Sidney Weintraub and American Post Keynesianism: 1938-1970

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Abstract

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Sidney Weintraub (1914 – 1983) was an American economist who spent most of his career at the University of Pennsylvania. A distinguished economic theorist (and the author's father), he was a co-founder of the *Journal of Post Keynesian Economics*, and the leading figure in the US in the early years of the Post Keynesian movement. This article shows how the early development of American Post Keynesianism, despite claims to the contrary by historians of Post Keynesianism, had no connection to the UK group centered around Joan Robinson in Cambridge.

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Early Background

My understanding of Sidney's transition from mainstream economist to Post Keynesian economist tracks back to his earliest work in microeconomics, or what was then called price theory. He started as a price theorist, a Marshallian theorist who wrote a long doctoral dissertation on questions of market structure, competition, monopoly pricing and so on. That project took up his time from the period that he was in graduate school at NYU in the late 1930s through the year spent as a special student at the London School of Economics in 1938-39. It was at LSE that he got to read the economics literatures, and also came into contact with Keynesian ideas through discussions, meetings, and collegial talks with individuals like Nicholas Kaldor and Abba Lerner.

Returning to the US before the outbreak of war (he had decided not to try for the LSE degree instead of the NYU degree – it was a matter of money) in summer of 1939, Sidney had a succession of jobs in finance and money areas following on some internships and research assistant work that he had done through his connection with some of his NYU mentors. This was really financial journalism as well as research on monetary policies and practices in the banking system[1]. I know that he worked at the New York Fed editing their newsletter around 1940-41 (where he met his to-be-lifelong friend, Arthur Bloomfield). He also did a stint at the Treasury in 1941-42 doing essentially financial/macroeconomic work as I understand it. So though he in the early 1940s was primarily a theorist in what we consider microeconomics, he was the same time conversant with macroeconomics[2].

Following wartime service, his book *Price Theory* was published in August 1949 while Sidney was teaching at St. John's University, which was at that time located in Brooklyn. He taught lots and lots of courses and lots and lots of hours and among those courses was a two semester course in "Economic Analysis, Parts I and II". It appears that in Part II Sidney was teaching "Income and Employment Analysis" which was title of the book that he published in March 1951, a book which today we would call an intermediate macro book[3]. As that book

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[1] That was why later, in the 1950s, there was a period when he was writing on the bond market, doing a regular column on forecasts for bond prices in a magazine called *Business Scope* which was edited by Arnold Soloway out of the Harvard Business School.

[2] His astonishing publication record during this period is certainly linked to his doctoral dissertation: "The Classification of Market Positions: Comment". *The Quarterly Journal of Economics*, Vol. 56, No. 4 (Aug., 1942), pp. 666-673; "Monopoly Pricing and Unemployment". *The Quarterly Journal of Economics*, Vol. 61, No. 1 (Nov., 1946), pp. 108-124; "Price Cutting and Economic Warfare". *Southern Economic Journal*, Vol. 8, No. 3 (Jan., 1942), pp. 309-322; "The Foundations of the Demand Curve". *The American Economic Review*, Vol. 32, No. 3, Part 1 (Sep., 1942), pp. 538-552; "Monopoly Equilibrium and Anticipated Demand". *Journal of Political Economy*, Vol. 50, No. 3 (Jun., 1942), pp. 427-434.

[3] This was the teaching that got him into trouble with conservative *Brooklyn Eagle* newspaper one of whose columnists complained about Sidney's teaching Keynes and socialism to the nice

shows, Sidney was fully engaged with macro, and was already a Keynesian. That transformation had occurred for him in the 1938-39 LSE year (much influenced by Nicholas Kaldor and Abba Lerner, although Lionel Robbins roughly supervised his study) and it had great import because Sidney had voted for Alf Landon in 1936(!), and my mother had refused to marry him until he 'got some sense' in her view and became a Roosevelt Democrat. He may not have become a liberal democrat but he did become a Keynesian and my parents subsequently married in August 1940. Nevertheless, when he was hired at the University of Pennsylvania in 1950 it was to teach price theory and, strangely given his lack of mathematical knowledge, mathematical economics. He quickly moved into teaching, instead of mathematical economics, income and employment analysis; I do not know when that course was eventually titled Macroeconomic Theory. So from the earliest point in his career at Penn, he was teaching basic graduate micro theory and macro theory using his own books.

