The Cowles Anti-Keynesians 1943-1954*

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Today, it seems, just about anyone can get away with calling themselves a Keynesian, and they do, no matter what salmagundi of doctrinal positions they may hold dear, without fear of ridicule or reproach. Consequently, some of the most extraordinarily absurd things are now being attributed to Keynes and called "Keynesian theories". For instance, J. Bradford DeLong, a popular blogger and faculty member at Berkeley, has in a (2009) paper divided up the history of macroeconomics into what he identifies as a "Peel-Keynes-Friedman axis" and a "Marx-Hoover-Hayek" axis: clearly he has learned a trick or two from the neoliberals, who sow mass confusion by mixing together oil and water in their salad dressing versions of history. The self-appointed "New Keynesians" of the 1990s (including Gregory Mankiw, David Romer and Michael Woodford) took the name of Keynes in vain by unashamedly asserting a proposition that Keynes himself had repeatedly and expressly rejected, namely, that market-clearing models cannot explain short-run economic fluctuations, and so proceeded to advocate models with "sticky" wages and prices (Mankiw, 2006). George Akerlof and Robert Shiller (2009) have taken three sentences from the *General Theory* out of context and spun it into some banal misrepresentation concerning what Keynes really believed about the notion of "animal spirits," not to mention his actual conception of macroeconomics.[1] And we observe contemporary journalists going gaga over Keynes, with almost no underlying substantive justification from the economics profession:

More than three decades have passed since Richard Nixon, the Republican US president, declared: "We are all Keynesians now." The phrase rings truer today than at any time since, as governments seize on John Maynard Keynes's idea that fiscal stimulus - public spending and tax cuts - can help dig their economies out of recession. (Giles, 2008).

It is undeniably a Sissyphyssian task to lean against this blustering tide of misrepresentation in the current Humpty Dumpty climate, with its gales of misinformation and gusts whipping about the turncoats, where economists harbor such

[1] In a delicious turnabout, the neoliberal Richard Posner has pointed this out: "Akerlof and Shiller think that by 'animal spirits' Keynes meant noneconomic motives and irrational behaviors, and they imply that he wanted government to countervail the excesses that occur because of our animal spirits. This is a misreading. The passage in the *General Theory* is not about excesses, and it does not argue that 'animal spirits' should be dampened" (Posner, 2009) at http://www.tnr.com . At least the neoliberals do their enemies the courtesy of reading Keynes, unlike some of our modern self-styled 'Keynesians'.

easy contempt for history that words can be purported to mean anything that is convenient or politic for the selfish purposes of the writer. Indeed, in the current crisis, it may be a matter of urgency for the 'second-hand dealers in ideas' (as Hayek called them) them to hastily rewrite history, if only to cover up the extensive complicity of modern economists in neutralizing financial deregulation, occupying governmental agencies and justifying the invention of the so-called 'toxic derivatives', hence contributing mightily the contraction, not to mention their need to minimize their utter disarray when it comes to agreeing how to respond to the worsening situation. Peter Hall (1989) has already demonstrated conclusively that the government policies pursued in the Great Depression across many nations had little or nothing to do with what Keynes wrote. Brad Bateman has pointed out that almost the entire stylized history concerning Keynes and Keynesianism has be refuted by one historian or another, essentially to no avail (Backhouse & Bateman, 2006, p.272). The ultimate irony for the historian is that Keynes after 1936 kept insisting there were limits to the state's ability to manage the economy (Bateman, 1996, p.146). So what could be the value-added contribution of one further attempt at clarification of the tortured relationship of Keynes and a small subset of the Keynesian pretenders, at this late hour?

I want to begin with a proposition that can serve as a quick and dirty litmus test to separate out most of the faithful Keynesians from the plethora of preening pretenders, particularly in the current climate. This proposition, hardly original in its inspiration, is that whatever Keynes' rhetoric may have done to blur the boundaries of his renunciation of his contemporary orthodoxy[2], subsequent history demonstrates beyond a doubt that the Keynesian system is inherently incompatible with neoclassical microeconomic theory in its most rigorous manifestation, viz., the theory of Walrasian general equilibrium. This case has been made time and again in the analytical mode, by figures as various as Robert Clower, Frank Hahn, John Hicks, Alan Kirman, Franklin Fisher, Paul Davidson, Hyman

[2] The standard quote from the *General Theory* is: "But if our central controls succeed in establishing an aggregate volume of output corresponding to full employment as nearly as is practicable, the classical theory comes into its own from this point onwards" (1936, p.379). It may not be amiss even here to insist Keynes never referred to Walras as 'classical theory'. However, as Bleaney (1985, pp17 et seq) points out, he did avoid the appearance of obvious departures from (Marshallian) orthodoxy in his assumptions.

Minsky and a whole host of other illustrious figures.[3] Further, there is the oft-overlooked denunciation by Keynes himself, in a December 1934 letter to Nicholas Georgescu-Roegen: "All the same, I shall hope to convince you some day that Walras' theory and all the others along those lines are little better than nonsense!" (in Clower, 1984, p.190). So, if there has been any progress at all in macroeconomics since 1936, one might at least hope that everyone could come to agree that the economic theory of Keynes has proven conceptually antithetical to rigorous neoclassical microtheory, whichever of those traditions they wished to champion. If you fancied yourself a true believer in modern microeconomics, then you would graciously and prudently stop confusing and confounding it with Keynesianism, and vice versa. Reflex sneers about 'microfoundations' would have to get checked at the door. Probably most of the actual macromodels deployed would persist unchanged in any event. The only substantive benefit from hewing to this proposition would be that the non-economist might have some prayer of sorting through all those self-appointed saviors of the economy who clog the airwaves and Internet and what is left of the serious press.

But as I am sure my reader is aware, there has been no progress in macroeconomics in this respect. A few try to invent a third overlapping category by suggesting there is still a viable 'Marshallian' economics that is both rooted in contemporary legitimate neoclassical microeconomics and at the same time respects the actual arguments one finds in Keynes' *General Theory*. This paper does not have anything to say concerning that position.[4] Others, less nostalgic for those simpler Marshallian verities, simply assert that the only legitimate economics must start with the Walrasian model, or else something that bears a family resemblance to so-called dynamic general equilibrium, maximize this or that, and then go on to derive propositions which are loosely dubbed 'Keynesian' with no further evidence or justification. This constitutes the vast bulk of what passes for research in economics journals these days, and indeed,

[3] For modern restatements, see (Colander et al, 2008; Colander, 2006; Clower, 1984; DeVroey 2009). A nice summary of the state of play circa 1985 was (Bleaney, 1985).

[4] That does not mean I don't think the position is crucially flawed, not because it is easy to argue that Keynes retained many Marshallian methodological moves (which he did), but rather because one cannot bridge the yawning conceptual gap between Marshallian practice and Walrasian theory. The most astute advocate of this position, Axel Leijonhufvud, has been one of the most indefatigable advocates of using history to maintain a Third Way between Walras and Keynes. But his interpretation is explicitly rejected by establishment figures like Paul Samuelson (in Samuelson & Barnett, 2007, p.148).

the range of public discussion of the economic crisis seems almost entirely confined within the boundaries of this position. Sadly, it is literally gibberish. No wonder journalists are beginning to openly suggest that economists are as treacherous and slippery and dangerous as the bankers and hedge fund buccaneers who are conventionally indicted with causing the crisis (Coy, 2009).

I have no illusions that mere historical research could somehow rectify this most unsatisfactory tenacious predicament. Every attempt in four decades to slay the Gorgon has resulted in the monster growing three new heads, meanwhile turning the pathetic Keynesian challenger to pumice. Charlatans will continue to hide behind the flimsy façade they call 'Keynes' well into the foreseeable future. Nevertheless, I believe it would be a minor yet significant advance in the history of economic thought would be to make some effort to understand why the neoclassical yoke on the Keynesian donkey apparently can never be lifted. Once it becomes established that Keynesian economics will never be delivered from neoclassical corruption and misrepresentation, at least in this world, then perhaps a new set of options for the future of macroeconomics can be seriously put on the table—but that awaits a different future than the one we now face.

In the meantime, historians can demonstrate that Keynesian ideas were hobbled from birth, in the sense that two (and maybe even all three) of the postwar schools of American neoclassical economics were in a very palpable sense *hostile* to the theoretical content of the *General Theory*. The putative "Keynesian Revolution" of the 1940s in an interesting sense didn't really happen.[5] (Many similar arguments have previously been supplied for the European case: see (Young 1985; Bleaney, 1985; Hall, 1989; Louca, 2007).) In other words, I want to counter the proposition that, "The radical approach identifies the problem as Walrasian theory" as Kevin Hoover (2006, p.239) so clearly puts it. Here the role of historians is *not* to become embroiled in the old endless futile argument over 'what Keynes really really meant'; and it is certainly not to 'rescue' the shade of Keynes from his detractors. Rather, what historians should insist upon is that, if and when we get serious about what Keynes actually wrote, and how it was received in

[5] I want to be clear that contemporary self-described pro-Keynesians do not perform any better than anti-Keynesians in this regard. When Paul Krugman, the darling of the contemporary left, is quoted (in Coy, 2009) as saying, "This is really shameful, that we should be wasting precious months as an [economics] profession retracing debates that were settled 70 years ago", it is evidence that *nothing* Krugman writes about the history of economics should be taken at face value, Nobel or no Nobel.

the immediate aftermath to 1936, then at least in America, it becomes clear that almost everyone responsible for the postwar neoclassical ascendancy in its first decade actually conceded in one way or another that Walras was indeed the problem, and that, were Keynesian ideas to be pursued, macrotheory must be revised and reconfigured to conform to Walras (or something very like it). If that meant lopping off some inconvenient appendages here or negating some doctrines there, then so much the worse for *The General Theory*. *There is nothing 'radical' about this proposition*; it is simply historical fact.

