

It's Baaack, Twenty Years Later

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This paper is an exercise in self-indulgence and self-aggrandizement.

The background: In the late 1990s some U.S.-based economists began to grow disturbed about the economic troubles facing Japan. The source of our unease wasn't, to be honest, mainly concern for Japan per se, or even about the impact of Japanese troubles on the global economy. What bothered us, instead, was the sense that our intellectual framework was falling down on the job; that Japan's stagnation and slide into deflation called into question the monetary policy orthodoxy of the day, which basically asserted that central bankers could always head off deflation and get unemployment down to the NAIRU.

This unease also had a practical dimension. Japan, for all its cultural distinctiveness, is in economic terms much like other nations, our own in particular: a big, advanced economy with its own currency, effective administration, a competent civil service, and economic policymakers who, if rarely ideal, aren't usually idiots. If Japan could get stuck in an economic trap for years on end, the same thing could happen to the rest of us.

In early 1998 I set out to reassure myself by writing down a little model to show that if Japan was having troubles, it was simply because the Bank of Japan wasn't trying hard enough. But as sometimes happens when you try to model your intuitions explicitly (and is one of the main reasons for doing formal analysis), the model ended up telling me something quite different – namely, that when short-term interest rates are near zero it is not, in fact, easy for the central bank to reflate the economy. In fact, even very large increases in the monetary base will have

essentially no effect unless the private sector is convinced that there has been a permanent shift in the central bank's objectives, a new willingness to accept and even promote inflation. As I put it, the central bank needed to "credibly promise to be irresponsible."

I wrote up that analysis in a short online theoretical paper in March 1998, then enlarged it later that year in a Brookings paper (Krugman 1998) that took on issues of the performance of the financial sector, the role of fiscal policy, and the relevance of historical experience.

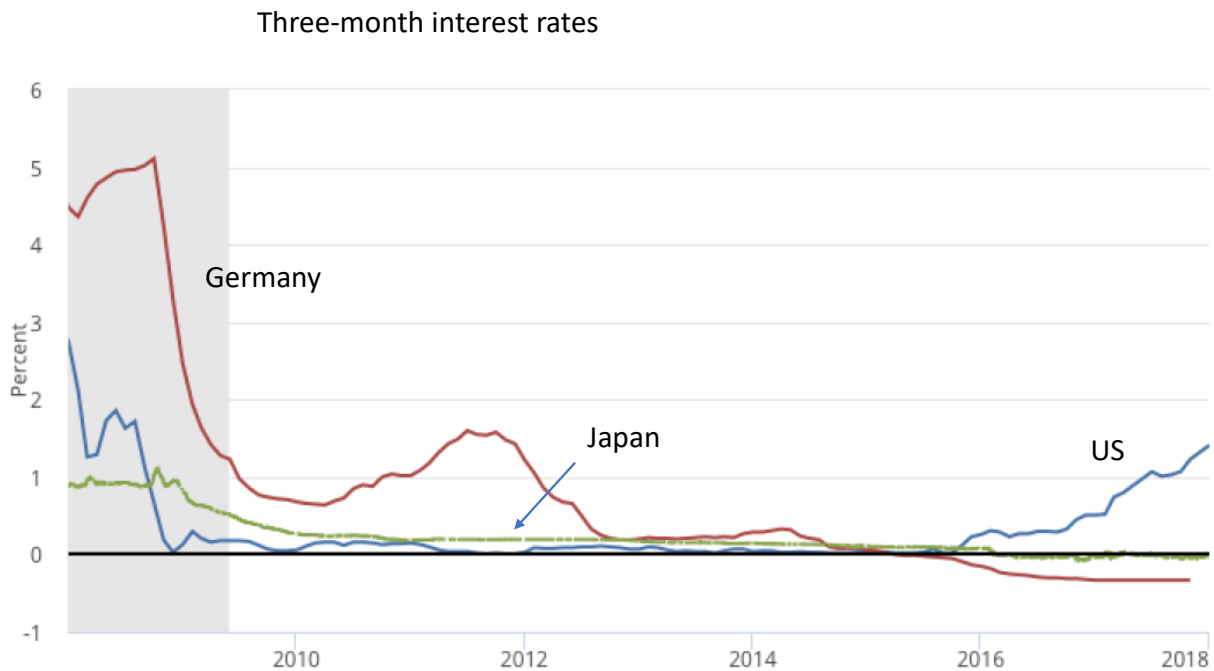


Figure 1

Ten years later my fears came true: as Figure 1 shows, with the coming of the global financial crisis the whole advanced world basically turned Japanese, experiencing a protracted era of

near-zero interest rates. The United States has emerged from that era, barely; Europe and Japan itself have not.

What I want to ask in this paper is how good the analytical approach of 1998 looks in the light of subsequent experience. Were its basic predictions correct? Where did it fall down? What new issues have arisen? And how does its policy prescription look after all these years?

1. Modeling the liquidity trap

The concept of a liquidity trap goes all the way back to Hicks (1937). Interest in the issue faded during the long postwar boom and the inflationary, high-interest rate era that followed. Still, some economists, notably Summers (1991), expressed concern that too low an inflation target might lead to frequent episodes in which monetary policy was constrained by the zero lower bound. In fact, the now-conventional 2 percent inflation target was adopted in part because simulation analyses like Reifschneider and Williams (2000) suggested (wrongly) that 2 percent was high enough to make ZLB episodes rare and brief.

So what was new in my 1998 analysis? Three main things, I think.

