Don Patinkin and The Origins of Postwar Monetary Orthodoxy

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Abstract:

Don Patinkin's Money, Interest, and Prices (1956) set the ground rules of postwar monetary discourse, for better or worse. A close look at the intellectual origins of the book in Patinkin's own life shows it to emerge equally from the Old Chicago School of Simons/Mints/Knight and the Cowles Commission of Lange/Marschak/Haavelmo. Patinkin's conception of money as essentially an outside asset is argued to emerge from the historical context of war finance, and is contrasted with the Gurley-Shaw conception of money as a form of inside credit.

Biographical Note:

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> The Theory of Prices must be conceived as an <u>organon</u>, in the sense in which Marshall's <u>Principles</u> may be said to be an organon. The Theory of Prices, for those who accept this view, is not a set of maxims of monetary policy, which can claim neither the general validity nor the authority that may be said to attach to a set of conceptual tools. Nor is it a series of speculations as to matters of fact, with respect to which the last word must be provided by investigations of the "facts" themselves.
>
> Marget (1938, p. vii).

<u>Money, Interest, and Prices</u> (1956), Don Patinkin's largest and most influential work, might very easily never have been written.

The main theme of the book, indicated in its subtitle as "the integration of monetary and value theory", developed from two <u>Econometrica</u> articles (1948a, 1949a) which were drawn from the first part of Patinkin's 1947 PhD thesis. The odd thing is that, although the thesis was submitted for oral defense on July 29, as late as March 6 the draft thesis contained only what would be the second section of the final thesis, the part that would eventually be published as "Involuntary Unemployment and the Keynesian Supply Function" (1949b). Further, the March 6 draft was essentially a wage bargaining model, and there was almost nothing in it about the monetary themes that would appear in the first part of the thesis and at the core of <u>MIP</u>.[1] Exactly how this first part came to be added, and the crucial role of Cowles Commission staffers Jacob Marschak, Trygve Haavelmo, and Kenneth Arrow in stimulating that addition, is a story that will be told in due course. For now, the important point to hold in mind is that, if we want to form an accurate impression of Don Patinkin's intellectual formation and trajectory, we must begin by abstracting from the book on which his reputation is based.

Palestine

In the fall of 1946, the biggest thing on Patinkin's mind was to find a workable thesis topic so he could get his PhD and emigrate to Palestine. The previous spring he had started a thesis on "The International Economic Position of Palestine", a topic quite deliberately chosen as preparation for emigration, but he had to give it up for lack of data. At that time, Jacob Viner was Patinkin's intended thesis sponsor but Jacob Marschak, research director at the Cowles Commission since 1943, played an equally important role in Patinkin's life as, in effect, his emigration sponsor. Marschak himself had been approached about possible emigration but he declined and recommended instead Patinkin (among others): "He is an ardent Zionist but wants to finish his education first. As he sets himself very high standards, this will take two or three more years, before he goes to Palestine."[2] This was in June 1945. Patinkin's July 1947 thesis defense came almost exactly two years later.

Born January 8, 1922, the son of Russian Jewish immigrants, Don Israel Patinkin grew up in a tightly knit immigrant Jewish community in Chicago. Although he went to local public high school, then college, then the University of Chicago, always he lived a parallel life within the Jewish community. From 4:00 to 8:00 p.m. every afternoon, starting October 1933 and continuing until March 1943, he studied at the local yeshiva, the Hebrew Theological College. During the day, his life was economics (in English), and then in the afternoon it was Talmud (in Yiddish). The pattern continued even after he emigrated. Living in Jerusalem, Patinkin consulted on practical matters at the Finance Ministry during the day (in Hebrew), and returned home in the evening to write on the general equilibrium theory of money (in English). What seemed to one friend a schizophrenic existence, Patinkin himself viewed as "living quietly and happily in two worlds at once."[3]

Notwithstanding his unusual ability to compartmentalize different aspects of his life, inevitably there was a tendency for the compartments to leak one into the other. When two compartments could no longer be kept separate, Patinkin responded by trying to integrate them, making two compartments into one. In Patinkin's mature writing on the history of economic thought, he refers repeatedly to his guiding belief that "man is an inconsistent animal who does not realize the full implication of what he says" (1995, 388). This is the view of a man whose essentially synthetic intellectual project was driven by the psychic need to integrate and synthesize the various dimensions of his own thought and character. In 1945, his thought was to integrate his Jewish life with his academic life by taking an academic position in Palestine.

In February 1946, Marschak wrote a reference letter for Patinkin that is worth quoting for its insight into Patinkin's many-sided character at the time:

> The breadth of Patinkin's mind is characterized by the fact that, while studying economics, he did solid and successful work--receptive, creative, and critical at the same time--in two border fields which usually call for very different talents and are seldom cultivated by the same man: advanced mathematics and historical research. These auxiliary studies have helped him to develop a quiet mastery of his main subject....
>
> Patinkin is young and has potentialities of developing in a great many directions, every one a significant one. He has the promise of becoming a leading

> economic theorist of our time; or he can develop into a man whose advice in matters of economic policy will be listened to. He can become the economic historian of his people, or one of the builders of the economic framework of his country.[4]

The first paragraph highlights the contrasting methodological styles, mathematics and history, that Patinkin would struggle to integrate throughout his life, not least in MIP with its long mathematical and historical appendices (1956, pp. 273-370 and 373-478 respectively). The second paragraph highlights a different contrast, between scholarship (as theorist or historian) and practical affairs (as policy advisor or administrator), but one that posed yet another integration challenge in Patinkin's later life. For present purposes, it is enough to emphasize that, at the time Marschak was writing, all four compartments were already plainly visible but very much separate.

Marschak had firsthand knowledge of Patinkin's potential as a theorist in three courses Patinkin had taken with him the previous year. He would also have seen Patinkin's potential as an economic historian in a paper he had written for John Nef's "Introduction to European Economic History" that would become his first professional publication (Patinkin 1946). The reference to Patinkin's potential as policy advisor and practical administrator has less obvious provenance, but concerns most probably the influence of Henry Simons and Lloyd Mints, an influence that Marschak cannot have wholly approved.

In Marschak's mind, since Patinkin could be a theorist, he should be a theorist. Theorist rather than historian, and theorist rather than practical advisor. However, he plainly knew of Patinkin's other interests and this explains his worry, expressed in the closing paragraph of the reference letter, that "he may underestimate his own abilities and thus waste his best years by accepting work which others, less gifted, can do as well." Marschak had turned down the opportunity to help make the dream of Palestine a reality in favor of pursuing his own dream of making economics an empirical quantitative science. Patinkin was making the opposite choice. The burning question in Patinkin's mind in 1946 was not what kind of economics to build but rather what kind of society to build, and it is through that lens that we must understand his intellectual trajectory.

