mundell's days in Italy, then the paper was by that time an old manuscript which had not yet seen the light of day in terms of publication.

While it is true that the work was done before Mundell's arrival in Chicago in 1965, it is not correct to see this paper as independent of Friedman's influence. Mundell had been a post-doc at Chicago during 1956-1957, when he had conversations with Friedman about open economy macro and exchange rate regimes. He took a particular interest in Friedman's (1953) celebrated essay, as is apparent from his citation of it in (1960a) and (1961b). The first of these articles includes a number of citations of this Friedman work, and one of these provides the quotation of a whole sentence from it. As we have noted above, the second article repeats ideas about the choice of exchange rate regimes which appear in Friedman.

Since Mundell cites material from various pages throughout this essay, it seems likely that he, at least at times during this period, was aware that the ideas behind the Monetary Approach to the Balance of Payments and Optimum Currency Areas are included in that work.

Mundell's neglect of Friedman's contributions in this area can be easily seen in the various drafts of the introduction to his Barter Theory paper. He writes that the purpose of the paper is to build "…bridges between…the barter and the monetary models, on the one hand, and between the classical and Keynesian traditions in international economics, on the other…[because it]…is…self-evident…that they do not yet exist…" In introducing these ideas Mundell says that his model "…owes its origin to David Hume…", and he goes on to mention the names of numerous economists, including twelve from the 20th century: Viner, Meade, Johnson, Keynes, Hayek, Myrdal, Tinbergen, Frisch, Leontief, Samuelson, Hicks, and Mosak.

In what appears to be a direct swipe at Friedman, Mundell describes Mosak's work as being different from all the other economists mentioned because it is not in a Keynesian mold. Mundell's (1968a, 112) explanation for the exceptional nature of Mosak's work is that "…he was working in a different milieu (Chicago) in the 1940s where Keynes' ideas were not regarded as so novel…" Does not this description fit Friedman's work even more closely, and is not the relevance of Friedman's work to that of Johnson and Mundell an order of magnitude greater? Furthermore, the quantity theory of money is mentioned at least five times in this paper. Since no name was more closely associated at that time with this framework, it is noteworthy that Friedman's name does not appear in the original version of the paper.[22]

6. Friedman's Statement of the Optimum Currency Areas Argument

As I have recounted elsewhere (Boyer 2009), the Optimum Currency Areas Argument appears in virtually identical terms in Friedman (1953) and in Mundell (1961b), so that Friedman's essay provides the first definitive statement of it (although Friedman calls it "mixed system").[23] The reason that this is not well-known in the profession *is that the most accessible form of Friedman's essay is an abridged version,* for which the "mixed system" argument has been elided. This version appears in the American Economic Association's *Readings in International Economics* (Caves and Johnson (1968))

We have provided examples above showing that the strategy employed by Johnson and Mundell was to use Friedman's ideas without acknowledgment, as is seen in their use of the expressions "quantity theory of money" and "monetarist" without citation. This strategy culminated in the spring of 1969, when Johnson was invited to contribute to the Hobart Papers, presenting a lecture on the alternatives which the UK faced, subsequent to the devaluation of sterling. Johnson chose as the title for this essay "The Case for Flexible Exchange Rates, 1969." This title is identical to that of Friedman's celebrated 1953 essay on exchange rate regimes, but with the current date appended to it. Johnson graciously notes in the first footnote that "The title acknowledges the indebtedness of all serious writers on this subject to Milton Friedman's modern classic essay…" (Johnson (1969b, 12)) Johnson then includes a full citation of both the original publication and the reprint in Caves and Johnson. While it is noted that the reprint is abridged, the extent and nature of the abridgement are not indicated. This is strange since the elision included precisely the topic which Johnson's paper covers, namely, What are the appropriate criteria for a country to use in choosing between a rigidly fixed exchange rate and a fully flexible exchange rate regime?[24]

Stranger still is the way in which Johnson's article proceeds. At no point is Friedman's name mentioned in the body of the text. Consequently the reader is baffled as to whether the material being presented is to be interpreted as Johnson's original thoughts on the subject, or as Johnson repeating Friedman's ideas. If the latter, are these words purporting to represent Friedman's

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[22] Friedman's name does appear, but without citation (for work that was published as Friedman (1960b)), at the end of the version published in Mundell (1968a, 130), merely in the context of questioning whether the Friedman monetary growth rule is appropriate in a fixed exchange rate setting.

