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Zen and the Art of Modern Macroeconomics:  
The Search for Perfect Nothingness

Comments on Alan Stockman

Monetary Policy in an Era of Change  
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Alan Stockman's paper addresses the question what international economists have to say of use for monetary policy-makers. To do so, it first evaluates two competing theories: the "sluggish-price" models on the one hand, and on the other the "equilibrium" models -- short for "dynamic stochastic general equilibrium models based on individual optimization."

This is a reasonable-sounding paper. It admits to some "gaps in the evidence" in favor of the equilibrium models; it does not insist that the transitory component to the real exchange rate are zero, only that it is much smaller than the permanent component; the author is even willing to put some probability on the possibility that he is wrong and that the sluggish-price model is right. Furthermore, the main conclusion is one that I agree with (for reasons that I will return to at the end of my comments): "A reasonable

monetary policy will place little weight on exchange rates and the current account, and most weight on domestic inflation or output stabilization and growth" (p.33).

All this reasonableness and modesty is endearing. But it is also a bit misleading. Stockman does not bang on the table and say "I know the right answer." He does the opposite. His answer to the question "What does the theory have to say about what policy-makers should do?" is, in essence, "nothing." The misleading part is when the answer, "I have nothing to recommend to policy-makers" becomes "I recommend that policy-makers do nothing." The word "nothing" will play a key role in my comments. ["We know nothing, therefore we should do nothing."] The word does not often appear explicitly in the writings of equilibrium theorists. The popular phrase in the econometric writings is "random walk." [The usual conclusion is stated as "I have found that such-and-such a variable follows a random walk." Or, at best, "I cannot reject the hypothesis that this variable follows a random walk." You seldom hear someone say, "After studying this variable for 6 months, I have absolutely nothing to say that would help to predict its movements." But the statements mean the same thing.] In Stockman's paper, the phrase is "in the current state of knowledge:" "In the current state of knowledge...exchange rates and the current account should play little role...[in the conduct of monetary policy]" (p.1).

I should make clear that my remarks do not apply just to the present paper in particular, but equally to Stockman's other papers, other "equilibrium" writings, and indeed to much of modern macroeconomics. I will refer to the disease as the "zen" of modern macro-econometrics: the search for perfect nothingness.<sup>1</sup>

Let me explain.

It used to be that the goal in econometric work was to get results that were statistically significant, to reject the null hypothesis. In order for an author to stand up in front of a conference proudly, or to expect to publish his paper in a journal, he or she sought to get significant results.<sup>2</sup> This is difficult to do in

macroeconomics. The world is a complicated place; it is unlikely that the few key variables that emerge from the particular theory that one has developed will actually go far toward explaining a real-world time series. So what we have done -- quite cleverly -- is to redefine the rules. Now the goal is to fail to reject the null hypothesis, to get results that are statistically insignificant -- in essence, to find nothing. It is far easier to find nothing than to find something. Typically one fails to reject many hypotheses every day, even in the shower or on the way to work.

Examples where the goal is to find nothing abound, from tests of Euler conditions to Ricardian equivalence.<sup>3</sup> But I will pick an example that is central to Stockman's evaluation of the two competing theories of exchange rates: the question whether the real exchange rate follows a random walk.<sup>4</sup>

Not long ago, it was argued that (1) purchasing power parity held pretty well, even in the short run, -- i.e., that there was a near-infinite speed of adjustment of the real exchange rate toward a long-run equilibrium constant (or slow-moving trend) -- and that (2) this was evidence in favor of the equilibrium view of goods markets. Subsequently, clear statistical rejections established the fact that PPP does not hold in the short run, and the question became whether it holds in the long run. Many of the equilibrium theorists now claim that (1) the speed of adjustment of the real exchange rate toward PPP is zero, or close to it, and that (2) this is evidence in favor of the equilibrium view of goods markets. It is true that it is difficult to reject the hypothesis that the real exchange rate follows a random walk, or comes close to it. A typical estimate is that the speed of adjustment of the real exchange rate to long-run equilibrium is 3 per cent a month (an autoregressive coefficient of .97), or 30 per cent a year (an autoregressive coefficient of .70) and that this speed is not significantly greater than zero. But (even waiving the change in position) one might wonder why anyone would consider the finding of a zero speed of adjustment as evidence in favor of the equilibrium

view.