What kind of society to build in Palestine? One strong contender was the liberal economic order, as imagined and promoted by Patinkin's teacher Henry Simons in his "Positive Program for Laissez Faire" (1934). This is the ideal that eventually won out, as we shall see, but not before Patinkin gave serious consideration to socialist planning, as imagined and promoted by Oscar Lange in his <u>On the Economic Theory of Socialism</u> (1938).[5] Patinkin's attraction to the mathematical economics of Lange, and of Marschak, needs to be understood in this context. It was not about making economics better, but about making the world better. In an unpublished manuscript written probably in 1948, Patinkin wrote:

> Today the people who are mathematical economists are mostly the young economists believing that government must play a larger role in the economic sphere. The connection between this belief and mathematical economics is quite straightforward: a necessary condition for planning is the knowledge of the value of the economic parameters of the system; a necessary condition for measuring these parameters is econometric studies; a necessary condition for econometrics is mathematical economics. Put more briefly: to act, we must know; to know, we

> must measure; to know what to measure, we must have mathematical economics. Thus mathematical economics today is the weapon of those who would modify the system; not those who advocate the status quo.[6]

Youthful exposure to the competing ideals of Simons and Lange proved good preparation for the debate in Israel that Patinkin met when finally he emigrated in February 1949. In an environment where most of his fellow Zionists were socialists if not communists, Patinkin became a strong advocate of the classical liberal position. Following Simons, he saw liberalism not as a natural state or tendency but rather as a deliberate choice, a system that would need constant constructive attention to ensure survival, but nonetheless worth all the trouble. The real issue was not social control versus laissez faire, but rather how practically to use social control to implement and sustain liberalism. Simons' reform plan for the United States had included strong anti-monopoly policy, progressive taxation, credit control through 100% reserve banking, and countercyclical monetary policy. As advisor to the Ministry of Finance in Israel, Patinkin worked to adapt Simons' plan for Israeli conditions.

Why did Patinkin choose for Simons over Lange? One reason is clearly the negative example of Lange's apparent co-optation after leaving Chicago in 1945 to pursue his ideal in Poland (Patinkin 1995, 373). Another negative reason was Patinkin's discovery that his advanced mathematical training was of little practical use for the actual problems facing the nation. But the most important reason was simply his positive discovery of the practical utility of the quantity theory framework he had learned from Henry Simons and Lloyd Mints.

In Patinkin's contemporary account of monetary affairs from 1949-1953 (1954c), the influence of Simons and Mints is ever-present. There are resonances of the 100% reserve proposal, for example, in Patinkin's emphasis on the importance of raising reserve ratios to control private credit. Further, Patinkin's criticism of the tendency of banks to acquiesce to demands for monetization of private credit (p. 72 n. 20) makes explicit reference to Mints' critical analysis of the real bills doctrine. These are specific instances, but more important is the overall approach which deploys the equation of exchange, MV=PT, with great flexibility and even subtlety. Significantly, Patinkin does not assume that causality always flows from exogenous money to prices, and he remains always aware of the extent to which the money supply arises endogenously within the credit system. In Patinkin's hands, the quantity theory is a general framework for macroeconomic analysis, and one especially suited to environments (like Israel) where national income statistics are not available so that the Keynesian income-expenditure approach cannot be applied.

As Patinkin tells the tale, the early years of the Israeli state were marked by inability to borrow on commercial terms combined with inability to reduce spending on defense, immigrant settlement, and economic development. Faced with war finance conditions, the state financed its operations primarily through money issue both directly through the Issue Department, and indirectly through bank borrowing financed by deposit creation, all the while imposing price controls to prevent inflation. The result was suppressed inflation and distortion of relative prices as between the controlled and black markets. A solution was sought by allowing prices to rise, so diminishing real balances and demand pressure, and by controlling new credit. But pressure to maintain real incomes and expenditure continued to show up in excess government borrowing and in continued expansion of bank credit, both to finance government outlay and also to finance private expenditures. In this way, as Patinkin emphasizes, the money supply became an endogenous variable, caused by prior price increases, and the economy lost its nominal anchor.

Building on this early empirical work, Patinkin subsequently broadened his view to include the economy as a whole. With the help of Simon Kuznets, under whom Patinkin studied at Johns Hopkins in 1955-56, he retooled himself as an economic historian in order to serve, starting in 1956, as the first research director of what became the Maurice Falk Institute for Economic Research in Israel. The Fourth Report of that Institute (Patinkin 1959) was a short book The Israel Economy, The First Decade, a tour de force of the economic historian's art that pulled together available data to tell a plausible story of the economic birth of a nation.

In the context of Patinkin's intellectual development, this little book takes on more importance than it has usually been afforded. It must, I think, be understood as Patinkin's completion of the thesis attempt he had been forced to abandon more than a decade before. With the birth of the nation came the birth of systematic data collection, at first rather rudimentary but increasing in sophistication over time. What proved to be impossible for a student at the University of Chicago became possible for the director of a research institute in Israel. Thus, Patinkin did become the economic historian of his people, much as Marschak thought he might, and his scholarship in this respect grew directly from his practical experience as policy advisor and academic administrator, notwithstanding Marschak's worries. It is against the background of this first, apparently integrated, life that we must understand Patinkin's second career, as economic theorist and author of MIP.

The Thesis

To repeat, in the fall of 1946, Patinkin was an idealistic young economist, tooled-up and prepared to begin the project of building a new society, but in need of a thesis. He had been awarded a Social Science Research Council fellowship for the year, and appointed as Research

Associate at the Cowles Commission, so he had some time. What to do with it? Stimulated by conversations with the young econometrician Lawrence Klein, then a newly minted PhD from MIT, Patinkin got the idea to build an econometric model of manufacturing during the interwar years.[7] This work subsequently led him to think about the general theory of involuntary unemployment and he produced a rather Kleinian paper on the subject titled "Unemployment in Keynesian Systems" (December 26, 1946). Patinkin seems to have thought this theoretical paper was done because he submitted it to two journals for publication before leaving Chicago to spend January in New York and Washington, gathering data for the econometric model. From this sequence of events, we can infer that he was planning to finish up the econometric model, submit it along with the theoretical paper and call the thesis finished. But that's not what happened.