My impression is that with one foot in each of micro and macro, from the early 1950s Sidney's own understanding was engaged with the questions of income distribution which he saw as both micro and macro in part. This was the McCarthy period in the U.S. when the first edition (1947) of Samuelson's *Principles of Economics* did not discuss theories of income distribution, thus avoiding the accusations being made to the MIT Board of Overseers that Samuelson's book was going to be was a socialist tract. This led to Sidney's long quest for a synthesis of micro and macro. His idea, that this synthesis was to be found through the theories of the distribution of income, eventuated in his book *An Approach to the Theory of Income*

Catholic boys. Sidney's dean promoted him to tenure on the spot in order to protect him, which was rather courageous for a Catholic administrator in that McCarthy period.

Distribution which appeared in 1958. It was in those years in the early to mid 1950s that he wrote the papers on microfoundations of wage theory, and he seemed to be engrossed in thinking about microfoundations. Of course it was not the case that microfoundations as a topic was unknown. The link between general equilibrium theory and macro in the guise of monetary theory had been a live topic and had been pursued by a number of different individuals in this period (e.g. Lange, Mosak, Bronfenbrenner, Klein, and Patinkin) until it was finally, for most economists, settled by Patinkin's book *Money, Interest, and Prices* (1956).

Feeling Marginalized by the "Mainstream"

Some of Post Keynesian negativity/anger toward the "mainstream economics profession" in the U.S. results I believe from their self-perceived marginalization by mainstream economists and institutions. Such paranoia has to be rooted in something real, and my view is that it was very specifically rooted in Sidney Weintraub's persona. Sidney saw himself as either ignored or marginalized by mainstream economists in his attempts to write and publish *An Approach to the Theory of Income Distribution* (1958). As a full professor at the Ivy League University of Pennsylvania, with extensive publications in the "best" journals to his credit, with two books and regular outside consulting, Sidney had thought of himself as having arrived professionally. Sidney saw his new volume as his own major contribution to economics. His development in that book of the aggregate supply/aggregate demand framework was, he believed, what Keynes had really "meant" in the *General Theory*.

This book project consumed Sidney in the mid 1950s, which was also a time Sidney's mathematician brother Hal was dying of Hodgkin's disease. Hal had helped Sidney with some of the mathematical derivations in the income distribution book, and Sidney was crushed by Hal's death at age 31. He was in an emotionally very vulnerable state at that time and saw his book as a very personal expression of his connection to his brother (to whom it was dedicated) as well as his own final triumph in the field of economics.[4]

For his first two books Sidney had had as publisher the English firm Pitman and Sons, who did not wish to publish any more economics, so Sidney sought a new publisher for his *Approach to the Theory of Income Distribution* volume. He tried McGraw Hill. He was unsuccessful. I would assume that in his papers there are correspondence files about his attempts to get a book contract, and hostile readers' reports recommending against publication. By and large, as Sidney would tell the stories to me, it was that nobody wanted to hear about such deviant ideas: specifically there was a Keynesian mainstream and Paul Samuelson was at the head of it. These were the American Keynesians and Sidney believed that his own work on macro in the *Approach* volume undermined their understanding of Keynes. Of course he never did find a real publisher, and so one day he walked into a publishing company that was just off the University of Pennsylvania campus, one that he passed driving to and from the office each day. It was Chilton Incorporated, the largest publisher of automobile service and repair manuals in the world. Somehow, Sidney persuaded

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[4] I've written about this before, in my chapter "Sidney and Hal" in my 2002 *How Economics Became a Mathematical Science*. I reproduced there the long blank verse poem Sidney wrote, but never published or shared with anyone, as a memorial to Hal.

them to go into economics publishing with him as the general editor of their economics list, handling all marketing, editing, scheduling etc., and thus publishing his book. They did so.