[23] As Cesarano [2006] has pointed out, Lerner (1944) and (1947) asks the key question concerning Optimum Currency Areas. Since he deals specifically neither with asymmetric shocks nor with the difficulties inherent in the maintenance of a fixed exchange rate regime, he should be credited only with an abbreviated stab at this problem. In contrast, Friedman (1953, 192-96, 198-201) deals with all these issues. Mundell (1961b) then is seen as merely a reiteration of Friedman's argument.

[24] Laffer (1973) writing at the time notes that the key pages in Friedman (1953) which deal with this question are 191-200. All the material in these pages was either deleted or revised for the abridgement. This material constitutes about two-thirds of all the alterations which one finds in the Caves and Johnson version.

contemporaneous ideas or those of sixteen years prior as applied to contemporaneous circumstances?

The only name that appears in the body of Johnson's paper is Keynes. The gist of the argument is that Friedman's essay had contributed little of value, because "The case for flexible exchange rates on these grounds has been understood and propounded by economists since the work of Keynes and others on the monetary disturbances that followed the First World War." (Johnson (1969b, 13)) In contrast, the important recent contribution to exchange rate thinking was not done by Friedman, in Johnson's view. Instead, the key "development in the theory of exchange rates…[is]…the theory of optimum currency areas, developed by R.A. Mundell and R.I. McKinnon." (Johnson (1972b, 13))

For much of his essay, Johnson directs his attention to what should be done with sterling, which is appropriate since this paper was delivered in front of an audience in London. However, Friedman (1953, 192-96)) also discussed sterling and the sterling area in his essay. Johnson should have, therefore, made a comparison with what Friedman had said. This is all the more the case, because it was in this context that Friedman had raised the issue of Optimum Currency Areas (although his term for the idea was "mixed system").

Certainly a possible interpretation of what Johnson was doing in this article was to make the Mundell position seem reasonable relative to that of Friedman. He is portrayed as being irrationally attached to flexible exchange rates, and adamantly opposed to fixed exchange rates. Friedman pointed out numerous times in various lectures that this was not an accurate statement of his point of view. His position was essentially identical to that of Mundell: both opposed pegged-but-adjustable exchange rate regimes, however Friedman was more consistent in pointing out that this implied that central banks under rigidly fixed exchange rate regimes would need to end their attempts at independent stabilization policy.

7. The Historical Origins of the Monetary Approach to the Balance of Payments According to Johnson and Mundell

We have pointed to the role played by the classical adjustment process in Friedman's discussion of balance of payments problems. Since at the time money played only a minor role in most other presentations of the mechanism of open-economy re-equilibration, to omit Friedman's name from a narrative of the historical origins of the modern revival of the price-specie-flow mechanism leaves a conspicuous gap in the account. Nonetheless that is the way in which Johnson (1977) and Mundell (1967b) proceeded.

A narrative which fails to include a discussion of the impacts of the Keynesian revolution[25] and the monetarist counter-revolution spearheaded by Friedman, raises the question immediately as

[25] Strangely Frenkel and Johnson (1976, 9) refers to "…the 'Keynesian Revolution' in monetary theory…" but not to the monetarist counter-revolution. This, too, seems to be an attempt to minimize the impact of Friedman's interest in monetary factors in the post-war environment.

to why there was a revival of interest in the 1960s in the mechanism laid out in an essay (Hume (1898 [1752])) that had been written over two hundred years before. There had not been such interest in the preceding fifty years, since Taussig (1966 [1927]) and his students had failed to find a convincing quantity theoretic explanation which yielded empirically useful models of adjustment in open economies. Viner (1957 [1937]) had refined the thinking to such a point that Hume was no longer the relevant reference to cite in this context.