At that time, Patinkin was working with H. Gregg Lewis, not Jacob Marschak, but Marschak got hold of a copy of the theoretical paper and took an interest. On January 16, 1947, Marschak wrote to Patinkin in Washington, asking him to withdraw the paper and to submit instead to discussion with Cowles staffers. In February, after Patinkin had returned, there was a staff meeting at which Patinkin presented the paper to Marschak and Klein, but also Leonid Hurwicz, Tjalling Koopmans, Trygve Haavelmo, Ragnar Frisch, and Jack Hartog. The March 6 draft, mentioned above, was a revision subsequent to the staff meeting.[8] This sequence of events leaves no doubt that the entire first part of the thesis was added later, and added because of the intervention of the Cowles staffers. It seems clear that they took an interest in Patinkin and pushed him, as well as helped him, to produce the kind of thesis they could publish as a Cowles Commission Paper. Patinkin was, after all, the most mathematically competent of his graduate student cohort and so must have appeared the most likely recruit.

Thanks to Patinkin's own careful records, it is possible to trace the Cowles influence even more closely. Table 1 provides a chronology, pieced together from the available evidence. It has already been established that the second part of the thesis was written before the first, but now it is important to observe that, within the first part of the thesis, the "macroeconomic" chapters that build on Lange (1944), were written before the "microeconomic" chapter, and this is so even though they were published in the opposite order. Thus, Patinkin wrote first "Unemployment" then "Absolute Prices" then "Relative Prices", but they were published first "Relative Prices", then "Absolute Prices", then "Unemployment".

----------------------------Table 1 about here--------------------------

Furthermore, although Klein was Patinkin's initial interlocutor on "Unemployment", it was Haavelmo who "first stimulated and encouraged my interest in the problems discussed in ["Absolute Prices"]" (1981, 125). Most important, the suggestion to focus on the question of homogeneity came from Haavelmo.[9] Subsequently, it was Arrow who provided most help with "Relative Prices", "whenever my mathematical intuition outran my mathematical competence" (1948a, 135). Patinkin goes so far as to give Arrow explicit credit for proof of the key Theorem XIV (1948a, 151 n. 20).

This chronology of intellectual development and influence helps to interpret later developments. Patinkin did very little to develop his first Kleinian "Unemployment" line of thought, which shows up only as Chapter 13 in <u>MIP</u> (1956).[10] He did even less to develop the Arrow-influenced "Relative Prices" line of thought. Indeed, he rejected that whole line of analysis as "misguided" (1981, 11 n. 18), specifically mentioning Theorems XII and XIV, and offering the rather thin explanation that the theorems put nominal balances, not real balances, in the utility function. This wholesale rejection is quite unlike Patinkin's typical practice, which was to publish corrections and then reprint the corrected paper. In the case of "Relative Prices" he simply dropped the entire thing and moved on. What's going on here?

Marschak (1950, 71) provides a clue. Referring to "Relative Prices", he writes: "Patinkin has proved the inconsistency of traditional microeconomics with positive quantities of paper money. After having contributed this important criticism, he has not attempted to give a better microeconomics improved [sic] upon Walras and Pareto". Marschak proceeds to give his own suggestion of what such a better microeconomics might look like, making reference to "class discussions back in 1946" where the material was first developed. What he wants is a microeconomics that incorporates multi-period decision making under uncertainty, in the presence of "market imperfections, transactions costs, and the property of money as the legal tender" (Marschak 1950, 73). Presumably Patinkin was privy to the 1946 class discussions, and he had engaged that material in "Relative Prices", and he had even served as referee for Marschak's 1950 article, but his heart was apparently never in it. The microeconomic research program was Marschak's, not Patinkin's.[11]

By the time that Marschak presented his paper at the December 1949 meeting of the Econometric Society, Patinkin was already in Israel and there was plenty of evidence that his future research would more likely develop the macroeconomic line of thought suggested by "Absolute Prices". Already in his post-thesis "Price Flexibility" (1948b), Patinkin had crystallized the idea of the real balance effect as the key link between money and real expenditures, a link that he furthermore claimed would fix the inconsistency problem while saving the essential features of the traditional classical analysis. Further, in the "Ten Paragraphs" added to "Absolute Prices" at galley stage[12], Patinkin had already crystallized his Gordian-knot solution to the problem of money demand, which involved putting real balances directly into the utility function. In this way Patinkin essentially avoided Marschak's microeconomic problem by treating it as solved, so that he could move on to the macroeconomic problems that were of more interest to him.

The decision to focus on macroeconomics was also prompted by Patinkin's desire to write for a larger economics audience, not just the small and somewhat marginal (at that time) group of mathematical economists (see Arrow 1979). In attempting to publish his thesis chapters, he had discovered fairly quickly how small was the market, outside the pages of Econometrica, for the kind of thing that his Cowles colleagues admired. Econometrica took "Relative Prices" right away, but "Absolute Prices" only after it was rejected by the Quarterly Journal of Economics. The rejection of "Unemployment" by the Journal of Political Economy stimulated Patinkin to try his hand at a non-mathematical summary which was published as "Price Flexibility" in the American Economic Review. When Marschak declined to push "Unemployment" for Econometrica, and then also declined to reprint "Price Flexibility" as a Cowles Paper[13], Patinkin saw that he could not write for both audiences simultaneously. He would have to choose, and so he did, or anyway so he tried to do.

At the time he left for Israel, only a month after Econometrica published "Absolute Prices", Patinkin had good reason to think that the thesis work was behind him. He had made a few technical criticisms of classical monetary theory, and they were duly noticed by the small group that appreciated that kind of thing. The subsequent vigorous critical response came as a surprise to him, and not a terribly welcome one at that. In defending himself, Patinkin found himself drawn back into a world he thought he had left behind.

Not only did he have to defend himself, he had to do so from afar, hampered by unreliable mail service and lack of regular professional contacts. He was in Israel completely on his own, with no Haavelmo or Arrow to help. What some have noted as Patinkin's tendency to oversensitivity in the face of criticism must be understood in this context of relative isolation from professional circles. If he developed a somewhat paranoid sense that people were talking behind his back, nevertheless the objective fact was that they were. Moving to Israel in 1949 meant that Patinkin's intellectual interlocutors became the remembered voices of his Chicago professors, plus authors he met in books. Patinkin became in effect a kind of time capsule, preserving in his own intellectual makeup the ferment of the years at Chicago from 1941-1947, long after everyone else moved on.

Patinkin responded, and vigorously, to criticisms of his Econometrica pieces (1951a in Econometrica, but also 1950, 1951b, 1954a), backing off from some of his specific claims while sustaining the overall thrust of the argument. But he did not make any further attempt to establish himself within the circle of mathematical economists. Quite the opposite, the whole process of defending himself seems to have soured him on mathematical economics.[14] In the midst of it all, he wrote to a friend:

> I think the whole framework of training in economics is incorrect; too much emphasis is put on purely formal curve drawing. The accolades go to those who think of brilliant devices and mathematical developments with little regard as to whether they have any relevance at all to the real world....It is a scholasticism in which students develop and embellish the theoretical structures of their teachers without anyone feeling the necessity to subject these structures to empirical tests. As a result, over time, the structures grow further and further away from reality until suddenly it is realized that we are talking complete nonsense.[15]

Instead of consolidating his position as an economic theorist, Patinkin turned his attention to the practical problems of building a nation (as we have seen), and to the project that became Money, Interest, and Prices (1956).