The book appeared, and the reviews were mixed. Sidney was battered in a number of them, including one from Abba Lerner whom Sidney had considered a good friend. That was very difficult for him to take. This then was the state of his mind in the late 1950s with respect to micro/macro. He believed that the establishment, in his mind centered in Cambridge Massachusetts or at the *American Economic Review*, had rejected his ideas because they were too threatening to the prevailing Keynesian orthodoxy. It was during this time that Sidney's paranoia, which had been present as near as I can tell through his entire life (see my quoting in my 2002 book of various of his wartime letters to my mother for how different officers were out to deny him promotions, etc.), was on display. In this period an emotionally labile Sidney was to thrash about in a number of ways, professional and personal.

Inflation Analysis, and Post Keynesian Beginnings

This was about the time in the late 1950s that there was a nascent concern with inflation in the US. In the very late 1950s, the United States had a bout of simultaneous inflation and unemployment. In response to this inflation "problem", in 1959 Sidney produced what he believed at the time to be a revolutionary book which he titled *A General Theory of the Price Level, Output, Income Distribution, and Economic Growth*. The title, pace Keynes, suggests his own view of its importance. He wrote that book in a four day period in 1959[5] when I was still living at home as a high school student. What happened was that in Sidney's income distribution work, he had employed an assumption of a constant money wage in his initial derivation of the aggregate supply curve, and which he then generalized to the case of variable money wages in his derivation. Following that derivation of aggregate supply, and demand, and the level of effective demand, one could infer that there were reasons for expecting a constant wage share out of gross business product. Playing around with that definition one day, Sidney started moving terms around in an income/expenditure identity and produced a formula which tied the rate of change of the price level to the rate of change of money wages minus the rate of change of the average productivity of labor. This truism became an equation if one assumed a constant wage share.

Sidney believed he had found the Holy Grail itself, equivalent to the discovery of the Equation of Exchange and the Quantity Theory of Money that developed from the assumption of the constant velocity of money. This episode led Sidney into a reverie which became a manic four day period with almost no sleep at all during which time he constructed the statistical background and wrote the chapters and edited and re-edited the manuscript. As a high school student with one semester of high school physics, I was enlisted to write about gravitation constants and how one turns an identity into a theory through a process of measurement. I of course knew nothing, but I complied. The relation that Sidney claimed to have discovered based on the constant wage share he dubbed the WCM equation which he described in that

[5] The preface was dated March 1959, so that the book was composed probably over the Penn semester break in late January 1959.

text as the Wage Cost Mark-up equation. He told me, but I don't think to any others though I might be wrong, that he expected it to be called, over time, Weintraub's Constant Mark-up equation.

The book came out right in the period of simultaneous inflation and unemployment. In the summer of 1959 Sidney went to the University of Minnesota economics department, then chaired by Walter Heller, to give lectures on inflation theory based on his new book. Sidney became a champion of a money wage - productivity based theory of inflation in strict contrast to what he considered to be the Keynesian demand pull or monetarist money expansion views.

The book reviews of his "General Theory" came out the following year, and they were brutal. His old friend and ally Abba Lerner called his theory a mere variant on his own 'sellers' inflation' and suggested that inflation theory does not, cannot, fit into Keynes' theory. Ralph Turvey's review noted that "Professor Weintraub has enunciated a truism and observed a constancy ... One definitional equation and one exogenous variable do not make much of a theory." And it went downhill from there. Consequently in the very early 1960s he was very depressed, and he lashed out at many people. He effectively abandoned economics and took up golf. He didn't know what else to do.

It was during that time, in September 1960, that I went off to Swarthmore College 15 miles down the road from home. I was no longer around to witness Sidney's activities on a daily basis until 1964, except for his investment of his own energy in my career and his directing my energies to repair, with me standing in for him, the mathematical deficits which he believed had so marginalized his ideas and writings. He accepted few boundaries between us.