1. The Keynesian Revolution that Wasn't: an overview

The primary contention of this paper is that too many postwar neoclassical economists have been uncritically and misleadingly counted as 'Keynesians' in America; indeed, a more careful and thorough census would reveal that, in practice, they were actually much more scarce on the ground. The commonplace idea that there was some sort of grassroots groundswell 'Keynesian Revolution' in the 1940s/50s should itself then eventually come in for renewed skepticism and historiographic revision. Indeed, that particular story line was first popularized by one of the major protagonists in the current paper, Lawrence Klein (1947), whose own Keynesian credentials were curiously dodgy, as we shall soon discover. Instead, the predominant motivation seems to have been to render neoclassical theory safe *from* Keynes, whatever that safety was taken to mean in the various American precincts. This claim about the American economics profession—that it was overwhelmingly inclined in an anti-Keynesian direction-- is rather broad, and would seem to require an impossibly large sample of representative agents to be done justice within a single paper. Luckily, we can make reference to some previous literature in the history of economics to whittle the problem down to a manageable size.

We shall take as our point of departure a set of arguments made elsewhere[6] that there was no neoclassical orthodoxy in America before WWII; and thus when neoclassicism did rise to dominance, it did so in the format of three very different schools of thought—here identified for convenience sake as the Chicago School, the MIT School, and the Cowles School. Everyone knows that one of the main tenets that united the

[6] See (Mirowski & Hands, 1998; Mirowski, 2002; Mirowski, 2006)

Chicago School was hostility to everything they believed that Keynes stood for. Furthermore, this was a major consideration right out of the starting gate in 1946.[7] Hence we can save time by simply presuming that there is no problem recruiting the Chicago school to support our contention that neoclassical theory was incompatible with Keynes.[8] The challenge comes instead with the other two schools.

The cases of MIT and Cowles would at first glance appear more daunting. Paul Samuelson was a standard-bearer of the self-declared vanguard of the Keynesian Revolution, and undisputed leader of the MIT school; indeed he popularized 'Keynes the economic theorist' through his successful introductory textbook, where he coined the later ubiquitous catchphrase "grand neoclassical synthesis". But historians have progressively become aware just how elusive and insubstantial the grand 'synthesis' really was around MIT. Luckily, we can point to the work of the very same Kevin Hoover, who in (Pearce & Hoover, 1995) indicates that:

> Samuelson's…analytic goal is to resolve the paradox between Keynes and the (neo)classics. This is accomplished through studious vagueness. There is no serious attempt to reconcile the macroeconomics of his first edition with microeconomics; resolution of the paradox is then mostly an avoidance of its implications. (1995, p.200)

In the second edition of the textbook Samuelson addressed the problem by simply wishing it away:

> In recent years, 90% of American economists have stopped being 'Keynesian economists" or "anti-Keynesian economists." Instead they have worked toward a synthesis of whatever is valuable in older economics and modern theories of income determination. The result might be called neo-classical economics and is accepted in its broad outlines by all but about 5% of extreme left-wing and right-wing writers (Samuelson, 1951, p.260).

This statement was a shameless whitewash when it was published, but does tend to capture the mindset of those postwar economists at MIT who struggled to reconcile their conflicting theoretical allegiances in the late 1940s. Samuelson revealed he was aware there might be some threat of incompatibility, but signaled he would proceed as if little things like logical contradictions could be easily evaded. What this meant in practice

[7] This statement is documented in (Mirowski & Plehwe, 2009; Mirowski & Hands, 1998). The article by Mirowski & van Horn in (Mirowski & Plehwe, 2009) explains why the birthdate of the Chicago school should be 1946.
[8] Of course, most of the early Chicago members were also eventually hostile to Walras—but that is a complication which we need not explore here.

was strict adherence to a lowbrow version of price theory pitched somewhere between Marshall and Walras, plus promotion of a few ‘macroeconomic’ models nowhere to be found in the *General Theory*. Of course, the infamous 45º diagram placed so prominently near the front of the textbook didn’t represent the higher reaches of macroeconomics at MIT in the late 1940s: that tended to revolve instead around the IS-LM model, the subject of intense historical scrutiny in the interim.[9] Again, in this paper we want to avoid the interminable and sterile arguments over whether or not IS-LM was ‘really’ legitimately Keynesian; rather, we simply want to endorse the literature which suggests that macroeconomic innovation at MIT happened almost entirely within the precincts of what was then deemed good neoclassical theory; whereas Keynes the author was treated at MIT more or less disparagingly as a bumbler who “did not really understand what he had written, and chose the wrong thing to publicize as his innovation” (Klein, 1947, p.83); “Keynes seems never to have had any genuine interest in pure economic theory” (Samuelson, 1946, p.196); rather like a delirious prophet who spoke in tongues. By the way, it was not snarky critics that first drew that particular religious comparison; it was the Pope of MIT himself: “True, we find a Gospel, Scriptures, a Prophet, Disciples, Apostles, Epigoni, and even a Duality; and if there is no Apostolic Succession, there is at least an Apostolic Benediction” (Samuelson, 1946, p.189). Again quoting (Pearce & Hoover, 1995, p.203), “Like the church, Samuelson’s analytical framework had to adapt to modern conditions and confront the forces of schism and reformation.” Along Mass Ave., testifying your fealty as ‘a Keynesian’ was nearly as elastic an affiliation as avowing conversion as a born-again Christian,[10] which is perhaps why it has been so easy for Samuelson to write in retrospect, “The Keynesian Revolution was the most significant event in 20th-century economic science” (Samuelson, 1988).

[9] See (Young, 1985; Darity & Young, 1995; DeVroey & Hoover, 2004). For an example of how this manifested itself, witness Lawrence Klein’s PhD thesis (1947, pp.87-8). IS-LM first made its appearance in Samuelson’s third edition of his textbook, but only as an appendix (Pearce & Hoover, 1995, p.210).

[10] “To brand or label someone as a Keynesian economist…is not very constructive, from my point of view” (Klein, 2006, p.165). “In [the *General Theory*] the Keynesian system stands out indistinctly, as if the author were hardly aware of its existence or cognizant of its properties…Flashes of insight and intuition intersperse tedious algebra. An awkward definition gives way to an unforgettable cadenza. When it is finally mastered, we find its analysis to be obvious and at the same time new” (Samuelson, 1946, p.190).

What would it mean for an historian to seriously assert that MIT was more anti-Keynesian than favorable to Keynes, on balance? It would start by scrutinizing the ways in which MIT bent over backward to insist that models having no basis in the *General Theory* (but obvious ancestry in neoclassical artifacts like utility functions and production functions) were in fact inspired by an imaginary personage they suggested should be called "Keynes". My own favorite would be MIT's most important contribution to macroeconomics in the first postwar decade, Robert Solow's version of growth theory. The theory had nothing whatsoever to do with Keynes, except possibly as a reprimand delivered to Harrod's growth model, which did exhibit a certain inspiration in Keynesian themes. But there are a whole raft of similar exercises emanating out of MIT in the postwar decades, such as the Phillips curve, the overlapping generations model, Samuelson's early models of options pricing, the Modigliani-Miller theorem as opening the floodgates to the proliferation of financial derivatives, and a host of others. The exercise would continue by showing how MIT was proudly in the vanguard of the quest to discipline and punish the actual followers of Keynes (Samuelson & Barnett, 2007, p.149) who had been maintaining personal loyalties in their Cambridge UK redoubt in the 1960s. Then it would explore how MIT just crudely suppressed all discussion of Keynes' rejection of Tinbergen's econometric models (Samuelson, 1946, p.197 fn11). It would conclude by taking seriously the admission – albeit in a Samuelsonian jocular tone -- in the very midst of the events here documented that, "I am not myself a Keynesian, although some of my best friends are" (1946, p.188). [11] This inability to face up to the consequences of their own theories has long been a hallmark of the MIT style: "I would guess that most MIT Ph.D.s since 1980 might deem themselves not to be Keynesians" (Samuelson in Samuelson & Barnett, 2007, p.149).

Yet beyond MIT, the most dramatic historiographic re-evaluation would have to come with regard to the third school of postwar orthodoxy, the Cowles Commission in its Chicago years, especially the period 1943-1954.[12] One of the most frequent mistakes in

[11] Serious historical re-examination of the actual history of the MIT school must await the promised delivery of Samuelson's personal papers to the Perkins Library archive at Duke.

[12] This reconsideration would extend to retraction of a sentence I had once penned: "Sometime between the first flush of enthusiasm in 1944 and 1947, structural econometrics at [Cowles] stopped being about price theory and switched allegiance to Keynesianism" (Mirowski & Hands, 1998, p.280). While this is a

the literature on the history of macroeconomics is the simple presumption that because Cowles was politically identified with "the Left" in that period, its members must have been pro-Keynesian. It therefore may come as a shock to realize that many of the primary protagonists at Cowles were fairly hostile to Keynes; and further, to come to appreciate that an abiding reason for their disdain was precisely because that they knew that the *General Theory* was incompatible with Walrasian neoclassical economics. Since Cowles was the Trojan Horse responsible for the introduction of Walras into the USA, it was a foregone conclusion that Keynes was viewed with skepticism by the opinion leaders at Cowles, at least during its most influential period, from the departure of Oskar Lange and before its move from Chicago to Yale in 1954. This period coincided with the research directorships of Jacob Marschak (1943-48) and Tjalling Koopmans (1948-54). It is the second contention of this paper that a clearer understanding of the nature of the suspicions concerning Keynes at Cowles, and especially by Marschak and Koopmans, will result in a better understanding of why the Keynesian donkey would not slip out from under the neoclassical yoke in the later 20th century, thus further illuminating the predicament we broached at the outset of this paper.