Money, Interest, and Prices

According to Patinkin himself, the idea for writing MIP came when he was writing "Keynesian Economics and the Quantity Theory" (1954b), an article that, in its attempt to reach a general economics audience, recalls his earlier "Price Flexibility" (1948). If "Price Flexibility" might well have been titled "Mr. Lange and the Classics" (with apologies to Hicks), the new article might well have been named "Mr. Keynes and the Classics" (more apologies). In both articles, Patinkin argues that his subject's presumed deadly critique of the classics can in fact be accommodated within the classical system. MIP can be understood as pulling both lines of analysis together into a single unified framework.

From this point of view, the central interpretive question must be, what are The Classics? Or, more precisely, how did Patinkin conceive of them? It is clear from Patinkin's intellectual formation that he always began from the "Chicago Tradition" of Frank Knight, Henry Simons, Lloyd Mints and, to some extent, Jacob Viner. Even when he cited Knut Wicksell, or Irving Fisher, or Leon Walras, he always viewed them through the lens of his training. MIP can therefore be read as Patinkin's attempt to integrate not only monetary theory with value theory, but also the new critical perspectives of Lange and Keynes with the older Chicago Tradition.

Patinkin himself says as much in the work leading up to the book. "The Pigou analysis [of the real balance effect] presents in a rigorous fashion part of the theoretical framework implicit in the fiscal-monetary policy of the Simons-Mints position" (1948b, 561, n. 32). "There is no logical reason why a model which is based on a Keynesian "income and expenditure" approach should necessarily yield results differing from those of one based on a traditional "quantity of money" approach" (1954b, 140). The first draft of the book (November 1953) is even clearer: "the preceding analysis can be taken as providing proof in terms of general equilibrium analysis of the validity of the classical quantity theory of money".[16] In MIP, Patinkin intends to reconstitute the old wisdom, using new more rigorous general equilibrium language.

For understanding MIP, it helps therefore to have clearly in mind the outlines of the quantity theory that Patinkin wished to rescue from the critical attacks of Lange and Keynes. Most important, Patinkin's quantity theory was about macroeconomics not microeconomics. The economy as a whole was conceived not as a system in equilibrium but as a system in constant disequilibrium. Equilibrium positions were worth considering, but primarily as reference points against which to evaluate disequilibrium positions (for example, to measure the extent of involuntary unemployment). There was no presumption that the system tended to move from disequilibrium toward equilibrium, and stability was by no means guaranteed. The operations of bank credit were particularly suspect since they were thought to make deviations from equilibrium worse, not better. Money being the other side of bank credit, it was by no means an inessential veil over presumably more fundamental "real" phenomena. Rather the economy was essentially a money economy in which it was impossible to dichotomize the real sector from the money sector. In equilibrium it could be said that money affects only the price level, but the importance of this result was minimal since the real world was never in equilibrium. In the disequilibrium of the real world, money affects and is affected by everything else as well. This is the world view that Patinkin absorbed as a student, the view he brought to understanding monetary disorder in Israel, and the view he was trying to reconstitute in MIP.

This reconstitution project helps to explain the methodological straddle in MIP between the old literary style of economics (which was the style of Patinkin's own intuition), and the new mathematical and analytical style (which was the language of his argument). Notwithstanding Patinkin's sometime negative views on the use of mathematics in economics, he participated actively in the process of replacing the old style with the new by accepting that expression of a theory in the language of general equilibrium constitutes an acid test of the theory's validity. He accepted this premise and used it to argue for the validity of the classical quantity theory. In doing so he wound up in effect arguing also for the validity of the general equilibrium approach. Although this latter implicit argument was tangential to Patinkin's main purpose ("central message"), it accounted for much of the book's success as a graduate text, and its lasting legacy as well.

I have emphasized that the quantity theory Patinkin wanted to reconstitute was fundamentally macroeconomic, but that does not at all mean that he rejected microeconomics. Quite the opposite in fact, since he saw the theory of prices as encompassing both. Micro analysis was done with supply and demand (Marshallian partial equilibrium or Walrasian general equilibrium, both concerned with relative prices) and macro analysis was done with the quantity equation (MV=PT or other variations, all concerned with absolute prices). The important point to keep in mind is that Patinkin's quantity theory was not primarily a theory of the equilibrium price level, and a fortiori not primarily a theory of the long run price level since the long run equilibrating tendency of the economy was precisely the point in question. Rather, his quantity theory was primarily a theoretical framework in which questions of stability could be posed and discussed, as well as questions of possible stabilizing policy intervention.

Viewed from Patinkin's standpoint, Lange's critique seemed to be about how the macroeconomic theoretical apparatus fit together with the microeconomic apparatus, a question that could be most easily posed in an equilibrium context. Patinkin's achievement, as he understood it, was to show how one could bring the two together by treating real money balances as a form of wealth, and as such a factor that influences expenditure. This was an achievement in equilibrium analysis, to be sure, but for Patinkin the real importance of the result lay in its consequences for disequilibrium analysis. With this analytical structure as support one could, for example, provide theoretical foundations for Simons' policy of countercyclical monetary policy. The reason that monetary expansion (brought about by government deficits) causes real expansion is that it increases perceived wealth and so stimulates private spending. It's a different mechanism from the direct government spending multiplier emphasized by Keynes and his American followers, but one with similar implications.

From this same standpoint, Keynes' critique seemed to be about the articulation between his portfolio theory of money demand and the traditional loanable funds theory. Patinkin's contribution, as he understood it, was to show how the theory of liquidity preference could be made consistent with the traditional theory. This move had the auxiliary effect of undermining Keynes' equilibrium theory of unemployment because it undermined the liquidity trap argument, but again, for Patinkin, the real importance of the result lay in its consequences for disequilibrium analysis. For Patinkin in MIP, the real importance of Keynes was in developing the portfolio approach to money demand, an approach that deepened and enriched the traditional analysis, but did not replace it.