We are told that this approach is novel, that in 1969 it was "newly emerging." As late as 1974 the approach is described as both "new" and as having "...a long, solid, and academically overwhelmingly reputable history…" (Frenkel and Johnson (1976, 9, 29)) If the latter is true, should there not be a consensus about which are the important contributions in this area? Should there not be discernable links among these contributions? Should any single contribution not be seen as adding new insights to those that had preceded it? And should not these contributors be viewed as thereafter embracing only this approach, abandoning rival frameworks which had become outdated with the new insights?

That there is no uncontroversial history of the monetary approach is apparent from the many versions that have been published, and the fact that there is wide-spread disagreement on the documents which are central to it. In some cases, particular economists have had constantly shifting views of which individuals had developed the key insights. Thus in 1968, Mundell (1968b, 5) uses the expression "monetary approach" in its application to balance-of-payments analysis, and cites only one paper: Johnson (1958).

Johnson must have been flattered by this claim, especially since it noted the superiority of this work over the traditional elasticities and absorption approaches, because it "…is valid even when there are capital movements…"[26] This assertion is reprinted in Chapter 10 in Mundell's (1968a, 150) *International Economics*. Johnson returned the compliment by pointing to the important pieces on the monetary approach as being Chapters 8 and 9 in that same volume. The original forms of these two chapters (Mundell 1967b) have a total of three citations: one to a book on Zen Buddhism; one, a self-citation; and one to a little-known paper by Prais (1961). So the view that Johnson made a major contribution seems defective in that that work cites no earlier work that conforms with Mundell's references, and, in turn, it is not cited frequently by others until after Mundell's championing of it. These points raise doubts as to whether Johnson (1958) is an "essential document" in the monetary approach's development.

In 1971 Johnson (1972a, 84) claimed that the key originators of the modern revival of this framework were Holtrop and Koopmans (and Mundell (1960b), as well, continues to be singled out as a major contribution), although Johnson admits that this view is grounded "…on the basis of a hunch derived from the literature, rather than of scholarly research." By 1974 this hunch had proved to be a dead-end. These names do not appear in the list of fifty-two references which

[26] Read Mundell's (Vane and Mulhearn (2006, 95)) reassessment of Johnson's contribution, in order to determine whether Mundell's initial praise was likely to have been sincere.

Frenkel and Johnson (1976) provide, in what should have been a definitive statement, being the first chapter in the eponymously titled book. In 1975 Johnson (1977, 4) has a different view, and sees the key contributor as being Meade. (In this account Mundell as well continues to be an important figure, but for reasons different from his being associated with the Dutch economists indirectly, through Polak, during his period of service at the IMF.)

Without Friedman there is no simple line connecting the various contributions in this area. Johnson (1958) conspicuously lacks a citation to any writing before the War, and therefore can hardly be considered a link in the narrative. But Mundell is part of a completely disjoint set of references, so there are at least two channels by which the monetary approach developed.

In addition, one must take account of the input provided by the Research Department at the IMF. Their claims, which they press with equal vigor, distinguish between two different views of the origins of the monetary approaches, Keynesian and Johnsonian, and neither of these corresponds with the narrative as told by Frenkel and Johnson, as will now be shown.

8. Polak and Helliwell's Competing Views of the Origins of the Monetary Approach

Polak (2002) has his own view on this subject. According to him there are two monetary approaches: Keynesian and Johnsonian. The Keynesian approach is evolutionary, and his own work (Polak 1957) is the key document. The Johnsonian approach is revolutionary, and is based on Johnson (1958).