Why haven't historians noticed the significance of Cowles as an anti-Keynesian stronghold in the first postwar decade? Curiously, part of reason can be attributed to the efforts of some members of the MIT School in retrospectively suppressing evidence that they were pretty much alone amongst their postwar neoclassical peers in trumpeting the virtues of Keynes, as we have already suggested in the citations from Samuelson's textbook. For whatever motivation, another MIT luminary has sought to rewrite history in the Orwellian mode in order to erase certain key events in the history of Cowles. Robert Solow has had the audacity to write in a retrospective on Cowles:

> It should be evident from the story so far that macroeconomics was not the chief glory of the Cowles Commission in its Chicago days. There emerged, as I mentioned early on, no Cowles Commission tradition in macroeconomics distinct from what was going on elsewhere… The intellectually dominant senior figures at the Cowles Commission were Tjalling Koopmans and Jacob Marschak. Koopmans had no real interest in macroeconomics…Marschak had much more interest in the subject… Nevertheless, Marschak, for all his breadth of interest and sureness of taste, was not really 'into' macroeconomics either, in yesterday's cliché… those senior people who were

reasonable representation of what happened to econometrics within the American profession, the situation at Cowles was more baroque, as this paper explains.

> fundamentally involved in macroeconomics – Milton Friedman, Lloyd Mints, Henry Simons – were out of sympathy with the whole Cowles Commission enterprise of mathematical rigor and generality.[13]

It seems if what happened at Cowles didn't fit one's preconceived notions of "macroeconomics" after the fact, then maybe it just didn't exist.

2. Tinbergers, *not* Keynesians

We have already suggested that Paul Samuelson's theoretical fealty to Keynes may have been less than reliable or comprehensive; but his treatment of the history of the period has been downright louche. Hard experience has dictated that one should not depend upon his published reminiscences for validation when it comes to other people's infatuations. In the following, another bold MIT move to rewrite history, he claimed to be paraphrasing something told to him by Hans Neisser:

> My friend, fellow immigrant Jacob Marschak, was right and I was wrong. When each new innovation came along – game theory, Keynes' notions of effective demand, econometric identifications – he embraced them all with enthusiasm, even overenthusiasm. (Samuelson, 2004, p.xii)

Either Neisser was not as close to Marschak as he believed, or Samuelson is an unreliable *rapporteur*. Marschak was never an unalloyed enthusiast for game theory[14], nor was he especially enamored of Keynes. If there was a hero to whom Jacob Marschak and those he hired at Cowles pledged their troth, a contemporary economist who embodied intellectual virtue in their eyes, it was unambiguously Jan Tinbergen. And since, at least in the 1940s, Tinbergen's *bête noire* was Keynes, it would just seem unlikely that Cowles would just naturally become a bastion of Keynesian thought.

In order to understand the Cowles Commission at Chicago in the 1940s/50s as a nest of Tinbergers, it may be prudent to get straight on the timeline and cast of characters first. Cowles harbored a fair number of distinguished figures who helped shape macroeconomics in the immediate postwar period: Oskar Lange, Jacob Marschak,

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[13] (Solow, 1991, pp @@@). This bit of apocrypha then got repeated elsewhere, e.g.: "[Cowles] largely stood apart from the Keynesian Revolution, focusing more on developing estimation techniques than on solving the riddle of macroeconomic failure" (Parker, 2005, p.195).

[14] He was rather enthusiastic about the appendix to *the Theory of Games and Economic Behavior* which sketched what became known as von Neumann-Morgenstern expected utility theory; in other words, he was a decision theorist, *not* a game theorist. This is discussed in (Mirowski, 2002, pp.289-292).

Lawrence Klein, Tjalling Koopmans, Donald Patinkin, Franco Modigliani, and Karl Brunner. Oskar Lange was already a member of the Chicago economics department when Cowles moved there in September 1939, and for a short time was its most illustrious member. As early as 1938, Lange had already pioneered a trademark theme of macroeconomics at Cowles: "Thus both the Keynesian and the traditional theory of interest are but too limiting cases of what may be regarded as the general theory of interest. It is a feature of great historical interest that the essentials of this general theory are contained already in the work of Walras" (1938, p.20).

However, dissatisfaction with events on the ground at the University of Chicago prompted him to serve as visiting professor at Columbia from 1942-44, "and it was uncertain whether he would return" (Cowles Commission, 1952, p.25). Since the existing research director, Theodore Yntema, had been diverted by war work, Lange suggested that Jacob Marschak be appointed research director in 1943. Marschak had briefly taught at the New School, after having been director of the Oxford Institute of Statistics 1935-9. While at Oxford, Roy Harrod wrote to Keynes, "We have a sort of minor Tinbergen here in the form of Marschak… He himself is content with what you once called wisecracks, very good ones I think. But he happens to be a shrewd person." (Keynes, 1973, p.298). The war emptied out Cowles; but in compensation the war also deposited many refugees upon the doorstep of the Social Sciences Building at Chicago. Lange never did return, opting instead for joining the newly reinstated Polish government at the end of hostilities. In the meantime, Marschak set about putting his stamp on the research program and stabilizing the funding situation, which prior to that time had been supplied almost entirely at the pleasure of Alfred Cowles.[15] One way he did this was recruit people he perceived as fellow 'little Tinbergens' to invest economics with a bracing dose of scientific rigor.[16] Lawrence Klein reports on Marschak approaching him at the 1944 meetings of the Econometric Society: "Marschak prevailed on me to drop all other job search activities and develop what he said the country needed desperately – a new

[15] For data on the funding situation at Cowles during the war, see (Mirowski, 2002, pp.217-8).
[16] It has not gone unnoticed that Marschak believed training in the natural sciences was the best way to achieve this. Tinbergen had started out as a physicist. Marschak himself had an advanced degree in engineering, while Koopmans was a PhD physicist, Leonid Hurwicz and Kenneth Arrow had backgrounds in meteorology, and Carl Christ held a physics degree.

Tinbergen model of the US economy" (Klein, 1991, p.108).[17] Klein was employed as 'research associate at Cowles (*sans* faculty appointment) from November 1944- July 1947; and then had to leave for reasons we shall shortly explore. Marschak also hired an actual direct protégé of Tinbergen in the person of Tjalling Koopmans in July 1944. Marschak brought on board one of his own protégés from his brief sojourn at the New School, Franco Modigliani, as research associate in 1947; but Modigliani rather rapidly decamped for the University of Illinois in November 1948. Another Marschak protégé was plucked from the graduate students at Chicago as a research assistant in May 1946; Donald Patinkin was promoted to assistant professor and research associate in 1947, only to leave for Illinois in June 1948,[18] and Hebrew University in February 1949. Marschak brought Kenneth Arrow on board in April 1947, initially suggesting that he work on the Klein econometric model for his master's thesis. Finally, Karl Brunner had been a guest of the Commission from January 1950 – June 1951, supported by a Rockefeller grant.

On the face of it, with the possible exception of Karl Brunner, this roster might look at first glance like the Midwest farm team of the Keynesian Revolution in America; but appearances are often deceiving. Most of these individuals maintained an assured cleared distance from what Keynes actually wrote during their careers, as explained below. But more dramatically, Marschak's Tinberger project proved a roaring failure in sweet short order at Cowles, to such a stark degree that Koopmans brought all macroeconomic research to a screeching halt in the early 1950s, and effectively repudiated the prior empiricist orientation of Cowles in the 1940s: all econometric estimation was stopped cold. Hence, contrary to Solow, *Cowles at Chicago was not so much 'uninterested' in macroeconomics as repelled by its internally generated consequences*. By 1955, there was established but one God at Cowles, and his name was Walras; Arrow/Debreu became his newly anointed prophet. If (as the conventional wisdom has it) structural econometrics rode the back of the Keynesian Revolution to research dominance in the postwar American economics profession, then it didn't happen

[17] Note well: Marschak did not say a *Keynesian* model, even though Klein's thesis had been on Keynes.
[18] The debacle at Illinois is described in (Solberg & Tomilson, 1997). We briefly return to the case of Patinkin at the conclusion of this paper.

at Cowles.[19] There has only been one lone historian who has noted just how strange and abrupt the multiple renunciations, purges and repudiations were at Cowles in the late 1940s and early 1950s: Roy Epstein's sadly neglected *History of Econometrics* (1987). Epstein's clarity of vision derived from making use of the Cowles Commission archives, which we also access in this paper. However, even Epstein did not see fit to explore the full extent to which the ambivalent critique of Keynes was part and parcel of the volte-face.