It is clear then what Patinkin was trying to do. But it is less clear that he achieved what he intended, or even could have done so. In retrospect, it can be seen that the Walrasian general equilibrium framework was simply not well-suited for the task Patinkin set himself. Most obviously, if the quantity theory is supposed to be about disequilibrium positions, then reconstitution of that theory in a framework that limits attention to equilibrium positions is going to leave out a lot. In Patinkin's mind, the formal equilibrium analysis provided theoretical justification for the informal disequilibrium quantity theory analysis, but that link remained at the level of intuition, a level that Patinkin's audience did not share. Thus, quite against Patinkin's intentions, his book appeared to reconstitute the quantity theory as an equilibrium theory, and as a long run theory as well.

Less obviously, if the quantity theory is supposed to be about money (and bank credit), then reconstitution of that theory in a framework designed precisely to abstract from the monetary character of exchange is going to leave out a lot. Patinkin tried to finesse the problem by treating money as if it were a commodity, which means by putting real balances in the utility function. But even if this finesse had worked at a technical level[17], it remains deeply incoherent at a conceptual level since, as Patinkin himself recognized, the essence of money lies in the way it is <u>better</u> than mere commodities: "Money buys goods, but goods don't buy money" (1956, 1). Again, in Patinkin's mind the formal analysis provided justification for the more flexible informal analysis but, again, that link remained at the level of Patinkin's own private intuition.

Taken at face value, Patinkin's book provided a conceptualization of the central problems of monetary theory rather different from that of Patinkin's classics, and one that moreover proved decisive for shaping the direction of thinking over the next decades. What is money? In

MIP, the most important economic feature of money is that it is an outside asset (like gold) that is costless to produce (unlike gold). From this point of view, the central question for monetary theory is why this mere token has any determinate value at all. This question has remained with us ever since (see Gale 1982 and Wallace 2001, for example), a fundamental question that has never been answered satisfactorily, and one that therefore continues to pose an obstacle to further theoretical development.

Inside Money

One way to conceptualize the history of monetary economics is to see it as a continuing debate between those who emphasize the inside credit character of money (as bank deposits are bank liabilities) and those who emphasize its outside character (as gold is no one's liability). A typical focus for the debate is the question whether it is better to think of hand-to-hand currency as a kind of inside credit (say, a promise to pay gold) or a kind of outside token (paper gold). As a student of both Viner (1937) and Mints (1945, 1946, 1950), Patinkin knew this debate very well. Further, having cut his monetary eyeteeth on Chester Phillips' Bank Credit (1920), he very much understood that the largest part of the actual circulating medium is bank issued inside credit money, and that this part of the currency (Phillips calls it "derivative deposits") is highly elastic. The question then arises, Why did Patinkin place so much emphasis on the relatively small part of the money supply that is issued by the state, and why did he treat this currency as unambiguously an outside asset? Three reasons may be adduced.

First, the outside character of money is absolutely crucial to the real balance effect through which money is supposed to affect expenditure, and so also presumably crucial to the interventionist policy implications of Patinkin's framework. Thus, the outside character of money is crucial to Patinkin's project to reconstitute the quantity theory, and he was bound to insist on it. Note that, because the quantity of state-issued currency is so small, the empirical importance of the real balance effect depends mostly on treating government bonds as net wealth as well. Significantly, Patinkin proceeded to do exactly this, so running roughshod over the qualitative distinction between promises to pay (bonds) and means of payment (money). In Patinkin's mature formulation, all government issue is outside money, and none of it is inside credit.[18]

Second, the 100% reserve proposal for monetary reform can be understood as a proposal to eliminate the instability that arises from inside private credit simply by eliminating inside private credit entirely, so leaving (outside) state-issued money and (outside) privately-issued equity as the only financial assets. Even though Patinkin recognized that, in the real world, state-issued money (including bonds) exists along side privately-issued near-money, he seems to have convinced himself that in an ideal world (Simons' "good financial society") this near-money would be eliminated. In this sense, Patinkin's outside money theoretical construct should be understood not as a positive description of the world, but rather as a normative ideal against which to compare the world. (In a very similar way, Patinkin always insisted that we need a correctly specified model of full employment as a reference point for understanding the involuntary unemployment we see around us.)

Third, and I think most important, at the time Patinkin was writing, it just seemed realistic to treat state-issued money as an outside asset. Given the breakdown of the gold standard, and of international monetary arrangements more generally, during the depression of the 1930s and a fortiori during the world war that followed, for most people national paper currency was the best money there was, the end of the line and not a promise to pay something better. Thus, it just made sense, not only to Patinkin but also to his critics, to build monetary theory on that assumption. And it made even more sense to Patinkin, given his daily monetary experience in Israel where the evaporation of foreign reserves had removed any possibility of commercial acceptance of the Israel pound in international exchange.

This last reason especially explains why practically everyone viewed the idea of constructing monetary theory by analogy to the theory of value as a plausible road forward for the subject. If money is an outside asset, issued by the state for reasons of state (whether war finance or economic stabilization) and not as part of any regular commercial process, then that explains supply and we had best be getting on with the task of understanding the determinants of demand, which means the theory of portfolio choice (as Marschak et al.). Then, once we have described supply and demand in the money market under ceteris paribus assumptions about other markets, we should be able to describe general equilibrium in all markets at once (as Patinkin). Looking back, we see that the foundational assumptions of this research program were based on rather specific historical circumstances that began to change as soon as the war ended, though less soon in places like Israel that remained effectively on a war footing.

The first sustained challenge to the outside money point of view came from John Gurley and Edward Shaw's book Money in a Theory of Finance (1960) which sought to develop the inside money view for the postwar world.[19] In the book, the principal (though unnamed) antagonist was Patinkin, whose net-money (outside money) view Gurley and Shaw sought to contrast with their own gross-money (inside money) view. Chapter 2, titled "Rudimentary Finance", was intended to paraphrase the model of Patinkin's MIP, preliminary to demonstrating its inadequacy. In recognition of that fact, Patinkin was asked to review the book for the

American Economic Review, and his review (Patinkin 1961) became standard reading in the graduate economics syllabus.

Patinkin of course defended himself, and he did so on the turf, so much more familiar to him than to Gurley and Shaw, of general equilibrium theory. In effect, he used the opportunity to extend his own analysis, and so to show that the criticism of Gurley and Shaw was no more devastating to the classical wisdom than was the criticism of Lange or Keynes. It was a resounding success for Patinkin but, it has to be said, not such a great success for economics. In retrospect, the two sides never really engaged, mostly because they were constrained by the ground rules of general equilibrium theory, rules that both sides accepted though for different reasons.