The logic goes as follows. When one has finished running through some mathematics of dynamically-optimizing equilibrium, one comes out with nothing to say about movements in the real exchange rate. It can move up as easily as down. [The problem is that the equilibrium theorists have not identified the "fundamental disturbances." (This is the "gap in the evidence" to which the paper refers.) As Stockman (1987) says, the theory is still in its infancy.<sup>5</sup>] In other words, as far as the theorist knows, the real exchange rate follows a random walk.

The sticky-price model of Dornbusch, on the other hand, does have something to say of use in predicting movements in the real exchange rate. It says that when the real exchange rate has overshot its long-run equilibrium [in response, for example, to a shift in the monetary/fiscal policy mix], as the dollar clearly had by 1984, the best expectation is for it to return gradually back toward that equilibrium. So a failure to reject the hypothesis that the real exchange rate follows a random walk is (understandably) interpreted as evidence against the sticky-price theory. But it is also interpreted as evidence in favor of the equilibrium theory, even though the latter has no more testable implications for the real exchange rate than does the proposition that 9 is a prime number.

Stockman has offered a second kind of evidence. In addition to arguing that some of the things that the sticky-price model predict seem not to be true (the adjustment of the real exchange rate toward a long-run equilibrium), he argues that some of the things that the model does successfully predict can also be explained by varieties of the equilibrium theory.<sup>6</sup> Two examples stand out.

One piece of evidence that is traditionally considered to support the sticky-price model is the observation that fluctuations in the real exchange rate are very highly correlated with fluctuations in the

nominal exchange rate. But Stockman has an explanation how such behavior can also come out of an equilibrium model. To begin with, there are always real shocks to productivity, technology, tastes trade policy and taxes that would move the real exchange rate no matter what the regime. Why do these movements in the real exchange rate happen to show up almost entirely as movements in the nominal exchange rate instead of the price level? Because the monetary authority tries to stabilize the price level.

The second piece of evidence that is traditionally considered the exclusive preserve of the sticky-price model is the observation that the fluctuations in the real exchange rate are much greater for countries and time periods when there is a floating rate regime than when there is a fixed rate regime. Surely this clinches the case? No, Stockman has recently shown how to coax out of the equilibrium theory an explanation for this fact too.<sup>7</sup> This story begins with the proposition that under fixed exchange rates, governments are likely to put on (and take off) trade controls more often, in order to protect their foreign exchange reserves. Then Stockman turns loose his dynamically-optimizing agents, who manage to adjust to this behavior on the part of the government in such a way as to smooth out fluctuations in the real exchange rate. Voila! An equilibrium model that is "consistent" with the facts. Such explanations are clever, and make for good journal articles that are popular among academic economists. But that doesn't make them true.

Speaking of "agents," spy novels are a good analogy for stories that are clever and make entertaining reading, but have little to do with the truth. Datum: a few minutes ago, I got up from my chair next to Alan Stockman on the stage, and walked over to take my place here at the podium. Hypothesis 1: I am a spy for a foreign power, Alan is a CIA counterspy who was about to assassinate me, and so I got up to move out of range. This hypothesis is "consistent with the facts" in the sense that, if true, it would explain them; but it is convoluted and not very plausible. Hypothesis 2: John Le Carre was in British intelligence before he

began his second career as a novelist. This hypothesis is interesting to speculate about. I have no idea whether it is true or not. It is also "consistent with the facts" in the weak sense that it does not contradict the datum. But it seems no more relevant than the statement that 9 is a prime number, the proposition that agents dynamically optimize, or the hundreds of other hypotheses that I "fail to reject" every morning in the shower. Hypothesis 3: I came up to the podium for the simple reason that AEI invited me here to comment on Alan's paper. While not as clever as the other propositions, this hypothesis is simple, plausible, and consistent with the facts in the strong sense that it would explain them while most other hypotheses would not. (I will leave it to you to decide which hypothesis is the correct one.)