In hindsight, the recruitment of a platoon of Tinberger clones to construct a mathematical macroeconomics conformable to the nouveau Walrasian ascendancy was a prescription for the disaster that actually occurred. As Marschak was well aware, Tinbergen had been commissioned by the League of Nations to test various business cycle theories; "but practically all of them proved to be too ambiguous to allow of such a test…. Tinbergen took over all the relevant relationships suggested by the various authors and arranged them into a logically coherent system of equations." [20] Or, to put it in a less flattering light, the 'model' of the business cycle in (Tinbergen, 1939) "eventually presented was not derived from any a priori economic theory, but rather defined his own theory" (Epstein, 1987, p.50). Tinbergen himself had never been especially partial to the Walrasian system; nor had he ever been a stickler for a dominant role for theory in empirical investigations. Rather, he displayed the rough-and-ready pragmatism of the bench physicist in those prewar days: you work with the flawed and incomplete data available, adjust the specifications of the equations according to data availability and statistical significance, and simply drop equations that don't 'work' according to loose intuitive criteria. But incongruously, Tinbergen also made sweeping claims for his chosen equations, suggesting that one might stabilize the business cycle by government changing the magnitudes of one or more of his estimated coefficients, much the way one might tune an antenna by turning a frequency dial. This could hardly constitute some sort of exemplary "mastery of the craft of model building" (Hendry & Morgan, 1995, p.52); nor was it a rigorous program of statistical theory testing according to the (then-)nouveau doctrines of Neyman and Pearson. Rather, not a few harbored suspicions that it bordered

[19] The story gets muddied when Cowles moved to Yale in 1955, with James Tobin as the new research director. That story lies beyond the scope of the current paper. See, however, (Mirowski, 2002, chap.5).
[20] Marschak to Robert Redfield, Feb 15, 1944; CFAY.

upon a glorified exercise in curve-fitting, wedded to a misbegotten ambition to control the economy by altering some inexplicably manipulable behavioral parameters. (This was one motivation for Keynes' scathing assessment of Tinbergen.) But closer to the heart of the matter, in retrospect, the very notion that a Tinbergen-style approach would eventually lend credence to Walrasian 'pure' economics as the foundation for the most rigorous option in the ongoing development of macroeconomics beggared belief.

Thus, if we can briefly imagine ourselves back into the 1940s, temporarily ignoring what has happened in the interim, perhaps we can begin to appreciate just how outlandish the ambitions of someone like Marschak may have appeared to his contemporaries. As Epstein (1987, p.64) says, "It appears Marschak did not think a satisfactory theory existed in the textbooks that could offer causal explanations of macroeconomic phenomena." Pointedly, Keynes' *General Theory* was one of those textbooks.

3. Jacob Marschak

Pace Solow, Jacob Marschak was concerned with macroeconomics from the very start of his career. Marschak's 1922 Ph.D. at Heidelberg was on the quantity theory of money, and during his tenure at the Kiel Institute (1928-30) he engaged in numerous controversies about cures for the worsening depression in Germany. In 1931 he was running a seminar on Keynes' *Treatise on Money* at Heidelberg. Ejected by the Nazis in 1933, he was inducted as a member into the Oxford 'trade cycle group' (Young, 1989). As founding director of the Oxford Institute of Statistics, he attracted funds from the Rockefeller Foundation by promising a "multi-faceted attack on the problem of the business cycle". Marschak's OIS hosted the 1936 meetings of the Econometric Society where Harrod, Meade and Hicks presented their mathematical interpretations of the *General Theory*. Although he had written on Marxian theory in Germany in the 1920s, it appears by the early 1930s he had decided that many socialist precepts could be better expressed in the idiom of neoclassical theory.[21] While this was not an especially popular position in the Great Depression, neither was it altogether isolated; his insistence contra

[21] "I knew that Marschak is a Socialist, but I have a very strong impression that in the matters of the Econometric society he is guided by uniquely scientific motives." (Ragnar Frisch to Joseph Schumpeter, 12 November 1932, quoted in Louca, 2007, p.347).

Mises that neoclassical theory revealed that calculation could take place under socialism allied him with Oskar Lange, the other famous neoclassical 'market socialist' of the era.

It is an interesting fact uncovered by Louca (2007) that Marschak was instrumental in staging the famous session on the *General Theory* at the 1936 Oxford Econometric Society meetings. He reproduces a letter from Marschak to Ragnar Frisch:

> Incidentally, I had a few days ago a somewhat similar idea – that it would be good to ask one of Keynes' adherents to explain to us in a clear (i.e., mathematical) way the substance of his new book which now creates a sensation among English economists… On pp.297-8 of his new book Keynes makes some nasty and unfounded remarks against mathematical economics. Owing to his enormous influence, that makes our task even more urgent.[22]

One observes from this letter that from his first encounter with it, Marschak regarded the *General Theory* as murky and enigmatic, and potentially hostile to his own research program. But at this stage, living as a vulnerable émigré in clubbish Oxbridge, he was not willing to go public with his doubts.

Further roiling this volatile situation, Keynes decided at that juncture that he would upbraid Jan Tinbergen's League of Nations volume *Statistical Testing of Business Cycle Theories* in the pages of the *Economic Journal.* The seeming incongruity of Keynes denouncing econometrics in its infancy, juxtaposed with the commonplace belief that Keynesian macroeconomics and econometrics mutually supported each other's rise to orthodoxy, has proven just too delicious a paradox to be passed up by historians; the result has been a massive secondary literature on the Keynes/Tinbergen imbroglio.[23] Because of this embarrassment of riches, I hope I need not summarize the content and early phases of the controversy here; nor do I need endorse or otherwise dispute the later Cowles/MIT view that Keynes was dodderingly inept when it came to technical topics in probability and statistics; instead, I merely direct our attention to how the critique had an impact upon Marschak and Cowles. Given his career up to that point, it is perhaps not surprising that Marschak interpreted Keynes' broadside as an oblique attack upon himself, at least as much as upon the hapless Tinbergen. Perhaps a little more unexpectedly, Oskar Lange equally took umbrage, and this prompted the duo to join

[22] Marschak to Ragnar Frisch, Feb. 8, 1936; quoted in (Louca, 2007, p.192).
[23] I will just point to some of the high points in this effusion of commentaries: (Keuzenkamp, 2000; Epstein, 1987; Pesaran & Smith, 1985; Louca, 2007; Garrone & Marchionatti, 2004; Dostaler, 2007).

forces to push back at Keynes. At this moment in time, Lange was at Cowles, while Marschak was still in New York.

These preliminaries set the stage for Marschak's first openly negative response to Keynes, in the format of a joint-authored attempt to defend Tinbergen from Keynes' critical review of his 1939 book. The 1940 manuscript was turned down by Keynes for the *Economic Journal*, and never appeared in Marschak's lifetime.[24] The tone, as befits the attempt to place it in the EJ, is respectful; both Marschak and Lange start off by pledging agreement with "the economic theories of Mr. Keynes"; but they are clearly distressed by Keynes' dismissal of Tinbergen's statistical methodology; and furthermore, they presume to lecture Keynes on what a 'theory' should look like, which in their view should constitute a closed "system" of equations. In part they attempt to respond to Keynes' critique that Tinbergen had no complete list of causal relations to hand by suggesting that statistical test would still be valid if one could divide the world *a priori* into two subsets of 'significant' versus minor causal factors, and then further tripped themselves up by insisting one could identify 'significance' with substantial statistical correlation with the dependent variable. Here Marschak presaged a later tendency in his career of conflating statistical induction with 'theory' *tout court*. Marschak and Lange ended up having to concede that changes in the parameters of the equations over time could wreak havoc with estimation, but as Louca (2007) reveals, the worries expressed in their correspondence with each other were avoided in the final manuscript. At bottom, Marschak and Lange were attempting to render Tinbergen's model building process seem much more disciplined and constrained and systematic than it actually was, which is why their final retort ended up being so ineffectual. Keynes' main objection remained: "with a free hand to choose coefficients and time lag, once can… always [be] cooking a formula to fit moderately well a limited range of past facts. But what does this prove?" (Keynes, 1973, pp. 286-7).

Lange personally lost interest in addressing this conundrum soon thereafter; but before he left the US, he brought Marschak to Cowles to keep hacking away at it. And

[24] The manuscript was first published in (Hendry & Morgan, 1995), along with reproduction of Keynes' original review. Keynes was not impressed with the quality of the performance, and wrote to Pigou that he had "a very poor opinion of Marschak and only a moderately good one of Lange" (letter Keynes to Pigou, March 29, 1940, quoted in O'Donnell, 1997, pp.154-5).

that's what Marschak did, hitting the ground running, seeking to leverage his prior connections with the Rockefeller Foundation in order to fund his vision of an econometric program of macroeconomic research guided by 'theory'. But Marschak encountered stiff resistance at Rockefeller, in part due to some skepticism expressed behind the scenes by referees and expert informants located at the National Bureau for Economic Research. Joseph Willits, the foundation officer in charge, then approached Dean Robert Redfield at Chicago, with the complaint, "I can't quite understand what Marschak really has in mind" (in Epstein, 1987, p.64). Willits forwarded some 'questions' about the program back to Chicago, which were probably ghosted by Rockefeller referees Wesley Clair Mitchell and Arthur Burns.[25] Redfield then turned to Marschak, inquiring what exactly it was he conceived that Cowles should be doing. Marschak's response revealed his real opinions about Keynes in a way his more diplomatic publications might not:

> There is the important question as to whether the words "economic theory" were used to designate economic theory of the sort contained in our textbooks. The answer is "no"… Yet there are notable attempts to make theory more precise. For example, the essence of Keynes' theory of unemployment has been expressed by Hicks (Econometrica, 1937), somewhat similarly by Lange (in Economica, 1938) – as a system of three equations… Although Keynes' theory thus formulated represents a great advance, it cannot be regarded but as a very rough approximation… it answers the question how a given change in the exogenous variable (money) affects income, savings, and interest rate; but it is blind to any autonomous movement of these three variables in time… In a sense, the systems of equations which Tinbergen attempted to test for U.S.A. 1919-32 … can be regarded, in spite of their different historical origin, as such an expansion and "dynamization" of Keynes' system – but also of other systems…
>
> J.M. Keynes severely criticized Tinbergen before reading his decisive second volume. Thus, Keynes was not aware of Tinbergen's (and others') method of formulating logically clear and consistent theories by means of dynamic systems of equations. Keynes' criticism was rather concerned with the permissibility of traditional statistical correlation applied to economic data… However, though Tinbergen's statistical techniques are weak, his general approach, namely, the setting up of theories in the form of equation systems, the statistical testing of such systems, and the subsequent measurement of the "destabilizing" effect of each single parameter seems highly promising. As to concrete economic theories, i.e., hypothetical systems of equations to be tested, there is a whole range between an over-simplified system like Keynes'

[25] "there seems to be a difference of opinions and sympathies between our approach here and that used by Mitchell and Kuznetz [sic] at the National Bureau… It seems that Willits has been advised by representatives of the 'other school', hence this difficulty in understanding some of our terms." Marschak to Louis Wirth, 8 February 1944, CFAY.