No one who reads only MIP (1956), then MTF (1960), and then Patinkin's review (1961) would ever sense the substantial area of fundamental agreement between Patinkin and Gurley-Shaw (see Table 2). Both sides viewed the economy as essentially monetary, perpetually in disequilibrium, and potentially improvable by correct monetary policy. Both sides also approached monetary economics as a macroeconomic, not a microeconomic, discipline and both rejected monetary theories of interest in favor of a real theory. This is a lot of common ground. But both also adopted the general equilibrium approach as a framework for expressing their thoughts, and this made it difficult for them to see the substantial area of agreement, and equally difficult to identify the significant area of disagreement, where progress might have been made.

--------------------------------Table 2 about here-----------------------------

In retrospect, we can see that what was really at stake between them was the question of the proper role of private credit, and more specifically the role of commercial banks as private credit intermediaries. Patinkin came out of an intellectual tradition that saw private credit as a key source of economic instability, and commercial banking therefore as something to suppress by imposition of 100% reserves. Shaw, by contrast, saw private credit as a key positive ingredient in economic growth, and commercial banking therefore as something to encourage. These two positions are potentially compatible since one concerns short run fluctuation and the other long run growth, but it was impossible to see this compatibility given the ground rules of the debate. Though he was mainly interested in short-run monetary non-neutrality, Patinkin had written a book defending long run monetary neutrality, which position Gurley and Shaw felt compelled to attack since they saw monetary constraint as a constraint on the growth of commercial banking, and hence also a constraint on real economic development and growth.

You would never know it from their book, but Gurley and Shaw actually developed their monetary theory inductively from immersion in the data of US economic history. Such an approach was of course worlds apart from the Patinkin of MIP (1956), but not so far from the Patinkin of "Monetary and Price Development in Israel: 1949-1953" (1955). Had Gurley and Shaw chosen to express themselves in economic historical language (as they did in various articles written in preparation for the book), they might have succeeded in engaging a different Patinkin, and having a useful conversation about the very different experiences of the US and Israel. But they chose to engage the Theorist, and so drew Patinkin back into the mode of his earlier Econometrica article defense, in which mode Gurley and Shaw were relatively easy opponents.

There is a further irony here. Patinkin's book came to be viewed as a theoretical bulwark of monetarism, and Gurley and Shaw's as a bulwark of Keynesianism, both quite against the author's original intentions. Gurley was rather Keynesian so this appropriation probably did not disturb him so much, but Shaw was always strongly anti-Keynesian. Indeed, Shaw's reservations about the workability of short-run stabilization policy even led him to promote that quintessential monetarist policy, constant money growth! And Patinkin, for all his doubts about the logical coherence of unemployment equilibrium, was actually rather Keynesian in his policy views. What an interesting conversation it would have been had Patinkin and Shaw ever really engaged. Instead we got the debate between Friedman and Tobin, a debate moreover that took place entirely within the framework that Patinkin did so much to establish, albeit with quite a different intention in mind.

Conclusion

MIP was supposed to reconstitute the quantity theory, and in such a way that the new Keynesian themes could be understood as continuous with that older tradition. Because Patinkin understood the quantity theory as being essentially about disequilibrium positions around a likely unstable equilibrium reference point, he advanced a disequilibrium interpretation of Keynes. In Patinkin's version of Keynes, the main problem is dynamic failure of the equilibrating process. Because the economy is typically in disequilibrium, there is room for effective demand to play a role.

The rise of the general equilibrium approach left the followers of Keynes with an obvious task, to reconstitute the master within a general equilibrium framework. The result of that reconstitution project was the Hicks-Hansen-Modigliani fixed-price equilibrium model, from which generations of students have learned their Keynes. Patinkin's disequilibrium interpretation of Keynes, by contrast, never attracted much of a following, and in retrospect it is easy to see why. Patinkin was, after all, engaged in a fundamentally different project. Everyone else was reconstituting Keynes, but Patinkin was reconstituting Simons-Mints-Knight. The two projects shared an intellectual strategy, namely reformulation in the language of general equilibrium, but they had different ultimate intellectual goals. It has become a staple of the Keynesian literature to express concern about how that reformulation distorted the Keynesian message. In the preceding pages, I have suggested how in Patinkin's hands this same reformulation strategy wound up distorting the Simons-Mints-Knight message as well.

After the second edition of Money, Interest, and Prices (1965), Patinkin turned his attention more and more to the history of economic thought. We can understand these researches, in part, as a search for the origins of the Simons-Mints-Knight world view, so clear in his memory and so much a part of his intuition, but so distant from MIP. At first he viewed Simons as the central figure (Patinkin 1972), but further inquiry showed Knight also as a central intellectual pole (Patinkin 1973). Uncharacteristically, Patinkin never followed up the reference in Knight to Allyn Young as "one of the ablest writers on monetary problems in modern times".[20] Nor did he apparently appreciate that Arthur Marget, whose magisterial Theory of Prices was Patinkin's starting point for understanding the history of monetary thought, was also a student of Young.[21] Even Shaw was a student of Young once removed, since his teacher and lifelong colleague Bernard Haley had been Young's student. Had Patinkin followed up on Knight's suggestive lead, he might have gained a greater appreciation for the way in which his beloved Chicago Tradition grew from deep roots in the distinctively American institutionalist tradition.

Instead, he turned his attention to Keynes (Patinkin 1976, 1982). Ever since he first read Keynes in graduate school, Patinkin saw the General Theory not as a radical break but rather as quite continuous, at least with the past he knew which means the Old Chicago School of Simons-Mints-Knight. The problem was that practically no one else agreed with him on this.[22]

Simons and Mints themselves were both strongly anti-Keynesian, and most Keynesians (certainly most UK Keynesians) were strongly anti-Chicago. After MIP, Patinkin's strategy for establishing the viability of a Keynesian quantity theory was first to elucidate the Old Chicago School, and then to elucidate Keynes. Both explorations can be understood as attempts to find the empirical origins of his strong intuitive sense of continuity and compatibility between these two dimensions of his own world view.

Patinkin's search for the origins of his own world view eventually brought him to reflect on other dimensions of his origins as well (1981, 3-20). But it was only in his last paper, published posthumously (Patinkin 1995), that he wrote openly about his ten years at yeshiva, and even then he wrote only in an appendix, not integrated into the main text. Apparently he was still unsure about just how this formative experience had influenced him, but he leaves us with at least one very good hint. He claims that, as a yeshiva student, he became impatient and annoyed with the Talmudical interpretive device of rendering consistent all of an author's statements, and suggests that this was the origin of his mature appreciation for the ubiquity and persistence of inconsistency. What can we make of this?