The Keynesian monetary approach, according to Polak, evolved from the Cambridge economists. The list of contributors which he provides is disjoint from what one finds in any other publication describing the origins of this approach. The names included are Kahn (1931), Machlup (1943), and Holtrop (1957). It is true that Johnson at one point gave credit to Holtrop, but not with sufficient specificity that he appears in the definitive list in Frenkel and Johnson.

Polak's outline of the Johnsonian version of the monetary approach refers only to Johnson (1958). About this paper Polak says "His discovery of the monetary approach as a completely new starting point for balance of payments analysis seemed to have had the liberating force of an epiphany…The 'essentially monetary' epithet made its first appearance in Harry Johnson's (1958) 'basic article'…devaluation remained 'the standard question'…on which the Chicago School would demonstrate its version of the monetary approach…" (Polak (2002, 26-27))

There are a number of deficiencies in this view of the development of the monetary approach. First, contrary to Polak's epiphany claim, Johnson was unaware that he had revolutionized balance of payments analysis until being told by Mundell (1968b) that he had done so. Before this time, Johnson's 1958 paper had languished. So it was after a delay of more than ten years that Johnson refers to the monetary approach as being a newly-emerging way of thinking.[27]

[27] Moggridge's (2008, 146) discussion of this paper shows that work on it began no later than 1954.

Johnson's paper contains two equations, both of which are obvious, and had been used in discussions of the absorption approach previously. There is no separate equation specifying equilibrium in the money market. The "model" in the paper has never been used in any subsequent discussion. Although a distinction is drawn between stocks and flows, it is not applied consistently.[28]

In addition, although Polak's statement of the key requisites of the monetary approach seems reasonable, we disagree with the way in which he applies them to the evidence. Polak is not correct as to the first appearance of the expression "essentially monetary" in this context. Friedman (1953, 166) writing about responses to fluctuations in trade patterns notes that "Adjustments are required continuously, and many are called for by essentially monetary phenomena, which, if promptly offset by a movement in the exchange rate, would require no change in the actual allocation of resources." This shows that by Polak's criterion, Friedman is a prior modern statement of the monetary approach, not only because the crucial wording appears there, but also because he makes the important point that devaluation is monetary policy under another name. In contrast, Johnson (1958, 162-63) distinguishes forcefully between a "policy of expenditure-reduction…through…monetary restriction…and… expenditure-switching …policies…[such as]…devaluation." If "expenditure-reducing" and "expenditure-switching" constitute a useful method of categorization, then Johnson must be arguing that monetary policy and devaluation are quite different initiatives. Later Johnson (1976, 274) seemed to recognize that there was an inconsistency here, but he continued to perpetuate it.

Finally, Polak saw Mundell as not contributing anything useful to the approach, and this viewpoint must be given serious consideration, since Polak was Director of Research at the IMF during the time period when Mundell was there. Since some dating of Mundell's (1960b) contribution sees this work as being written at this point, one would think that Polak would be an authoritative source on this matter. [29] Polak's claim is that the derivative nature of Mundell's contribution can be easily discerned by focusing on the relevant dates. Polak (2002, 19, 27) targets Johnson's view of this matter. That view is faulted as being "rather different (and no doubt audience-pleasing)" in that (in a lecture in Amsterdam) he highlights the Dutch contribution and, in that connection, "Mundell's period of service in the research department of the International Monetary Fund…" Polak makes the valid point that if Johnson (1958) is viewed as part of the early work on the monetary approach, then that dating of the approach is "long before Mundell moved to Chicago, indeed before he joined the Fund staff in 1961—which suggests that Johnson's genealogy of the Chicago monetary approach…was unduly modest." Polak's conclusion is that Johnson (1958) and Polak (1957) are the early valuable contributions. Ironically, Polak, the man who was Director of Research at the time, dismisses as of minor

[28] Hynes (2001, 628) considers Johnson (1958) to be a seminal paper, despite its numerous shortcomings, because its "…message was quite subversive."

[29] The facts, that Polak does not have a high regard for Barter Theory nor does he gauge its impact as being important, are consistent with the absence of any record at the IMF that a seminar presentation on this paper took place during Mundell's stint there.

significance at most the piece of work that Chicago insists is an essential document, and which Mundell dates to his period of service in that Research Department: his Barter Theory paper.

