Milton Friedman and Keynes
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Let me begin by expressing pleasure and modest surprise at being here, as well as thanks to Mark Skousen for inviting me. I did know Milton Friedman. We met once at my parents' house in Cambridge, and again by accident on the slopes at Alta in 1983, skiing together for three days in the company of William F. Buckley, jr. In 1990 I was his sparring partner, along with a very taciturn David Brooks, for the reissue of the first episode of *Free to Choose*. You can look it up on Youtube. He was a charming man, affable if a bit pedantic, very difficult to dislike. That said, as I said long ago on a similar occasion, I'm here to bury Milton, not to praise him.

Was Milton Friedman a Keynesian? It's a shocking question and the most damaging one I could come up with. But I think the honest answer is that of course he was, and not merely on the evidence of his Nixon-era remark, "We are all Keynesians now," which he archly qualified with "and no one is any longer a Keynesian." The deeper fact is that Friedman developed in the penumbra of Keynes. At least three of his major ideas – anyway, at least two of the three – were off-shoots of Keynes and Keynesian theory. The two clear cases are the permanent income hypothesis of 1957 and the natural rate of unemployment of 1968. The third case, monetarism, is more arguable; I'll come to that in due course.

The permanent income hypothesis, which was cited when Friedman won the 1976 Riksbank Prize, is a gloss on the consumption function, which appeared as Chapter 8 in Keynes's *General Theory.* Keynes had offered the marginal propensity to consume as a simple constant, with the inverse of the marginal propensity to save as the multiplier. Friedman split income into permanent (or long-term expected) and transient components, deflating the multiplier analysis that Keynes had adapted from Richard Kahn.

Friedman's hypothesis was fanciful, or anyway premature, when he offered it in 1957; it presupposed a middle-class society with stable habits and long-term horizons. This had not been the predominant mentality of the Depression or even that of the early 1950s; James Duesenberry's relative income hypothesis, with its ratchet-like Veblen effects, was arguably more realistic for most US households at that time. The situation today is very different as the surge in savings rates that came with pandemic relief showed. Indeed, Friedman's latter-day disciples could have trotted out the permanent income hypothesis to defend Biden from the charge that his big initiatives were "stimulus" and therefore inflationary. Curiously – or perhaps not – so far as I'm aware that didn't happen.

As permanent income is a gloss on the consumption function, so Friedman's "natural rate of employment" -- also advanced by Ned Phelps, and later characterized as the Non-Accelerating Inflation Rate of Unemployment (NAIRU, Mark may remember who came up with that term, was it Tobin?) -- is a gloss on Paul Samuelson and Robert Solow's 1960 version of the Phillips Curve, a barnacle on the hull of Keynes's economics. The Phillips Curve was based on primitive econometrics and no theory; Tobin once wrote that it resembled Pirandello characters in search of a plot. Giving the plot a famous twist – this was a standard trick of his – Friedman appended an adaptive expectations equation to the Samuelson-Solow artifact, and then showed that in the long run, when expectations and outcomes coincide, the resulting curve is nearly vertical, so that you can't reduce unemployment (much) without runaway inflation. Friedman thus stood the "Keynesian" case for full employment on its head.

The whole argument is complete nonsense and always has been. It would turn our advanced industrial and mostly private economy into a delicate and neurotic flower, prone to slip into hyper-inflation at the slightest turn toward the New Deal, the Great Society, or any other program for the public good. Such a situation, were it the case, would drive any sane person to price controls, to rationing and to nationalization of the commanding heights, at the very least.

But the fault isn't Friedman's. It lay with Samuelson, Solow and their many followers, including all the first-generation linear modeling shops (Data Resources, Chase Econometrics, Wharton) and their n-th generation successors still at work at the Federal Reserve and Congressional Budget Office, who built the Phillips Curve into their models and never got rid of it, though Keynes, Nicholas Kaldor, Robert Eisner and I spun in our graves.

In 1997 I published an essay entitled "Time to Ditch the NAIRU" in the *Journal of Economic Perspectives,* based partly on theory, partly on a review of the historical evidence, and partly on the dispositive fact that Alan Greenspan had demolished the myth or macroeconomic impossibility theorem – full employment without inflation *was* possible! Who knew? But even in 2018, by my count twenty-one years later, the best Olivier Blanchard could manage in the same journal was an article titled with a pusillanimous question, "Should We Reject the NAIRU Hypothesis?" I'd managed to raise two daughters to voting age in that interval, before one of our most senior and respected macroeconomists got around to asking whether maybe it might be time to do a little rethinking. And there is no evidence that Blanchard's gentle enjoinder was any more influential than the grenade I'd tried to throw a generation before.

In fact, there never was a natural rate, or a NAIRU, because there never was a Phillips Curve. It was an epiphenomenon of the 1960s in the United States and non-existent in national data for other countries. Again, I don't blame Friedman; my point is only that he was dissecting the corpse of a pseudo-Keynesian construct. The point is underscored by the fact that Friedman was uncomfortable with the further step taken by Robert Lucas – to "rational" expectations and fully-vertical non-trade-off, which removed the pseudo-Keynesian underpinning and fully resurrected pre-Keynesian “classical” thought, in which employment and output are determined in a “labor market” and “money is a veil.”