Certainly the chronology he suggests cannot be right. The young Patinkin was a man very much in search of consistency, and very much in love with the Talmudical device in this respect. We find him in 1950, for example, in the midst of the debate about his Econometrica articles, relaxing in an exchange of letters about Alfred Marshall with his old friend Milton Friedman:

> I must tell you that I found your article a thoroly enjoyable exercise in the Talmudical method. It even goes as far as employing a typical Talmudical device

> to meet an apparent contradiction in the words of one of the sages: that Rabbi Marshall held one opinion in his youth, and another in his age! And you also accept the fundamental Talmudical proposition that at one and the same time the sage could not have been inconsistent![23]

There is nothing here of the tone of impatience and annoyance that he retrospectively recalls!

Patinkin's autobiographical reconstruction should be understood, I think, not as a statement of historical fact but as a statement of psychological fact. He was at the end of his life coming to terms with what he perceived as his own inconsistency, an inconsistency that persisted despite all his attempts at integration. Patinkin was, quite simply, a man of parts--American and Israeli, historian and theorist, thinker and doer, Simons and Lange, inside and outside money, and the list could be extended. Wasn't it too much to expect that he could integrate all the parts into a consistent whole? This, I think, is what he was trying to plead.

It seems a correct self-understanding, so far as it goes, but it doesn't go far enough in at least two dimensions. First, it fails to appreciate the fructifying aspects of inconsistency, which arise when the mind becomes aware of inconsistency and searches for a way to resolve it. The dynamic quality of Patinkin's intellect issued, I think, from the presence of inconsistency and his drive to resolve it. Even more, because the inconsistency in his mind reflected inconsistent strands within the social setting that produced him, his drive for resolution had social as well as personal value. In trying to resolve the inconsistency in his own mind, he was at the same time pushing our economic understanding forward.

Second, in emphasizing the inconsistency in the mind of the individual author, Patinkin fails to appreciate the underlying assumption of consistency in the mind of the Author whom we can never fully know. The world is apparently orderly, or so anyway we must presume, though its laws remain obscure. It is our job, as economist rabbis, to interpret the laws of the economic world as best we are able. Economic history is our primary text, and the history of economic thought is the record of past attempts to interpret that text. Inconsistency--whether empirical inconsistency between the facts and our interpretations, or logical inconsistency within and between different interpretations--shows us where our understanding still falls short and more work needs to be done. Understood in this way, Patinkin's economic history, his economic theory, and his history of economic thought can be understood as a unified research program, more unified as it appears to us in retrospect looking in from outside than it felt to Patinkin as he lived it.

Notes

References

Arrow, Kenneth J. (1979). Jacob Marschak. Pages 500-7 in International Encyclopedia of the Social Sciences, Biographical Supplement.

Barkai, Haim, Fischer, S., and Liviatan, N. (1993). Monetary Theory and Thought, Essays in Honour of Don Patinkin. London: Macmillan.

Friedman, Milton (1956). The Quantity Theory of Money--A Restatement. In M. Friedman (ed.), Studies in the Quantity Theory of Money, Chicago: University of Chicago Press.

Gale, Douglas. 1982. Money, in equilibrium. New York: Cambridge University Press.

Gurley, John G. and Shaw, Edward S. (1960). Money in a Theory of Finance. Washington, DC: Brookings Institution.

Hahn, F.H. (1965). On Some Problems of Proving the Existence of an Equilibrium in a Monetary Economy. In F.H. Hahn and F.P.R. Brechling (eds.), Theory of Interest Rates, London: Macmillan.

Johnson, Harry G. (1969). Inside Money, Outside Money, Income, Wealth and Welfare in Monetary Theory. Journal of Money, Credit, and Banking, 1: 30-45.

Laidler, David (1969). Money, Wealth, and Time Preference in a Stationary Economy. Canadian Journal of Economics, 2: 526-35.

Lange, Oscar (1938). On the Economic Theory of Socialism. Minneapolis: University of Minnesota Press.

______. (1944). Price Flexibility and Employment. Bloomington, Ind.: Principia Press.

Makower, Helen and Marshak, Jacob (1938). Assets, Prices and Monetary Theory. Economica, 5: 261-288.

Marget, Arthur (1938, 1942). The Theory of Prices, A Re-examination of the Central Problems of Monetary Theory. 2 vols. New York: Prentice Hall.

Markowitz, Harry (1952). Portfolio Selection. Journal of Finance, 7: 77-91.

Marschak, Jacob (1938). Money and the Theory of Assets. Econometrica, 6: 311-325.

______. (1943). Money Illusion and Demand Analysis. Review of Economic Statistics, 25: 40-48.

______. (1949). The Role of Liquidity under complete and incomplete information. American Economic Review, 39: 182-95.

______. (1950). The Rationale of the Demand for Money and of 'Money Illusion'. Metroeconomica, 2: 71-100.

Mehrling, Perry (1997). The Money Interest and the Public Interest: American Monetary Thought, 1920-1970. Harvard Economic Studies #162. Cambridge: Harvard University Press.

______. (1999). The vision of Hyman P. Minsky. Journal of Economic Behavior and Organization, 39: 129-158.

Mehrling, Perry and Sandilands, Roger (1999). Money and Growth: Selected Essays of Allyn Young. London: Routledge.

Mints, Lloyd W. (1945). A History of Banking Theory. Chicago: University of Chicago Press.

______. (1946). Monetary Policy. Review of Economic Statistics, 28: 60-69.

______. (1950). Monetary Policy for a Competitive Society. New York: McGraw Hill.

Patinkin, Don (1946). Mercantilism and the Readmission of the Jews to England. Reprinted as Patinkin (1981, 75-89).

______. (1948a). Relative Prices, Say's Law, and the Demand for Money. Econometrica, 16: 135-54.

______. (1948b). Price Flexibility and Full Employment. American Economic Review, 38: 543-64. Reprinted with revisions in Patinkin (1972, 8-30).

______. (1949a). The Indeterminacy of Absolute Prices in Classical Economic Theory. Econometrica, 17: 1-27. Reprinted in Patinkin (1981, 125-148).

______. (1949b). Involuntary Unemployment and the Keynesian Supply Function. Economic Journal, 59: 360-383. Reprinted in Patinkin (1981, 155-79).

______. (1950). A Reconsideration of the General Equilibrium Theory of Money. Review of Economic Studies, 18: 42-61.

______. (1951a). The Invalidity of Classical Monetary Theory. Econometrica, 19: 134-51

______. (1951b). Further Considerations of the General Equilibrium Theory of Money. Review of Economic Studies, 19: 186-95.

______. (1954a). Dichotomies of the Pricing Process in Economic Theory. Economica, 21: 113-28.

______. (1954b). Keynesian Economics and the Quantity Theory. In K. Kurihara (ed.), Post-Keynesian Economics. New Brunswick, NJ: Rutgers University Press.