My assessment is the same as Polak's: the role of Barter Theory has been misinterpreted. Specifically, my view is that Barter Theory did play a significant role in the monetary approach, but that due to questionable dating on Mundell's part, the usual interpretation of this contribution is incorrect: in fact, it was drafted much earlier than the timing given in most narratives. As to significance, many more chapters in Frenkel and Johnson (1976) use the template that is provided by Mundell's (1960b) "essential document" than use Johnson (1958).

Furthermore, the IMF did have a role to play in this approach, according to the consensus. As we have noted, one of the mystifying aspects of this subject is how few of the purported main contributions reference any of the others. The only piece which attracts multiple citations is Prais (1961) (with Mundell's (1967a, 448) reference being the first appearance in a publication). But this only deepens, rather than resolves, the mystery. Prais deals for the most part with the fixed- price models exemplified by Polak (1957). Since the bulk of subsequent work in this area instead assumes price flexibility, which was a key component of the price-specie-flow mechanism, it is unclear what relevance Prais's work has to the evolution of this literature. This is especially true concerning the popular assumption of purchasing power parity, which is used extensively in the monetary approach. Prais's model is incompatible with such a view.

Helliwell, too, is deferential to Johnson's views on the monetary approach. Helliwell notes that "Johnson did more than anyone else to define the boundaries and establish the claims of the monetary approach to the balance of payments." (Helliwell (1978, S63)) Nonetheless, Johnson's claim that his narrative of the history of the subject is based on "…a hunch derived from the literature rather than of scholarly research.." (Johnson (1972a, 84)) is the sort of tenuous evidence which Johnson used late in his career, and Helliwell notes that it "…reflects a departure from the well-referenced and careful analytical style that is usually a hallmark of Harry Johnson's exposition of balance-of-payments issues." (S64)

In keeping with our view that the use of this approach was a device to allocate credit for existing results, rather than a progressive research agenda to derive new results, Helliwell highlights some of its distinctive aspects. "One of the most interesting features of the monetary approach to the balance of payments, especially in the post-1973[30] development and defense of it by Harry Johnson, has been the adversarial tone of the analysis. This tone is surprising because there would appear to be little in the way of professional resistance to consideration of the consequences and difficulties inherent in sterilization policies, and hence to serious questioning of the assumption of national monetary independence commonly adopted in textbook analysis."(S63) He goes on to say, "It must remain something of puzzle why so much stress was

[30] The precise date of writing of this portion of Frenkel and Johnson (1976) is unclear. Since Johnson's first stroke occurred in October 1973, Helliwell seems to be alluding to work that was written afterwards. To focus on such work may not be an even-handed way of dealing with Johnson's overall impact in this area.

placed, by Johnson and others, on the extent to which the monetary approach differs from what is described, without much supporting detail, as the 'traditional approach.' The puzzle becomes even deeper when one notes that the same papers and authors (e.g. Meade 1951 and Mundell 1968a) that were regarded as leading exponents of the Keynesian policy approach are also seen as providing the essence of the monetary approach." (S64) By 1974 Johnson was seeing Meade (rather than the Dutch economists) as being the main influence on Mundell. It seems unlikely that Meade and Mundell were both the source of the traditional approach and the ones who overthrew it. To have both roles raises the question as to with which view their loyalties lay.