> (properly formulated and "dynamized") and the over-catholic and cumbersome one of Tinbergen… Beyond that, any specification of "the" theory would, at present, mean merely setting one's mind on preconceived ideas often affected by emotional preference, as in the case of the role of wage rigidity, monopolies, income distribution and public spending.[26]

It is hard to know what the anthropologist Redfield made of all this. Certainly he would not have been aware that this litany of complaint represented the pent-up resentment dating from Keynes' rejection of the Lange/Marschak intervention of 1940. Perhaps Marschak had begun to frame his American opponents as being of a similar ilk: enemies were lurking everywhere. For a research group purportedly primarily devoted to the development of neutral technocratic statistical methods of empirical inquiry, an inordinate proportion of the grievances seemed to have to do with the protagonist "Keynes" and the disputed meaning of "theory". Because this became bound up with Rockefeller skepticism over the Cowles program, in 1947 broke out into the open as the infamous "Measurement without Theory" controversy.[27] For reasons having to do with immediate funding concerns, the explicit targets of the Cowles attack were American Institutionalists like Mitchell and Burns; but few realize that behind the scenes, a concurrent parallel target was also Keynes.[28] (Keynes' death in 1946 obviated any direct attack; thus barbs were often deflected onto surrogates.)

Marschak initially managed to press forward his ambitious program on three fronts: (1) the later infamous statistical work of Haavelmo, H.B. Mann and Abraham Wald on the maximum likelihood estimation of systems of equations, later celebrated as "simultaneous equation methods" or "structural econometrics"; (2) managing to winkle

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[26] Marschak to Robert Redfield, February 15, 1944. CFAY

[27] The key texts are reprinted in (Koopmans, 1970, pp.112-161). For commentary on the controversy, see (Mirowski, 1989). It is interesting to note that Marschak had already laid out the characterizations of the opposing sides as early as 1944, even though Koopmans was to be credited with the argument in 1947: "the approach of Mitchell and Kuznetz [sic] is to manipulate on [sic] economic figures in a purely descriptive way. They fit a trend and describe the cycle of each economic activity without trying to find the causation by going down to assumptions of economic behavior. In other words, they treat the material on men as if it were physical bodies; they do not take advantage of the fact that we know something about men from our own knowledge and experience. The combination of economic theorists and statisticians in Chicago is interested in a closer connection between statistics and theory." Marschak to Louis Wirth, 8 February 1944, CFAY.

[28] The third leg of this hermeneutic macroeconomic triangle, namely, the relationship between the Institutionalists and the Keynesians, was also fraught with suspicions and misunderstandings. While we cannot discuss them here, consult (Rutherford & Desroches, 2008).

the measly sum of $7500 out of Rockefeller, using it to hire Tjalling Koopmans in July 1944 [more on this below]; and (3) from November 1944 Klein was explicitly engaged to begin building a Cowles macroeconomic model for purposes of postwar planning [also more anon]. Marschak highlighted all three in his interim report to Willits in January 1945, suggesting that all three would synergistically interact:

> The most recent example is the impasse reached in the utterly important question of measuring separately the effect of incomes on consumers' savings and the effect of incomes (and other factors) on entrepreneurs' investment… the size of national income, while affecting savings or borrowings is itself, in turn, determined by the actions of savers and borrowers. These simultaneous relationships cannot be measured by "classical" regression method… In the last six months, Tjalling Koopmans of the Cowles Commission, assisted by Herman Rubin, has given the full solution of the statistical problem for an important class of cases, while Lawrence Klein and myself are supplying the specific economic questions and hypotheses.[29]

It seems that relations between the team members were not exactly the smooth well-oiled machine that Marschak made out in his reports to funders. Referring explicitly to 1945, Lawrence Klein hinted at the frictions in retrospect:

> Great faith was placed on the ability of sophisticated statistical methods, particularly those that involved advanced mathematics, to make significant increments to the power of economic analysis. I [Klein] personally, take more faith in the data base, economic analysis (institutional as well as theoretical), political insight, and attention to the steady flow of information. Some of the other members of the team showed disappointment that the results, when finally produced, were not sharper and more precise… At the beginning of the project Marschak used to say, in public meetings, "just give us three years, and we shall deliver powerful new results for economic analysis." He always had at the back of his mind that we would be able to help decisively with postwar economic planning. (Klein, 1991, p.114)

But that didn't happen at Cowles, or at least not in the way Marschak had promised. First off, one must recall that full-fledged digital computers were not yet available at Chicago to carry out econometric exercises. Cowles did manage to get intermittent use of the University's Card programmed Electronic Sequence Analyzer, a sort of precursor to later mainframe computers (Klein in Hymans, ed. 1982, p.113), but elaborate calculations were slow, complex and awkward to carry out. Yet the Cowles

[29] Marschak to Joseph Willits, January 12, 1945; CFAY. This letter also expresses Marschak's disdain for interviews and surveys as a source of economic data, in the context of his wrapping up of the Rockefeller-funded project on Price Controls run by George Katona. Marschak summarily terminated Katona as part of his new clean sweep of the Cowles stables.

statistical theorists were proposing elaborate maximum likelihood procedures for structural estimation that were so complicated that there was little hope of implementing them on anything larger than two or three equations with existing technology.[30] Yet that was only the tip of the iceberg.

It perhaps might have been worth slogging through the elaborate calculations if the new methods had demonstrably improved the quality of the estimated parameters; but alas, they did not. In the few instances in Klein's models where the new techniques of structural estimation were tried, parameter estimates and model diagnostics looked *worse* than those produced by the simpler but supposedly flawed ordinary least squares techniques.[31] It became apparent that Klein was more interested in building a model that made sense from many different evaluative perspectives – which meant constant tinkering with specifications and trying out different combinations – than the hard-nosed 'theory testing' being advocated by Marschak (and soon Koopmans). His lack of fealty to any strict 'Keynesianism' also played a role. The tinkering ruled out both repeated fancy maximum likelihood techniques due to computational limitations, and simple Neyman-Pearson ideas of "testing." Hence it was noteworthy that, while he was searching for improved specifications, Klein kept reverting to simple OLS techniques, that is, methods which Cowles had taken to decrying in public. When Klein eventually found a specification he liked, he didn't even bother to test it for "identifiability"! Thus Klein was acting less and less like a team player from Marschak's perspective.

As if this were not bad enough, there was another source of dissention in the ranks. In his youth, Klein was as much or more inclined to be a Marxist as he was a faithful Keynesian. He joined the Communist Party while in Chicago in 1945, soon after moving to Cowles. There were other Party members at Cowles at that juncture: Kenneth May, for one. At first, there seemed to be no reason for concern: after all, most Cowles members in the later 40s were socialists of one stripe or another. In 1946, Klein wrote a paper favorably comparing Marxian to Keynesian theory; fissures began to be revealed when he proposed to have it included in the Cowles Discussion Paper series. Marschak,

[30] "An early version of the methods had been presented at the 1945 Chicago conference and it was recognized that they were generally very burdensome, frequently prohibitive" (Hildreth, 1986, p.50).
[31] See the discussions of Klein's Models I, II and III in (Christ in NBER 1951; Epstein 1987, pp.104-113; Basmann in Colander 2006). Andrew Marshall was assigned the task of re-estimating Klein's model III in 1947 and extending its purview, only to find discouraging results (Cowles Commission, 1952, p.49).

who had been a Menshevik in his radical youth, was doubly offended; Keynes may have been bad, but Marx was now situated beyond the pale. Marschak summarily refused to allow the paper to be linked to Cowles in any way. This did not auger well for Klein's future place at Cowles. And then, Klein's *Keynesian Revolution* appeared in 1947. Few people have read it today, but the first edition was an attempt to probe both the theoretical weaknesses and strengths of the *General Theory* from an unapologetic Marxian viewpoint, or more accurately, a Lange-style neoclassical Marxism. Consider some of the following excerpts:

> Capital accumulation has long been stressed by Marxist writers but never adequately incorporated into the models of bourgeois economics… Keynes' own treatment of the capital stock was exceedingly superficial. (1947, p.68)
>
> The application of the Keynesian model to the working of a socialist economy is ironic because Keynes was quite outspoken in his distaste for socialism, especially the Soviet system…Keynes, glorifier of bourgeois life, little knew that the arguments why the Russian economy has been and will continue to be one of interrupted full employment under socialism follow directly from his own simple model. (1947, pp.77-8)
>
> …some of the modern Marxists who think seriously about economic affairs have supported Keynesian economics. What is there in Keynesian economics that would appeal to a Marxist? (1947, p.130)
>
> In general, we can say that Marx analyzed the reasons why the capitalist system did not and could not function properly, while Keynes analyzed the reasons why the capitalist system did not but could function properly… With these points in mind, it would seem that the principal relation between Keynes and Marx would be in their respective conceptions of the historical time paths of the marginal efficiency of capital and the rate of profit. (1947, p.131)
>
> The Marxists do not oppose the Keynesian program… They consider it to be in the interests of the common man and therefore support it, but the only smooth-working long-run solution for them is socialism… Keynesian economics gives us a set of tools with which to work on the unemployment problem, but it does not deal at all with many other important socio-economic questions that also deserve a large share of our attention and study. (1947, p.186)

It would be hard to settle on which aspect of this would have revulsed Marschak more: his discomfort with emotional enthusiasms in economics; the elevation of his *bête noir* Keynes into an epoch-making macroeconomic theorist; or the endorsement of a Marxism he felt he had outgrown and renounced long ago. In any event, a confluence of forces soon rendered such equivocations moot. Three things dictated that Cowles attend to its political proprieties starting in 1947: first, the Red Scare started to really heat up, particularly in Illinois, with ominous threats of loyalty oaths being imposed upon the university; second, Cowles began to get access to serious military funding through its nascent connections developed at RAND (Mirowski, 2002, pp.219-221); and third, Cowles got its chance at informing high-level policy in a way that Marschak had been insisting all along was the ultimate objective of everything done at Cowles, and it had failed miserably.

None of these events has received adequate notice from historians of economics; but I believe the third debacle was crucial for dashing Marschak's previous ambitions for economics. The Cowles members had close relations with some members of the Committee for Economic Development, and one of them, Albert Hart, approached Marschak and Klein in 1945 for assistance in its quest to argue for expansive postwar economic policies. Hart suggested that the Klein model be used to project the effects of demobilization upon national income and employment after the war under different policy scenarios; the CED would then use them in its lobbying campaign (Klein, 1991, p.114). Klein generated some projections in 1946, and presented them both to various government agencies in Washington and to the CED, where they were roundly rejected. The lesson drawn from this at Cowles was that the political classes did not seem to want what Klein was purveying, and this proved the final straw. Klein (and May) were thus summarily ejected from Cowles in 1947; and macroeconomic model-building was demoted at Cowles to a remote minor region of the research agenda.[32] With the accession of Koopmans to the research directorship, all concern with Keynes was effectively banished for the rest of Cowles' tenure at Chicago.

One side effect of these developments was that Marschak's notion of the subordination of econometric modeling in the service of social engineering of political change rather shriveled after 1947, and it was this, and not some unexplained ineffable 'disinterest' in macroeconomics that accounts for the dramatic turn taken at Cowles, and indeed, in his own research agenda. From thenceforth, Marschak became even more

[32] As Klein (1991, p.115) puts it delicately, "senior researchers at the Commission were not satisfied with the performance of models that had been constructed during the expansionary phase of the research program and there was relatively little carry-on activity in empirical model building… Macro model building continued on a smaller scale after 1947, but it ceased to be the central thrust in the same way that it was during 1944-47". Klein was once again ejected for his leftist beliefs, from the University of Michigan in 1953/4 (Hymans, 1982, pp.221-2). Epstein (1987, pp.110-113) calls this period at Cowles, "The Retreat from Structure."

rigidly Walrasian, insisting that each and every single question be driven back to its 'fundamental' determinants in neoclassical tastes, technologies and endowments. The purpose of econometrics itself was transformed, from something that the economic scientist engages in to discipline empirical inquiry, to something that the *economic agent* does implicitly, in order to dynamize and operationalize the Walrasian equations in conditions of change and uncertainty. Marschak had already admitted as much in a letter to Joseph Schumpeter in November 1946:

> The great difficulty in deriving a macrodynamic system from the postulates of rational behavior consists (apart from the aggregation question) in the fact that the equations of rational behavior relate optimal (ie., profit-maximizing) values of certain measurable variables to certain variables that are the expectations of individuals. These expectations may be related to the measurable quantities by psychological equations. In the choice among these equations, one might look for help from the principal of rational behavior in the following way: one assumes the individuals to handle their, and their predecessors', past experience, in a way the rational inductive investigator, i.e., a statistician would handle it.[33]

This practice of treating the agent as a little intuitive econometrician whose cognitive makeup is then conflated with a neoclassical model of rational choice has become so ubiquitous in what is now deemed "macroeconomics", that it may have escaped notice that here Marschak was the earliest progenitor of what eventually became the main anti-Keynesian tradition in macroeconomic theory, the one that supposedly was only conceived in the 1970s as the brainchild of the "rational expectations" school. Indeed, Kevin Hoover has asserted repeatedly that most of what is currently known as the 'Lucas critique' of econometric models can be found in Marschak's (1953) paper "Economic Measurement for Policy and Prescription". [34] But this should not be read as a garden-variety exercise in the history of economics that searches for unsung "precursors." It is rather to point out that the "Keynesian Counter-revolution" began early in America— before any supposed Revolution had actually acceded to power; it just took a few decades for the profession to come to admit it.

[33] Marschak to Joseph Schumpeter, 23 November 1946; transcribed copy, CFAY.
[34] See, for instance, (Hoover, 2006; 2009). As usual, Epstein (1987, p.194) got there first, noting that the rational expectations model of the 1980s was "surprisingly similar to the general program that the Cowles researchers initially contemplated".

It may seem odd that one of the stalwart pillars of Cowles would have sought to undermine the credibility of econometric macromodels, until one comes to understand the extent to which Cowles by 1948 came to accept that their preferred Walrasian framework was incompatible with Keynesian theory. Marschak, for one, interpreted this as warrant for a new-found political quietism: "To experiment with the institution [of the economy] would require too much trial and error" (1953, p.25). After all, Cowles after 1947 was happy to describe macroeconomic fluctuations as attributable to unanticipated shocks to the system, rather than simple harmonic motion arising from a fixed economic structure (Marschak, 1953, p.12). As Marschak told the Cowles advisory committee in 1947, "we do not believe in past or future stability of structure" (in Epstein, 1987, p.70). Marschak's 1948/9 Chicago lectures on macroeconomics, published as (Marschak, 1951), were amazingly neutral with regard to the validity of Keynesian theory; but his lecture nineteen therein innovated the later practice of organizing the entire model around an aggregate supply/aggregate demand framework, and blamed unemployment on sticky wages (sound familiar?). By 1950, Marschak was admitting in print that he, at least, had given up on the whole idea of an empirical Keynesian macroeconomics:

> Like the rest of macro-economics, [Keynes' liquidity preference] equation is still in need of being related to assumptions of rational behavior. Should the liquidity preference equation and other Keynesian equations have purely empirical claims, *these would be hard to establish* {my italics}: the observed time series of relevant variables (quantity of money, interest rate, price level, consumption, national income and, possibly, its distribution) are no doubt consistent with a large number of equations systems other than the Keynesian one. (1974, p.96)

There is a bit of apocryphal history which states that Milton Friedman was the key culprit that killed Keynesianism in America, with a little help from the stagflation of the early 1970s, and the *coup de grace* delivered by Robert Lucas. In this, the neoliberals have once again managed to inflate their prowess and efficacy all out of proportion by wildly overstating their role in the intellectual ecology of the era. Keynesianism was killed slowly, and by degrees, by its purported promoters; its opponents needed only to give it a little nudge to finish it off.

4. Tjalling Koopmans

In a sense, the accession of Tjalling Koopmans to the research directorship of Cowles in 1948 merely ratified trends which had already become apparent the previous year: Cowles was pulling out of econometric empiricism, leaving behind structural estimation, and divesting itself of macroeconomics. Koopmans proved to be the ideal helmsman to negotiate these reversals, although there would have been no way of predicting his efficacy even just a few years prior to the changing of the guard. For Koopmans had been hired by Marschak first and foremost because he was a direct Dutch protégé of Tinbergen, carrying on the Tinbergen legacy in building econometric business cycle models at the behest of the League of Nations in Geneva, until he left Europe in 1940. Koopmans' training was in quantum physics, and in the late 1930s to the early 1940s Koopmans actually knew very little economics; he followed in his mentor's footsteps by selling himself as a specialist in mathematical statistics. Koopmans' career in economics had been extremely tenuous until Marschak plucked him from obscurity. In the mid-1930s, he had gone to Oslo Institute of Economics in order to sit at the feet of Ragnar Frisch, only to discover to his dismay that the titan of the Econometric Society rejected his own sampling approach to business cycle models (Louca, 2007, pp.232-6). Tinbergen then got him the job at the League of Nations, to demonstrate the efficacy of his approach. The model building at Geneva had not gone well; his two years there produced little of note. Subsequently as a foreign refugee in wartime America, he bounced from one temporary position to another: part-time lecturer in night school at NYU; statistician at Penn Mutual Life Company; and statistician at the Combined Shipping Adjustment Board. Were it not for Marschak, one can very easily imagine that this dissatisfied chap jumping from one brief job to another, never really fitting in anywhere, might have left no mark whatsoever on the history of economics.