______. (1954c). Monetary and Price Developments in Israel: 1949-1953. Reprinted in Patinkin (1972, 55-82).

______. (1956). Money, Interest, and Prices; An Integration of Monetary and Value Theory. Evanston, Illinois: Row, Peterson and Co. (1965). 2nd Ed. New York: Harper and Row

______. (1958). Secular Price Movements and Economic Development: Some Theoretical Aspects. In The Challenge of Development, edited by A. Bonne. Jerusalem.

______. (1959). The Israel Economy, The First Decade. Jerusalem: Maurice Falk Institute for Economic Research in Israel, 1967.

______. (1961). Financial Intermediaries and the Logical Structure of Monetary Theory. American Economic Review, 51: 95-116.

______. (1972). On the Short-Run Non-Neutrality of Money in the Quantity Theory. Banca Nazionale del Lavoro Quarterly Review, 100: 3-22.

______. (1972). Studies in Monetary Economics. New York: Harper and Row.

______. (1973). Frank Knight as a Teacher. Reprinted in Patinkin (1981, 23-51).

______. (1976). Keynes' Monetary Thought; A Study of its Development. Durham, North Carolina: Duke University Press.

______. (1981). Essays on and In the Chicago Tradition. Durham, North Carolina: Duke University Press.

______. (1982). Anticipations of the General Theory and other essays on Keynes. Chicago, Illinois: University of Chicago Press.

______. (1995). The Training of an Economist. BNL Quarterly Review, 195: 359-95.

Simons, Henry (1934). A Positive Program for Laissez Faire. In H. Simons, Economic Policy for a Free Society. Chicago: Chicago University Press, 1948.

Tobin, James (1958). Liquidity Preference as Behavior toward Risk. Review of Economic Studies, 25: 65-86.

Viner, Jacob (1937). Studies in the Theory of International Trade. New York: Harper and Bros.

Wallace, Neil (2001). Whither Monetary Economics? Mimeo, Pennsylvania State University.

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[1][1] All cited unpublished material is from the Patinkin Papers which are held at Duke University in the Rare Book, Manuscript, and Special Collections Library. Citations indicate box and folder within that archive. Thus, the March 6 thesis draft is at Box 4, Folder "Reconsideration of the Theory of Unemployment".

Of the six chapters in the March 6 draft, Chapters II-VI correspond to Chapters VI-X in the thesis. Chapter I of draft, which ran to a scant 7 pages, is the only one to mention money, and was clearly intended merely as an introduction to the wage bargaining chapters. By contrast, Chapters I-V of the thesis run 49 pages.

[2][2] Box 77, Folder "Autobiography--Raw Materials".
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[4][3] Box 25. Patinkin to Martin Bronfenbrenner, September 30, 1953.
[5]
[6][4] Box 77, Folder "Autobiography--Raw Materials".
[7]
[8][5] In this regard, it is interesting to compare Patinkin with Hyman Minsky, also a child of Russian Jewish immigrants, and also a devotee of both Simons and Lange as an undergraduate at the University of Chicago, two years ahead of Patinkin. Unlike Patinkin, Minsky was more socialist than Zionist. Also, Minsky interrupted his economics studies to go to war, and returned to complete the PhD at Harvard in 1954. In spite of these differences Minsky, like Patinkin, wound up doing his most influential work in the monetary field. For details, see Mehrling (1999).

[9][6] Box 28, Folder "MIP-1949 beginnings". The manuscript is apparently an introduction to the book that Patinkin was trying to put together before leaving for Israel, and it is this that suggests the 1948 date.

[10][7] Box 4, Folder "Manufacturing paper--thesis attempt". "Preliminary Model for Manufacturing in the United States" October 11, 1946; "Market Adjusting and Inventory Equations", December 4, 1946.˜

[11][8] Box 4, Folder "Unemployment in Keynesian Systems" and "Reconsideration of the Theory of Unemployment."

[12][9] Patinkin (1981, 132 n. 4) indicates that Section 9 of the paper follows the lines laid out in an unpublished note on homogeneous functions by Leonid Hurwicz.

[13][10] In a retrospective look at his own work, Patinkin judged this relatively underdeveloped line to have been more significant, finding links with Clower's dual decision hypothesis and the literature that grew from it (1989, xvi-xx).

[14][11] There is a straight line from Marschak's 1938 "Money and the Theory of Assets" to his 1949 "Role of Liquidity under Complete and Incomplete Information" and 1950 "The Rationale of the Demand for Money and of 'Money Illusion'". Important works that follow Marschak's preferred line include Harry Markowitz "Portfolio Selection" (1952) and James Tobin "Liquidity Preference as Behavior Toward Risk" (1958).

[15][12] On the ten paragraphs see Patinkin (1995, 381-4). The ten paragraphs substituted for material in Chapter V of the thesis, "Modifications of the Classical System".

[16][13] The grounds for the first were that Patinkin already had two pieces there and, "As you know, it is desired to avoid the impression that Econometrica is the house organ of the Cowles Commission." The grounds for the second were that the paper "really was not in the field of mathematical economics". Box 28, Folder "Marschak, Jacob".

[17][14] See correspondence in "Cowles Commission" Box 26, and "Milton Friedman" Box 27.
[18]

[19][15] Box 27. Patinkin to Alexander Morrell Henderson, June 18, 1950. See also the very similar paragraph in Patinkin to Martin Bronfenbrenner June 22, 1950.

[20][16] Box 9, Folder "Money, Interest, and Prices". Chapter Four, "The Quantity Theory Reexamined", p. 1.

[21][17] Hahn (1965) raises serious questions on that score.

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[23][18] See Johnson (1969) and Laidler (1969) for sympathetic attempts to produce a more reasonable formulation.

[24][19] For an account of Shaw's life and intellectual development that puts the book in context, see Mehrling (1997, 159-219). Gurley and Shaw began the project that culminated in the book in 1953, more or less at the same time that Patinkin began his own, but Patinkin finished first, and that in itself delayed Gurley and Shaw. Originally Gurley and Shaw were planning to attack the quantity theory, as "restated" in Friedman (1956). On March 26, 1958, however, Shaw wrote: "rewrite necessitated by the broad interest in Patinkin's book. His volume is the definitive statement of views that oppose our own, and must be met head on" (Calkins Papers, Brookings Institution).

[25][20] Box 1, Folder "Knight , Unpublished manuscripts". "Money", June 5, 1944.

[26][21] On Marget, see Mehrling (1997, 154-56). On Young, see Mehrling (1997, 13-81) and Mehrling and Sandilands (1999).

[27][22] Except Marget, to some extent. The influence of Marget on Patinkin remains an open question worthy of further exploration.

[28][23] Box 27, Patinkin to Friedman, September 9, 1950.