Helliwell's footnote at this point continues the expression of puzzlement. "Another slightly puzzling feature of some of the papers utilizing the monetary approach is the extent to which an air of controversy is built up by reference to un-named detractors who are said to have made extreme counterclaims. For example, in the introductory chapter of Frenkel and Johnson (1976), a brief account 'implicitly disposes of various, and often ill-informed, criticisms that have tended to have been made of it, and which amount essentially to red herrings across the trail of scientific study and understanding.' References to the sources of the red herrings are not included; this too reflects a departure from the well-referenced and careful analytic style that is usually a hallmark of Harry Johnson's exposition of balance-of-payments issues." (S64)

9. Friedman's Impact on the Monetary Approach to the Balance of Payments

The monetary approach reached its high-water mark with Frenkel and Johnson (1976) and the publications which appeared soon thereafter. By 1985 it had been abandoned, as is apparent from the survey volume edited by Kenen (Jones and Kenen 1985).[31] Although the expression appears there, it does so without the distinctive features which had set it apart a decade earlier. Not surprisingly, it is Frenkel (Frenkel and Mussa 1985) who continues to plow this material, with other authors using the expression in a perfunctory manner.

With its output arriving in such a concentrated way (over such a short period of time), it is easy to see that the model contained in Barter Theory is the central template for this approach, and therefore that that paper is the key document. Thus the question arises as to whether this paper merely replicates mathematically the ideas that are expressed verbally in Friedman's essay. In order to make this case we need to introduce a second paper by Mundell which follows Friedman's thinking very closely. This paper asserts that "Hume's law is inapplicable." (Mundell (1961a, 153)) Since this paper asserts the exact opposite of Barter Theory (which looks at the automaticity of balance-of-payments re-equilibration), it points to two logical inconsistencies. The assertion that the law is no longer operative logically requires that the Humean mechanism on which it relies be modeled. Since this publication does not present a Humean model, one looks instead to the mechanism that appears in Barter Theory. In that paper

[31] A similar assessment is made by Blejer et al. (1995, 709).

one finds such a theoretical construct: "The model owes its origin to David Hume." (Mundell (1967b, 445)). But the question arises as to this publication, Why would anyone, intending to apply the construct's insights to issues in current macroeconomic performance, model a process which is no longer operative?

Logical consistency in this matter can be restored if one follows the evidence which shows that this paper was drafted in 1960, but was not published until 1967. Being the earlier of the two drafted papers, Barter Theory is logically consistent with Mundell (1961a), in the same way that the first paragraph on page 171 of Friedman (1953) precedes the second paragraph. *Both* Friedman and Mundell's accounts start off by showing how Hume's mechanism would work in the absence of offsetting actions by the central bank. *But both accounts also go on to say* that due to a recent change in behavior on the part of the central bank, this automatic mechanism has been thwarted, so that balance-of-payments problems continue to fester.

In the event, the actual order of publication of Mundell's papers makes his position untenable. If "Hume's law is inapplicable," there is little reason to model the mechanics of an obsolete process, and even less to claim that the process in its original form has any relevance, let alone that the mechanism which it describes has any possibility of success.

We now point out how Barter Theory (Mundell (1967b, 441-62)) is very similar to Friedman (1953, 164-67, 170-72) in virtually every way. This is true as to the views expressed, the assumptions made, the results derived, and the conclusions drawn.

These similarities will be spelled out by covering the following points which these two papers have in common:

A. <u>There is a separation of real effects from monetary ones.</u>

Friedman's essay, immediately after the introduction, in the first paragraph of the substantive material, makes the distinction between "'real' conditions" and "monetary conditions." Examples of the former are "weather, technical conditions of production, consumer tastes." Examples of the latter are "divergent degrees of inflation or deflation in various countries." It is noted that real shocks are given relatively little attention in the essay since their "…effects on the relative price structure are likely to be much the same whether exchange rates are rigid or flexible and to raise much the same problems of adjustment in either case…" (159) The focus in the essay instead is on shocks which are "essentially monetary phenomena." (166)

Barter Theory too makes this distinction, looking for cases of adjustment for which "…the separation of real from monetary phenomenon [*sic*] is exact." (454) It is pointed out that in the "…dynamical, short-run, disequilibrium theory monetary elements assume a role of first-order importance in the adjustment process. But after the adjustment process was complete money turned out to be a mere veil, with no influence upon the nature of [*sic*] position of long-run equilibrium." (442)