Finding Allies in the US

Let me now pick up two other threads. Sidney's first trip overseas since his wartime experience (which had followed on his LSE experience) occurred in 1957[6]. It was then that he spent a short period of time, perhaps a month, visiting Oxford on sabbatical (it would have been his seventh year at Penn as a regular faculty member) where he met Sir Roy Harrod.[7] That connection with Harrod continued. I don't know exactly what Sidney was doing at Oxford, my guess is that he had come to talk about his *Approach to the Theory of Income Distribution* with people there, and that Nuffield College had some kind of financial support for him: Sidney's connections, I believe, were personally more with Francis Seton and folks at Nuffield. One of Sidney's jobs there was, as delegated by Penn's chairman Irving Kravis, to attempt to entice Larry Klein to return to the United States. In this he was successful.

The second thread was that in the late 1950s Sidney finally had graduate students who could carry on his work. He thanks Paul Davidson and Eugene Smolensky in his *General Theory* book, and they were both writing Penn dissertations in the late 1950s, the former with Sidney. He began, in that period when his own ideas were being rejected, seeing individuals, and not just economists, as either with him or against him, and this self-constructed Manichean vision of the economics profession was to endure. Its proximate cause was the professional failure of

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[6] In Colander's *EEJ* interview with Paul Davidson, Davidson claims that Sidney wrote the income distribution theory book in England in 1954. That is of course not true, as he did not go to England until several years later.

[7] He could not leave home for a longer period of time since my mother did not drive, I was too young to drive, and my brother was only six.

his magnum opus, and the failure of his caricature of a second magnum opus, all within three years. Underneath it all was his mourning the death of his loved brother, and my mother's incapacity to mitigate his pain or help him restore his emotional invulnerability.

What I'm suggesting is that Sidney had already, by 1960, begun to see himself as opposed to and oppressed by the macroeconomics establishment[8]. He saw himself as shunned by the people in power in the economics profession, where folks like Samuelson and Solow and Heller advised the President instead of him, Sidney, who had the right answer about inflation. He believed that he had been kept out of the major committees and publications of the American Economic Association because he was not willing to be gracefully deferential to the major figures of the profession. It was this kind of personality structure which led him to identify himself, in his one autobiographical essay, as a "Jevonian seditionist" from Jevons's essay "The Noxious Influence of Authority" which assays that "authority has ever been the great opponent of truth." These kinds of characteristics which describe the mindset of a number of members of the Post Keynesian Economics community had its roots in Sidney's own world view. That he found, in his student Paul Davidson, an individual who himself took on some of his characteristics (a delight in argumentation, a robust self-confidence, etc.) albeit without the paranoia that shaped that emergent community[9]. Davidson, after a short spell as an assistant professor at Rutgers, went off to Houston to become a chief economist for Continental Oil, and after a year returned to Penn as an associate professor.

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[8] He was already outside the pale in microeconomics, as he was mathematically illiterate at the time when "mathematical economics" was becoming a redundant phrase.

[9] In that same *EEJ* interview, Davidson recalls that once Smolensky left the theory field in the mid 1960s, there was only "Sidney and me" (p. 91) doing work on aggregate supply, the foundation for Post Keynesian ideas.

In January of 1963, for his second sabbatical, Sidney, my mother, and my brother Neil went to London. It was during that period away that Sidney launched a departmental fight at Penn to get Paul Davidson promoted, and lost some friendships over the issue. (The Weintraub Papers at Duke contain a letter from Chairman Irving Kravis on this subject calling into question Sidney's mental health!). Also during that time Sidney made a reconnection with Harrod who insisted that the only remedy for Sidney's kind of professional depression was to redouble his work effort and resume writing. Harrod, whom we now know to have been chronically depressed and who had struggled with this over his entire life, knew whereof he spoke.