It didn't hurt that Tinbergen would vouch for his protégé, but two things in particular recommended him to Marschak: in the midst of his exodus, he had managed (where Marschak had failed) to get a defense of Tinbergen vis-à-vis Keynes published in the *Journal of Political Economy* (Koopmans, 1941); and in another publication had revealed a predisposition to entertain a kind of non-Keynesian macroeconomics (Koopmans, 1942). In other words, *pace* Solow, he was hired (at least in part) as a particularly congenial species of macroeconomist for Marschak's Cowles. Although that intellectual identity didn't last for long, it may clarify matters to revisit just what kind of macroeconomist the tyro Koopmans once was.

Koopmans' defense of Tinbergen in the 1941 JPE was a very curious performance, even more so when we realize that he had shown previous drafts to Tinbergen for comments.[35] While it starts off suggesting it will respond point by point to Keynes' critique, in practice it is prosecuted at a very rarifed abstract level where an unspecified 'business cycle theorist' confronts a 'mathematical statistician' of uncertain provenance. In this disembodied ideal posited by Koopmans, the statistician attends to data problems while the theorist supervises 'outside' variables exercising their causal discipline over 'inside' variables. In Tinbergen's actual practice, there had been no separation; and at first, the paper seems to suggest that a better division of labor and separation of analytic roles might lead to better quality models. In the paper, Koopmans barely mentions many of Keynes' actual substantive objections, a number of which had to do with technical points of statistical inference (Garrone & Marchionatti, 2004). The two he does cite, having to do with strained assumptions of linearity of equations and constancy of coefficients, he dismisses as being subject to statistical test. Koopmans never said very much about the specific methods deployed by the 'theorist', probably because he rapidly would have found himself out of his depth there. After endorsing the newly proposed Neyman-Pearson approach to hypothesis testing (concerning which Tinbergen had been necessarily unaware at the time), the paper then takes a vertiginous turn, essentially rejecting the practical possibility of the division of labor he had previously broached:

> [N]o single clear-cut answer can be given to our initial question: to what extent the results of econometric business-cycle research depend on the data and to what extent on additional information and hypotheses. The relative importance of data and additional information varies from one case to another. Their combination is a complicated process, the result of a continuous dialogue, of a game of give and take, between economist and statistician (1941, p.178).

Perhaps Koopmans was correct that the quarantine between the concerns of the theorist and the statistician was impractical in the real world; but that did little to help

[35] See, for instance, Koopmans to Jan Tinbergen, 18 December 1940; JTEH.

motivate his original thesis. Somehow, this observation was supposed to justify the scientific legitimacy of Tinbergen's own iteration between estimation, model re-specification, and subsequent estimation as against Keynes' skepticism; although by most accounts this was precisely the sort of behavior Neyman-Pearson methods was supposed to restrain. Koopmans had defended his mentor by essentially ignoring the gist of the statistical objections which Keynes had broached in 1939. When Koopmans sent a copy to Keynes, the latter politely and patiently reiterated one of his primary objections, which remains as true today as it was in May 1941:

> There is one point, to which in practice I attach great importance, you do not allude to. In many of these statistical researches, in order to get enough observations they have to be scattered over a lengthy period of time; and for a lengthy period of time it very seldom remains true that the environment is sufficiently stable. That is the dilemma of many of these enquiries, which they do not seem to me to face. Either they are dependent on too few observations, or they cannot rely on the stability of the environment.[36]

This objection undoubtedly helped set the stage for Koopmans' later embrace of the Gospel of Structure at Cowles.

Koopmans' 1942 maiden foray into theoretical macroeconomics was, if possible, even more incongruous. The only economics text cited therein is Keynes' *How to Pay for the War*, which he praises as "an admirably clear and concise statement of the essential characteristics of inflationary war finance" (1970, p.50), but then proceeds to write down some algebraic expressions that have nary a whit to do with Keynesian macroeconomics, but are rather based on some tendentious manipulations of simple definitions, starting with the identity that the percentages of output going to labor, 'entrepreneurs' and government must sum to one. He then posits that time lags in expenditures can unhinge the identity, which is then maintained by a change in the price level. A simple fraction is driven to bedlam by the superimposition of further time lags and something he calls 'the marginal propensity to consume', but the consumption function is nowhere to be seen; indeed, there are no behavioral equations whatsoever. It seems plausible Koopmans had little idea of what a Keynesian model looked like, even though he had been occupied modeling business cycles in Geneva for two years prior to then. Further, there is no evidence that Koopmans was aware of neoclassical economics at this stage, and thus, a

[36] Keynes to Tjalling Koopmans, 29 May 1941; quoted in (Garrone & Marchionatti, 2004).

glaring absence of price theory suffused a paper nominally concerned with inflation. Amazingly, the entire discussion was carried on absent any mention of money or monetary theory whatsoever, probably a symptom of the holes in Koopmans' own economic education. There was no data to speak of, with Koopmans arbitrarily plugging parameter values plucked from the air into his overgrown fraction (for there were no equations in the conventional sense). Furthermore, for most of the paper it is simply assumed that production cannot be expanded in the face of government deficits, so it follows near tautologically that government expenditures cause inflation. It is difficult to see how the editors of the *Review of Economics and Statistics* were persuaded to deem the paper warranting publication.

Nonetheless, Koopmans privately harbored a high opinion of his two sallies contra Keynes. We know this from letters he sent back to Tinbergen during the war. In a pre-Cowles note sent in August 1941, he states:

> I believe I haven't told you that I had quite some response to my article on the logic of econometric business cycle research…. This response came in particular from the main person towards whom the article was critical [ie., J.M. Keynes-P.M.]. I received a very nice and appreciative letter, and on top of that, I had the honor to sit at the right hand of this person during an informal lunch, apparently to accede to the wish of the visitor and to the surprise of the big shots at Princeton. The matter became more comical because, at the same time, I was looking to them for a job. By the way, nothing materialized…[37]

As we have already pointed out, Keynes had used his charm to gently upbraid Koopmans about the 1941 critique; but the exiled and partially unemployed Koopmans misinterpreted this as encouragement. He also took the opportunity of the encounter at Princeton to press the second paper upon Keynes, which was met with even less in the way of encouragement. It is interesting to observe the skill with which Keynes dismembers the 1942 paper in his response:

> The main criticism I should be inclined to make is that the changes in the propensity to consume arising as the result of the inflationary process need more detailed consideration than you have given them. The only reference to the propensity to consume, which I have found, is on page 12, and that is incidental…
> There are four other basic assumptions to your argument, if I understand it rightly, which perhaps deserve to be made explicit and emphasized:- (1) You assume that

[37] Koopmans to Jan Tinbergen, 22 August 1941; JTEH. I wish to thank Albert Jolink for this translation from the Dutch original.

> prices are the element of freedom in the system. Historically this has predominantly been the case. But it is rapidly falling out of fashion… (2) You assume that there are no stocks of consumable commodities. If there are, this clearly decelerates the process. (3) You assume the sellers always exact the highest possible price and are not significantly influenced by any reasons (there are a variety of them) which might dictate a contrary policy. (4) You do not allow for a time lag in price raising which leads to shop shortages.[38]

This turned out to be the last contact between Keynes and Koopmans. Keynes did not devote much time to Koopmans; after all, he had much bigger fish to fry. But it is hard to miss that Koopmans' combative stance towards Keynes probably rendered him an epsilon more attractive to Marschak. In any event, Marschak used his Rockefeller subvention to pluck Koopmans from obscurity; and Koopmans discovered his métier in the process. His reports back to Tinbergen describe his delight:

> I got an opportunity I couldn't refuse. I have never been so happy in my work as here in Chicago. We have a small group under the supervision of Marschak which is working on a system of equations for the US 1920-40. But we are not in a hurry, although we are working hard. I am mainly busy reworking the statistical adjustment methods making it possible to estimate all equations simultaneously through an iterative method… Out work is based on a division of labor, where the different parts are highly complementary. Marschak is the general manager, and participates actively in all matters except pure mathematics. In particular, he works on the theoretical economics side. Lawrence Klein drafts the equations, tries different assumptions and because of the tempo by which he moves through the different subsets of the system of equations, he gives the others no opportunity to lose themselves in theoretical or methodological details. Leonid Hurwicz is our critic, a role which fits his sharp and nimble spirit.[39]

Yet soon thereafter, Koopmans rapidly soured on the Klein model, in the course of the events described in the previous section. By 1947 he realized all his high-tech armamentarium of simultaneous equations estimation procedures and identification tests were not getting Cowles much of anywhere. Yet, in the interim, Koopmans had picked up a fair knowledge and enthusiasm for Walrasian economics, as well as a shared determination to ground his models in their fundamental triad of tastes, technologies and endowments. Furthermore, in 1947 Milton Friedman had taken to attending the Cowles seminars and ridiculing the Klein model mercilessly to their faces. Friedman seemed to

[38] J.M. Keynes to Koopmans, 2 June 1941; TKSY Box 17, folder 311.
[39] Koopmans to Jan Tinbergen, 18 July 1945; JTEH, Jolink translation from the Dutch.

reserve special venom for Koopmans.[40] In this maelstrom, rather than blame the econometric techniques, it seems clear that Koopmans came round to the opinion that it was Keynesian macroeconomics itself which had frustrated the hopes at Cowles. By 1947, he basically concurred with Marschak that the time had come to "move gradually into the field of long run economics, while bringing to completion our attempts in the field of business cycle theory" [41]

Koopmans' accession to research directorship at Cowles in 1948 was the occasion to sweep out the old order and the old fascination with macro models. Klein was summarily ejected; although Koopmans did not make this clear to the Rockefeller Foundation for a few years.[42] He did hire Carl Christ to clean up the Klein model, re-estimate it for the period 1921-47, and "do it right" from the Cowles perspective; but unfortunately, the consumption function and other key equations did not pass muster with regard to serial correlation and characteristic root tests, and worse, predictions made for 1948 were no better than those made by crude trend extrapolation (Christ in NBER, 1951, p.87). The conference on business cycles held at NBER in November 1949 was the donnybrook for the remnants of the Cowles program in macroeconomics. Milton Friedman was scathing in his commentary at the conference where Christ's results were presented: "the construction of a model for the economy as a whole is bound to be almost a complete groping in the dark. The probability that such a process will yield a meaningful result seems to me almost negligible" (Friedman in NBER, 1951, p.113). Klein at the same conference washed his hands "for anything that Christ has done. I participated to a negligible extent in this work" (Klein in NBER, 1951, p.115). No one wanted to claim ownership of the failed research.