I have addressed two kinds of empirical claims in favor of the equilibrium theory, that it is "consistent" with the statistical failure to reject a random walk on the real exchange rate [an empty statement], and that it can be made consistent with the observed variability of the real exchange rate in a floating rate regime [too convoluted]. What about the claim that if the alternative sticky-price model were correct, one should be able statistically to reject the random walk hypothesis, and the disturbing fact that most studies have failed to do so? My answer is that one should not expect to be able to reject a random walk on the mere 15 years of post-1973 data that almost all of the tests use. Imagine that the truth is that the speed of adjustment to PPP is .03 per month, or .30 per year. A simple calculation reveals that one should then not expect to be able to reject statistically the hypothesis that the coefficient is zero unless one has at least 49 years of data.<sup>8</sup>

A long time series for the real exchange rate is available for the dollar/pound sterling rate. Tests of the speed of adjustment toward PPP give the following results. On post-1973 data, the speed is not statistically greater than zero. On post-1945 data, it still does not quite appear statistically greater than zero.<sup>9</sup> But, as I

have noted, this is precisely what one would expect from 43 years of data if the true speed were .30 per year or less. On the complete data set of 1869-1987, the speed of adjustment is clearly statistically greater than zero.<sup>10</sup> In my view this not only tends to vindicate the sticky-price view, but also provides a neat illustration of the irrelevance of tests that "find nothing" or "fail to reject the null hypothesis," merely because they have not looked in the right places.

Where does all this leave monetary policy? I think that Stockman's list of six proposed reasons why monetary policy might want to pay attention to the exchange rate and the current account is a very good list.

As should be clear by now, I reject the notion that the state of our ignorance (great as it is) is in itself a reason for policy-makers to pay no attention to these two important economic variables. I do agree with the conclusion that the monetary authorities should focus primarily on the price level and real output. But this is because I see many advantages to nominal GNP targeting for monetary policy.

I do not subscribe to the view that the \$150 billion U.S. current account deficits of recent years should not be a subject of concern. It is important for economists to keep explaining to the general public, as Stockman does, that not all deficits are bad deficits. A country may choose to go into current account deficit and borrow from abroad to finance a high level of investment arising from productive opportunities, or to finance a high level of consumption arising from the knowledge that income will be higher in the future. Korea's current account deficits of the 1970s are a good example. However the U.S. deficits of the 1980s are not of this type. Their origin lies in our \$150 billion federal budget deficits, which in turn originated in faulty economic reasoning and political gridlock, not in intertemporal optimization. I would add something to Stockman's list of proposed reasons to pay attention to the international variables: the current account deficit implies that we are going further and further into debt to the rest of the world. The only reason why I

agree that monetary policy should not focus primarily on the current account deficits is that the job belongs to fiscal policy.

The point is that saying that one can construct models in which a current account deficit is "nothing" to worry about because it is the outcome of dynamic optimization, is not the same thing as saying that the U.S. deficit of the 1980s is in fact nothing to worry about. At some stage, an economist has to come down from his tower of fantasy, and decide what he really thinks. The alternative is to leave the decision-making to those who may be still more ignorant.

#### References

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1. This seems a more polite label than the alternative possibility, the "know-nothing" school.

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2. This system was not without drawbacks. Many econometricians adopted the shady practice of trying out many different functional forms, combinations of variables, and sample periods in their regressions, until they found results that appeared as statistically significant.
  3. If one is a believer in Ricardian equivalence, one looks for an effect of the budget deficit on interest rates or exchange rates, one typically fails to find large and significant effects, and one concludes that therefore Ricardian equivalence holds. If one does not believe in Ricardian equivalence, one looks for an effect of the budget deficit on private saving, one typically fails to find large and significant effects, and one concludes that therefore equivalence does not hold.
  4. My comments here pertain, for example, to Stockman (1987). In that paper, like the present one, the author should be awarded a medal for bravery in taking on the question what the equilibrium models have to say of use for policy.
  5. Thus most equilibrium theorists do not wish even to hazard a guess as to what profound shifts in consumers' tastes for American products or in workers' productivity occurred in 1980-85 (to double the dollar's value against the yen and mark) or in 1985-88 (to halve it again).
  6. So far as I know, he doesn't offer any evidence of the third kind: things that can be explained by the equilibrium theory but not by the sticky-price theory.
  7. Stockman (1988).
  8. If the observations are monthly, then 47 years of data will do. This calculation and the test results for U.K.-U.S. data from 1869 to 1987 appear in Frankel (1989), as well as in some earlier papers of mine.
  9. Using the correct Dickey-Fuller test.
  10. As this sample period mixes different exchange rate regimes, it would be desirable to allow for heteroscedasticity and for different speeds of adjustment during different sub-periods.