B. The adjustment occurs automatically.

Friedman notes that "…monetary reserves…have…been used...as a shock absorber pending changes in internal prices…A deficit has been met out of monetary reserves in the first instance, but the proceeds or even a multiple of the proceeds have been, as it were, impounded; that is, the stock of money has been allowed or made to decrease as a result of the decline of monetary reserves, with a consequent…downward pressure on internal prices…" A footnote points out that "Under a pure gold standard, these effects follow automatically, since any international claims not settled otherwise are settled by gold, which, in the case of a deficit, is bodily extracted from the monetary stock…" (171)

Barter Theory portrays the same mechanism: "An over-issue by the banking system would quickly bring its own corrective, as specie flowed out and forced the banks to take back the redundant currency…The nominal quantity of money was thus determined in a single economy by international considerations…" (443) Thus, classical economists provided a "…demonstration of the automaticity of balance-of-payments adjustment…To have perceived the essential truth of these propositions, which even today exhibit a fundamental truth, was a supreme intellectual achievement of classical economic analysis…"[32] (442-43)

C. These ideas are presented in the context of a specific monetary shock: foreign deflation.[33]

Friedman deals with foreign deflation as a prime example of a monetary shock, covering it explicitly at least three times, and dealing with the issue implicitly a roughly equal number of times. Such a shock "…if promptly offset by a movement in the exchange rate, would require no change in the actual allocation of resources." (166) "…[A] decline of 10 percent in every internal price in Germany…would clearly have identically the same effect…as…a decline of 10 per cent in the dollar price of the mark, with all internal prices unchanged…" (165) In contrast, under fixed exchange rates because "[w]age rates tend to be among the less flexible prices…, an incipient deficit that is countered by a policy of permitting or forcing prices to decline is likely to produce unemployment rather than, or in addition to, wage decreases…In this way, it offsets the incipient deficit. But this is clearly a highly inefficient method of adjusting to external changes. If the external changes are deep-seated and persistent, the unemployment produces steady downward pressure on prices and wages, and the adjustment will not have been completed until the deflation has run its sorry course." (165)

[32] Note that these sentiments appear also in Mundell (1961a, 153), but with quite a different conclusion: "…this analytical separation…is…a[n]…artificial distinction…[which]…is now out-dated…"

[33] Such a topic has often been the subject of analysis in international financial tracts. Laursen and Metzler (1950, 283-84) look into the international transmission of the business cycle under flexible exchange rates. If these cycles take the form of price movements, they say that there is "general agreement" on the shielding provided by flexible exchange rates, based on an argument that is "extremely simple." Friedman and Mundell are deriving their conclusions from this simple argument.

Barter Theory introduces six diagrams, but employs them for just one exercise: a monetary shock for which a fifty percent devaluation would restore equilibrium, with no change in the values of real variables. A fifty percent decline in foreign prices is an example of such a shock. It is noted that an alternative path to the same equilibrium, in terms of the values of real variables, can be followed without any change in the value of the exchange rate. "Obviously a deflationary process would reduce the [domestic] price level and the money supply…[by fifty percent]…under the automatic fixed exchange rate adjustment process. But the deflation involved may be a painful process, and certainly would be if rigidity of factor prices meant that deflation would imply unemployment…" (454-55)

D. <u>Devaluation is monetary policy by another name.</u>

Friedman notes that "Adjustments are required continuously, and many are called for by essentially monetary phenomena, which, if promptly offset by a movement in the exchange rate, would require no change in the actual allocation of resources."(166) Since devaluation can offset a monetary shock, such as a monetary policy initiative, it must itself be a monetary phenomenon, and therefore is monetary policy by another name.