The notes on the post mortem on the conference back at Cowles revealed the depths of dissatisfaction with the entire program of building macroeconometric models:

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[40] "Koopmans was just foolish… I thought that Koopmans' was a very sophomoric attack and had no effective content—he didn't tell you where you went from here. And of course you realize that I had been involved in very long arguments with the Cowles Commission people when they were in Chicago…. So I was very unsympathetic to Koopmans from the beginning—before he wrote that [1947] article." (Milton Friedman in Hammond, 1992, p.231).

[41] Marschak to Evsey Domar, 11 April 1947; quoted in (Epstein, 1987, p.110).

[42] See the interview of LCD with Koopmans and Marschak dated 21 March 1951 concerning the Cowles research agenda; CRAN.

Koopmans: Noted that conference showed differences in expectation regarding possible results of our work…
Leontief: Many data, don't need hyper-refined methods…
Metzler: "theory" in Klein's work not sufficiently systematic…
Marschak: Clarification: behavior rather than mechanical relationships…
Hildreth: Traditional theoretical model not always the most useful to interpret data with (e.g., production function)…
Marschak: Education of profession in use of simple models
Koopmans: Not fight too much
Christ: Need to show some results
Hildreth: How anxious should we be to convince everyone?[43]

It is one thing to have to 'convince everyone' when your funding and institutional situation was precarious, as it was when Marschak took over at Cowles in 1943. It was quite another thing if instead you had just discovered a new patron with deep pockets, one avid to buy whatever you were selling, and indeed, one who had just become a major supporter of the University where you happened to be located. From the time when the atomic pile first went critical under Stagg field, the University of Chicago became a major outpost of military research. The Cold War, which in 1947 had seemed to be threatening the equanimity of Cowles, unexpectedly turned into the best thing that could have happened to Koopmans, Marschak, Hurwicz, and the rest blessed under the new research agenda.

Although initially the situation seemed dire, Koopmans already had an ace up his sleeve. From 1948 onwards, he cultivated close connections, first at RAND, and later elsewhere in the military, to fund an entirely different sort of research. The report to Rockefeller in early 1951, after briefly mentioning the work on the Klein model, launches into a litany of new departures: ""Under a contract for RAND corporation, for example, they are working on a theory of resource allocation for the Department of Defense… They are also working on the formulation of theory for the measurement of technological changes resulting from specified innovations. The study of the impact of atomic energy done under an earlier RF grant was one aspect of this work; another is a study for RAND on how to determine the ramifications in the defense program of technological changes. They are negotiating with the Office of Naval Research for a grant to carry forward their

[43] "Discussion to Size-up Results of Business Cycle Conference, December 1949" CFAY

work on the general theory of organization" [44] This reorientation led to such landmarks in the history of Walrasian theory as the Arrow-Debreu model, Koopmans' "activity analysis", linear programming, the CES production function, the Nash equilibrium in game theory, and a host of lesser innovations.

The New Order under Koopmans turned out to be such a success that thenceforth, macroeconomics was essentially banished from Cowles at Chicago. Perhaps this is why Solow could even think that Cowles had no macroeconomic footprint at Chicago. What tends to get overlooked is that, at least for Koopmans, it was the banishment of *Keynesian* economics that was the logical outcome of his directorship. It was simultaneously a capitulation to the growing influence of Milton Friedman within the Chicago economics department. Rather than contemplate that the failure of the efforts toward macro model-building in which he participated in the period from his hiring to 1948 was due to the choices made at Cowles, such as, say, the dead end of structural identification and estimation, the Tinberger ideal of snap-together models for policy, or his insistence on reliance upon Walras or nothing else, he would instead lay the blame at the feet of the literary economist Keynes and his ramshackle notion of aggregate economic theory. Koopmans felt sufficiently confident in his gravitas as an economist (even while in residence at a very different Cowles now situated in New Haven) to commit his opinion to print in 1959:

> In general the state of macroeconomic theory is unsatisfactory. There are too many reasonable alternatives which presently available observations of aggregate economic time series cannot easily discriminate. A greater stock of relevant observations could be collected and brought to bear if the basic assumptions of dynamic economics were made about the behavior of individual firms and consumers, and the implications traced through to the aggregates. (in Koopmans, 1970, pp.375-6).

5. Departing in Mid-stream, by way of a conclusion

The story did not end there, although here is where we must conclude. For instance, a serious history of the disdain held towards Keynes at Cowles would have to take into account a raft of other important figures, each of whose tenure at Cowles was too brief to justify fully-fledged individual narratives here, such as Franco Modigliani,

[44] Excerpt from interview of LCD with Koopmans and Marschak dated 21 March 1951 concerning the Cowles research agenda; CRAN. The story of the new research at Cowles is related in (Mirowski, 2002).

Donald Patinkin, Evsey Domar, Andrew Marshall, and a whole host of others. It would also have to devote some close attention to someone rarely understood as a skeptic concerning Keynesian economics, the iconic figure Kenneth Arrow.[45] And then there is the even stranger coda to our story: Cowles was uprooted lock, stock and barrel and transported to New Haven in 1954/5, where it was put under the leadership of someone whom many consider the archetypical neoclassical Keynesian of the postwar era, James Tobin. Why would our anti-Keynesians so readily acquiesce in devolution of their bustling thriving unit to someone whose entire intellectual identity seemed so wrapped up with Keynes?

These are questions I leave to other historians, perhaps more concerned with the fine points of Keynes' legacy and the way it became stunted in America.[46] Rather, I want to briefly consider the writing of the one protagonist of our Cowles drama who did avail himself of a retrospective on the events we recount here. Donald Patinkin later in life notoriously struggled with the impact of Keynes on economics, on his own career and that of others. In his (1990), he confronts the vexed historiographic problem of why Keynesian exegesis has persisted in being so multifarious and contentious. Not unexpectedly, he starts off his answer with the line which had become established at Cowles and MIT by the mid-1940s: Keynes knew not what he had done; he was a prophet babbling in tongues; the *General Theory* "never pulled together various analytical components into an explicit and complete model." So, says Patinkin, it was a hodgepodge where everyone could see in it what they wanted. Nevertheless, he insists that prior to 1960, there were "no significant differences among the various interpretation of the *General Theory*." Only afterwards did comity and decorum break down, with Joan Robinson starting to disparage opponents as "bastard Keynesians" around 1962 or thereabouts (Parker, 2005, p.699). Before then, everyone who mattered was united in concord, as if part of a single invisible college, in attempting to divine the true meaning

[45] See Arrow (1983, p.201), where in his Nobel lecture he treats "the great Keynes" as a critic of Walrasian general equilibrium. He also leaves no doubt whom he deems the weaker theorist: "The fundamental question remains: how does an overall total quantity, say demand, as in the Keynesian model, get transformed into a set of signals and incentives for individual sellers?"

[46] One example of the sort of work I have in mind is (Rubin, 2008), who demonstrates admirably that early versions of Patinkin's PhD thesis attempted a disequilibrium interpretation of Keynes based upon institutional bargaining power, but under pressure from Marschak's and Koopmans' harsh critique, was pushed in the direction of the infamous 'Pigou effect'.

and import of Keynesian macroeconomics. In other words, in his version of events, no one in Patinkin's inner circle was ever openly hostile to Keynesian macroeconomics.

I find this retrospective utterly implausible, historically misleading, and a symptom of the intellectual debility that we conjured at the start of this paper.[47] It is based upon what one might call the Cowles Creed: everyone who is said to count as a real economist necessarily believes what our crowd believes, viz., that Walrasian general equilibrium theory is the only game in town. If we then think someone has said something economically interesting, then they must submit to our recasting of it into our own favored mathematical idiom, and further, anything that doesn't fit must be jettisoned. Any objections to our procedures on the part of the originator are due to some lapse in moral fibre or congenital weakness. Since our Walrasian idiom is intrinsically neutral, we would never entertain the possibility that some of our orthodox peers petulantly dislike the doctrine under scrutiny, regard it as nonsense on stilts, and went out of their way to undermine and destroy it. There is only one Science, and it is Our Science. There is but one God, its name is Walras, and Arrow/Debreu is his prophet. In this Passion Play, Keynes was just a minor Simeon Stylites. Journalists who proclaim "we are all Keynesians now" are barking up the wrong pillar.

The evidence presented herein reveals the Creed has a few serious lacunae.

Archive Abbreviations

CFAY	Cowles Foundation Archives, Yale University
CRAN	Cowles Papers, Rockefeller Archives, Sleepy Hollow New York
JMUC	Jacob Marschak Papers, Young Library, Univ. California—LA
JTEH	Jan Tinbergen Papers, Erasmus University, Holland
TKSY	Koopmans Papers, Sterling Library, Yale University

[47] Not to mention covering up the history of his own personal road to Keynes. See (Rubin, 2008).

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