I said that monetarism is a more complicated case. That may be because Friedman’s monetary views strike me as a muddle of conflicting short- and long-periods and those famous “long and variable lags.” Thus, he could say almost anything. I ran across him speaking to camera at these meetings in San Francisco once and heard him predict a big recession and a terrible inflation both in consequence of the same monetary policy, at slightly different time intervals. And when the reporter asked whether the average American family would be better off or worse off a year hence, he replied, and this is a quote, “There is no such thing as the average American family. Some American families will be better off; some American families will be worse off.”

That said, while Friedmanites were generally monetarists, it is not the case that all Keynesians were simple-minded advocates of fiscal policy. The Keynes I studied at Cambridge was a monetary theorist; he believed in the power of central banking and wrote (in *The General Theory*)

> “ Unemployment develops, that is to say, because people want the moon; — men cannot be employed when the object of desire (*i.e.* money) is something which cannot be produced and the demand for which cannot be readily choked off. There is no remedy but to persuade the public that green cheese is practically the same thing and to have a green cheese factory (*i.e.* a central bank) under public control.”

The tendency of postwar American Keynesians to downplay the Federal Reserve – in his 1968 lecture Friedman cites Alvin Hansen and Arthur Smithies as examples -- was a consequence of its minor role in the war, in this respect Friedman – who after all blamed the Fed for the Depression! -- was more in line with Keynes, not less so. He did believe that monetary policy could drive the real economy up as well as down in the short run. In this respect, as noted above, he was also more Keynesian than Lucas.

For the long run, however (and whatever that means) Friedman believed in money neutrality and in a money growth rule consistent with price stability. This can be expressed as $MV = PT$ with M the policy tool, V a technological constant and T determined by equilibria in various Walrasian markets. This of course reflects the pre-Keynesian monetary ferment, involving Irving Fisher, the marginalists, and an overlay of business cycle analysis going back to Wesley Clair Mitchell and the young Arthur Burns. Publicly created helicopter money became a controlling metaphor, pushing endogenous bank money into the background via money-multiplier mechanism, which are essentially the Keynes-Kahn method transposed onto the central bank.

Theoretically and ideologically it was all a terrible mess, taken up and expressed in policy by Friedman and especially by his students as a demand for money growth rules – two to six percent annually for M1 is the one I remember. All of which bubbled over into debates over the definition of money, leading Kenneth Boulding to rhyme:

We must have a good definition of money
For if we do not, then what have we got
But a Quantity Theory of No One Knows What?

The money growth rules did serve one very valuable purpose – not necessarily one of which Friedman would have approved. In the mid-1970s on the staff of the House Banking Committee I allied with my monetarist friends and Friedman students, Bob Weintraub and Bob Auerbach, to use them as a wedge in setting up the regular process for Federal Reserve accountability on monetary policy, which became the Humphrey-Hawkins hearings under the Full Employment and Balanced Growth Act of 1978. You couldn't ask the Fed, back then, about interest rates – too sensitive, they'd say, perhaps citing Friedman also on his 1968 claim that the Federal Reserve can't control the interest rate. But you could ask about their money growth targets precisely because the answer carried less information.

In the same era, in early 1981 when I had become Executive Director of the Joint Economic Committee, the Fed's congressional liaison Don Winn stopped by to ask a favor: could they discontinue sending semi-annual (or possibly quarterly) letters demanded by Senator Proxmire on whether M1 was growing at between two and six percent? I said sure, the letters stopped; I did not mention it to Proxmire, and no one ever noticed. Friedman himself, and to his credit, late in life decided he had pushed those rules too hard.

In these matters, I've argued, some of Friedman's biggest ideas were framed by Keynes and Keynes's economics, and in that sense, he belongs to the Keynesian era as a dissenting Keynesian. Yet he also held on to pre-Keynesian dogmas of perfect competition, long-term equilibrium and money neutrality in the long run -- all pre-modern constructs that Jing Chen and I document and debunk in our new book *Entropy Economics,* published last year to the discomfort of Milton's ghost by the University of Chicago Press.

Among other things, we also note that Friedman's principle of "profit maximization" as the sole (or even leading) objective of business in a free-market economy is neither necessary nor sufficient. You can maximize and still make losses, in which case you will disappear. You can also *not* maximize and still earn profits, in which case you will survive. The idea that you should maximize, at each moment in time, is a sophistry for spoiled children and economists.

Keynes, like Schumpeter, Kaldor and my father – but unlike Samuelson, Solow and the MIT or "American Keynesians" -- had slipped away from those eighteenth-century doctrines, repurposed at the end of the 19th century to keep Karl Marx and Henry George and Thorstein Veblen down. Keynes, who managed to break through and effect, for a time, an actual revolution in economic thought, was all about short-term pragmatism and cumulative causation. I have likewise argued that the most successful countries in today's world, including China, Korea, and Japan (at least before the 1990s crisis) followed Keynes, Schumpeter and Galbraith -- while Friedman and his acolytes triumphed here. Friedman, like Samuelson, like Solow, would have been a better Keynesian, and a better economist, if he'd gone further in that direction.

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