At any rate, after their return from England, Sidney was more cheerful than productive. Thus it was in the mid-to-late 1960s, once Davidson had returned to Rutgers as a full professor and was only an hour away from Penn, that there emerged an informal Post Keynesian thought collective. Sidney kept telling me, whom he thought of as an extension of himself, that there were those who were like us or thought like us and we had to stay connected to them. Other of Sidney's young friends who were enlisted included his student Eileen Appelbaum, and young Penn colleague Don Katzner. Doug Vickers, who was also at Penn in the Finance Department at the time, was drawn into this orbit as was Paul Davidson's Bristol University friend, Miles Fleming. This then was the landscape in that period. Davidson was at Rutgers where he hired new Ph.Ds Joe Seneca and Ed Williams from Penn, and Roger Hinderliter who had been a Hyman Minsky student at Washington University. Jan Kregel was a Paul Davidson Ph.D. student at Rutgers whom Davidson had sent off to act as Joan Robinson's research assistant in

Cambridge, where he in fact wrote his thesis.[10] I got hired at Rutgers in Fall 1968 to teach mathematical economics, and of course became close to Davidson myself as he extended many kindnesses to me. Miles Fleming, Department Head at Bristol, himself lightly regarded by the British economists at LSE, Cambridge and Oxford, had been “converted” by Paul Davidson, and in 1970 he hired Jan Kregel to join the faculty at Bristol. Leaving Rutgers after two years, I went to Duke and after a year there in 1970-71, I went to Bristol as a visiting junior lecturer for 1971-72 and overlapped with Kregel for most of that year before he began teaching in Belgium at Louvain.

It was I believe in academic year 1969-70, or in late spring of 1969, that Hyman Minsky came through Rutgers after spending time in Cambridge and there having either access to or conversational access about the emergent collected works of Keynes, in particular the volume 13 on preparation for *The General Theory*. The new information about the links between Keynes’ *Treatise on Money* and *The General Theory* opened up by volume 13 fed into Davidson’s own ideas which he was working up at that time and which were to emerge in his book *Money and the Real World* (1972). My own contribution to that exercise was to have taught Davidson about the term “ergodic”. I had written a doctoral dissertation in 1969 on the stability of stochastic general equilibrium systems, which involved analysis of stochastic differential equations. I was familiar with ergodicity and gave a name to one of the arguments Davidson was trying to make.

[10] When I was hired to teach at Rutgers through Davidson’s agency in the fall of 1968, I was immediately enlisted to serve as a committee member for Jan Kregel’s dissertation defense.

Sidney and Cambridge UK Post Keynesians

This takes the story as I understood it, and as I saw it through my close connection with Sidney and with Paul Davidson, through about 1970. As should be clear, this was not a story that had any Cambridge UK people in it[11]. Sidney didn't know what they were thinking about or talking about. In his own autobiographical essay, he ended by asking "Would I do anything differently? Yes, on leave in 1957 I regret not spending the time with Joan Robinson and Nicky Kaldor, for I was doing some parallel work (p.55)." He had a distant respect for Joan Robinson, but since he wasn't the least bit interested in capital theory, he had really very little to say. His interest was always in short term policy even though he wrote about economic growth. He always thought about growth as a succession of Keynesian short runs, and for a period of time in the 1960s was going around the world giving lectures under State Department aegis about how economic growth was now a permanent feature of the American economy.

Because the argument about the (US) Post Keynesian dependence upon the Cambridge UK ideas has so often been made, and promulgated and accepted by followers of the UK Cambridge tradition (e.g. J. L. King, in his history of the movement, and Geoff Harcourt in most all of his "historical" writings) I must make very clear that in my view this is nonsense. The ideas

[11] Despite Davidson's recollections (in the Colander *EEJ* interview) about Sidney's connection with Joan Robinson and other Cambridge people in the 1950s, while he was writing his book on income distribution theory, there was in fact no connection. Frankly, Sidney never could follow her arguments about capital theory, and though he certainly put her *Accumulation of Capital* on reading lists, he began paying attention to her ideas, and Kaldor's and Kalecki's, on relative shares, only after his volume was nearly in print (see his preface). His critical discussion of Kaldor's work appears only as an Appendix in that book. And nearly all of his Joan Robinson references are to her *Theory of Imperfect Competition*! His efforts attracted their attention, to the extent that they read anything produced outside their own circle, only after his book had appeared.

that were to define the U.S. Post Keynesians were in place, published, and promulgated by Sidney, and his student Paul Davidson, in the mid to late 1950s. Those ideas produced a theory of inflation, and a policy perspective on inflation, that ***predated*** *any contact Sidney Weintraub had with Cambridge UK individuals like Robinson and Kaldor.*

Sidney and family (my mother and brother) went to England in the 1962-1963 academic year, probably the winter term. That was the period of time I surmise that Sidney made several trips to Cambridge, and "connected with" Robinson and Kaldor on substantive matters, particularly inflation policy. It was in that period in the mid-1960s that Sidney was going full bore on inflation theory attempting to develop his material that was to surface later in his tax based incomes policy work individually and jointly with Henry Wallach.