Barter Theory shows that when the problem is that money supply and prices are out of line, of the same proportion, against foreign trading partners, due to a monetary shock, then equilibrium can be restored through devaluation. Therefore devaluation and monetary policy are the same, only with different names. (454-55)

E. <u>The automatic mechanism described by Hume has been thwarted by sterilization.</u>

Friedman says about balance-of-payments surpluses and deficits, "Since the end of the first World War, nations have become increasingly unwilling to use reserves in this way and to allow the effect to be transmitted directly and immediately to internal monetary conditions and prices. Already during the 1920's, the United States, to cite one outstanding and critical example, refused to allow its surplus, which took the form of gold imports, to raise domestic prices in the way the supposed rules of the gold standard demanded; instead, it 'sterilized' gold imports. Especially after the Great Depression completed the elevation of full employment to the primary goal of economic policy, nations have been unwilling to allow deficits to exert any deflationary effect." (171)

Turning now to the companion piece to Barter Theory, we find Mundell (1961a) making a strikingly similar statement of the same idea: "The decline in automaticity dates from the first attempts of central banks to adjust the domestic supply of notes to…the needs of trade… instead of the requirements of external equilibrium (the bullionist principle)…the abandonment of the bullionist principle became widespread only after the revolution in Federal Reserve policy during the 1920's, and especially after legal or de facto recognition in post-war years of full employment as a primary goal of public policy…" (153)

These quotations show the extensive overlap between this portion of Friedman's essay and Mundell's Barter Theory paper, as well as his paper in *Kyklos*.[34] *Mundell does not cite Friedman in either of these papers.*

10. Conclusion

This paper has recounted how in 1959 Harry Johnson took up the "Keynesian" chair in the Department of Economics at the University of Chicago, initially offered a dozen years previously to Samuelson, and occupied earlier in the decade by Metzler. Since it is widely recognized that, at least in his open economy macroeconomic analysis, Johnson switched to a monetarist point of view during his stay there, and since the Department was "pretty much dominated by Milton Friedman," it is natural to see Friedman as having converted Johnson to his cause. This is a view that Johnson resisted, and with ever greater vigor over time as it portrayed Mundell and him as being the "lesser lights" at Chicago.

The resulting equilibrium seemed to suit the purposes of all three economists.[35] Although he appears to have been somewhat oblivious to the *specifics* of what they were doing, Friedman was flattered that the Chicago Keynesians were imitating him. Johnson got what was, in effect, credit for open economy macro-analysis which should have gone, first and foremost, to Friedman, and only then to Johnson, for having *popularized* Friedman's 1953 approach; and Mundell was able to claim that although his ideas were very *similar* to Friedman's he had arrived at them independently, via a different route.

[34] Further similarities between Friedman (1953) and Mundell (1967b) and (1961b) are: 1. It is a matter of indifference for the process of adjustment whether most of the changes in money income occur through movements in the price level or through movements in real output. This is stated explicitly in Mundell (1961a,154), and in passing in Mundell (1967b, 445, 453, 454, 461). Both publications thus encompass both the classical and the Keynesian points of view. Friedman (1953, 164) sees both possibilities as leading towards re-equilibration, as the title to this subsection ("Changes in Internal Prices or Income") indicates. 2. Capital movements are not an important part of the adjustment process. Friedman (1953, 166) says "interest-induced capital movements…come into operation only incidentally to the adjustment of internal prices." Mundell (1967b, 444-45) assumes that there is no capital market in the economy which he is analyzing; therefore "…wealth is held primarily in the form of money and goods…" Mundell (1961a, 165) sees the automatic adjustment process being at work, independent of the degree of capital mobility, including the extreme case when it is zero.

[35] Perhaps "equilibrium" mischaracterizes the situation, since others have viewed it as unsustainable. Dornbusch (2001, 22, 23) noted that there were tensions in the Department, if only because Mundell is an "*enfant terrible*" who "could play the bad boy with success." This was reflected in a lack of communication between members of the Money and Banking Workshop (run by Friedman), and the International Economics Workshop (Johnson and Mundell). One may speculate that Mundell's abrupt departure in 1971, to an institution (the University of Waterloo, Ontario, Canada) which was not in the same league as the University of Chicago, was not lamented by Friedman.

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