In any event, Sidney made those connections in that trip to England, and continued those connections on a number of other trips overseas mostly under State Department auspices where he might stop off for a period of time in England on the way to somewhere else. A lot of the details of this are certainly tracked in his correspondence files. Picking up these threads, by the later part of the 1960s there was more to and fro with Cambridge, certainly with Paul Davidson having spent time there arguing with Joan Robinson. It was Davidson's correspondence with Robinson that led to Jan Kregel, Davidson's student, going over to Cambridge to work with Robinson while he wrote his dissertation. Davidson had written his own dissertation on income distribution for Sidney (1958), and was, with Smolensky, writing an intermediate version of what Sidney was teaching in macro. Davidson thus was well placed for those kinds of Cambridge linkages. Kregel was the other connector. The connection between Sidney and Joan Robinson additionally was strengthened when, in 1969, Sidney went to Canada to become chair at the University of Waterloo. There Sidney hired his old students John Hotson and Hamid Habibagahi together with rootless old friends like Ivor Pearce and Don Katzner, stray unemployables like Vivian Walsh, and loose cannons like Sidney Afriat. The connection in Waterloo with Joan Robinson was that one of Joan's daughters lived only a few dozen miles away from Waterloo and so when Robinson would come over to visit her family, she would stop off and see Sidney or he would bring her to Waterloo for a talk or a lecture or a seminar. Sidney was at Waterloo from 1969 – 1971. Of course, some of the reason for his going was the tremendous monetary gain that accrued to him, because of a quirk in the tax treaties between the U.S. and Canada: if he was out of the US for over 18 months but less than 24 months he would pay taxes to neither government. Sidney was limited in the number of visits he could make back to the U.S. because of the tax treaties. He did however return to the U.S. for a year in 1971-72 (when I was at Bristol) to prepare to go back to Waterloo once the tax matters had been settled. But a longer term tenure was not to be, as Walsh and Afriat, old Oxford friends, brought Robert Mundell to Waterloo during the year of Sidney's absence, and Mundell persuaded the dean that they needed a Canadian as chair; that new dean then refused to honor the previous dean's offers to Sidney as he named Mundell chairman instead of Sidney. Mundell quickly fired the non-tenured Don Katzner who as a result ended up at the University of Massachusetts.

The point is that there were increasing connections between at least Sidney and Paul Davidson, and possibly Minsky, and Jan Kregel, in the late 1960s with respect to Joan Robinson and Cambridge. I am not aware of any other connections Sidney had to Cambridge besides

Kaldor. In any event, there was some sense that Sidney and his group and connections were doing macro-distribution theory similar to the kinds of things that Robinson and Kaldor had been doing. The inflation discussions were similarly based, with a mutual recognition in both the US and UK of what, in the popular label, would have been called cost push explanations. It was on this topic that Sidney and Kaldor reconnected, and Kaldor persuaded Sidney to write a piece for the *Lloyd's Bank Review.* This was not an acceptable view of inflation in the late 1960s for the U.S. Keynesians, whose econometric macro forecasting models increasingly employed a bolt-on Philips curve apparatus. The battles of the U.S. Keynesians on inflation theory were in those days organized around the attacks made upon them *not* by the cost push Post Keynesians but rather by Friedman and his allies in the name of what came to be called monetarism. Thus even in the inflation debates, though Sidney believed he had been an early participant in the mainstream debate between cost push and demand pull proponents, his views were marginalized as the so called Keynesian versus monetarist wars heated up at the end of the 1960s.