First, I framed the argument in terms of a New Keynesian model rather than some version of IS-LM: intertemporal maximization by representative agents and rational expectations, with short-term price stickiness the only deviation from full equilibrium. I did these things not because I

believe them to be an accurate description of reality – I’m very much an IS-LM fanboy – but to give hostages, to ensure that the results weren’t being driven by the ad-hockery of Hicksian macro.

Second, I focused on the effects of changes in the monetary base, rather than following what had by that time become the conventional approach of modeling monetary policy in terms of an interest rate target (e.g. as set by a Taylor rule.) Again, this was not a matter of realism, since monetary policy at the time was in fact usually formulated in terms of interest rate targets. Instead, it was an attempt to deal – in advance, it turned out – with an argument Bernanke (2000) and others made, which is that expanding the money supply must, more or less as a matter of definition, raise the equilibrium price level: “money issuance must ultimately raise the price level, even if nominal interest rates are bounded at zero.”

This was the view I myself held before setting out to model the issue. It turned out, however, not to be true.

Third, in the Brookings paper I spent considerable time on the implications of changes in the monetary base on financial intermediaries and hence on broader definitions of the money supply. This reflected contemporary policy discussions: it was common, indeed almost conventional wisdom, to attribute Japan’s troubles to problems in its banking sector, as evidenced by the failure of broader aggregates to expand despite supposedly loose monetary policy. But I ended up concluding that failure of broad aggregates to rise with the monetary

base – a collapse of the money multiplier – is exactly what one should expect in a liquidity trap, even if the banks are in perfectly fine shape.

I won't rehash the theoretical model here, just summarize the key strategic simplifications. It was, as I said, a New Keynesian-style model, but an extremely stripped-down one. There was no explicit modeling of production, and hence no discussion of the labor market. And while the model was infinite-horizon, I assumed that nothing would change after the second period, so that in effect it became a two-period model, with the first period being "now" and the second "forever after."

The result of these simplifications was an extremely minimalist model, with an immediate, striking implication. If, for whatever reason, the natural rate of interest in the first period was negative – that is, it would require a negative nominal rate to achieve full employment – the proposition that money issuance must raise the price level was false. Or if you like, it was missing a word: *permanent* money issuance would raise the price level. But a monetary expansion the private sector expected to be temporary, to be wound down after the crisis had passed, would do nothing at all: the extra monetary base would just sit there.

Furthermore, it was reasonable for the private sector to assume that even large increases in the monetary base in a liquidity-trap economy would be temporary. We saw this in practice when Japan adopted a policy of quantitative easing in the 2000s. As Figure 2 shows, this policy was quickly reversed once the economy appeared to be recovering.

Japan: Monetary base



Figure 2

More recently, the Federal Reserve, while it hasn't moved as abruptly as the BOJ did, is similarly moving to shrink its balance sheet now that the economy has emerged, at least for now, from the liquidity trap. Realistically, then, monetary expansion under conditions of near-zero interest rates should be viewed as temporary – and more important, *will* be viewed as temporary, meaning that the intuitive proposition that huge increases in the monetary base must be inflationary is wrong.

Of course, a model is only a model, and this model didn't even pretend to be realistic. What matters are predictions and prescriptions: what the model says should happen, and what it suggests about appropriate policy responses. So how has it worked out?

2. Liquidity-trap analysis in the global crisis

Although my 1998 paper focused on monetary policy, the framework had clear implications for fiscal policy as well. In fact, I would summarize its predictions as involving two propositions about monetary policy and two about fiscal policy. In a liquidity trap:

- Expansion of the monetary base, even if very large, would have little if any effect on nominal GDP or the price level
- Base expansion would also have little effect on broad monetary aggregates like M2, not because of some failure in the transmission mechanism, but because intermediaries would have no incentive to lend out excess reserves
- Because money demand would be perfectly elastic, and there would be an incipient excess supply of savings, there would be no crowding out: even large budget deficits would not raise interest rates
- Because there would be no conventional crowding out, fiscal multipliers would be larger than in normal times. In the formal model, the spending multiplier would be exactly one; but relaxing the assumptions to allow for credit-constrained consumers would suggest a multiplier of more than 1

In retrospect, these propositions may seem obvious. But they weren't, either in 1998 or in the midst of the post-crisis slump: many prominent figures denied one or all of them (and some still do).

Start with the proposition that monetary expansion would be neither inflationary nor effective.

One of the discussants of that 1998 paper was Ken Rogoff, who declared that

“No one should seriously believe that the BOJ would face any significant technical problems in inflating if it puts its mind to the matter, liquidity trap or no. For example, one can feel quite confident that if the BOJ were to issue a 25 percent increase in the current supply and use it to buy back 4 percent of government nominal debt, inflationary expectations would rise.”

And in 2009, Allan Meltzer declared that

“[N]o country facing enormous budget deficits, rapid growth in the money supply and the prospect of a sustained currency devaluation as we have ever experienced deflation. These factors are harbingers of inflation.”

Finally, in 2010 a who's who of conservative economists and pundits sent an open letter to Ben Bernanke warning of dire consequences from the Fed's policy of quantitative easing:

“We believe the Federal Reserve’s large-scale asset purchase plan (so-called “quantitative easing”) should be reconsidered and discontinued. We do not believe such a plan is necessary or advisable under current circumstances. The planned asset purchases risk currency debasement and inflation, and we do not think they will achieve the Fed’s objective of promoting employment.”

QE and its (non?) consequences

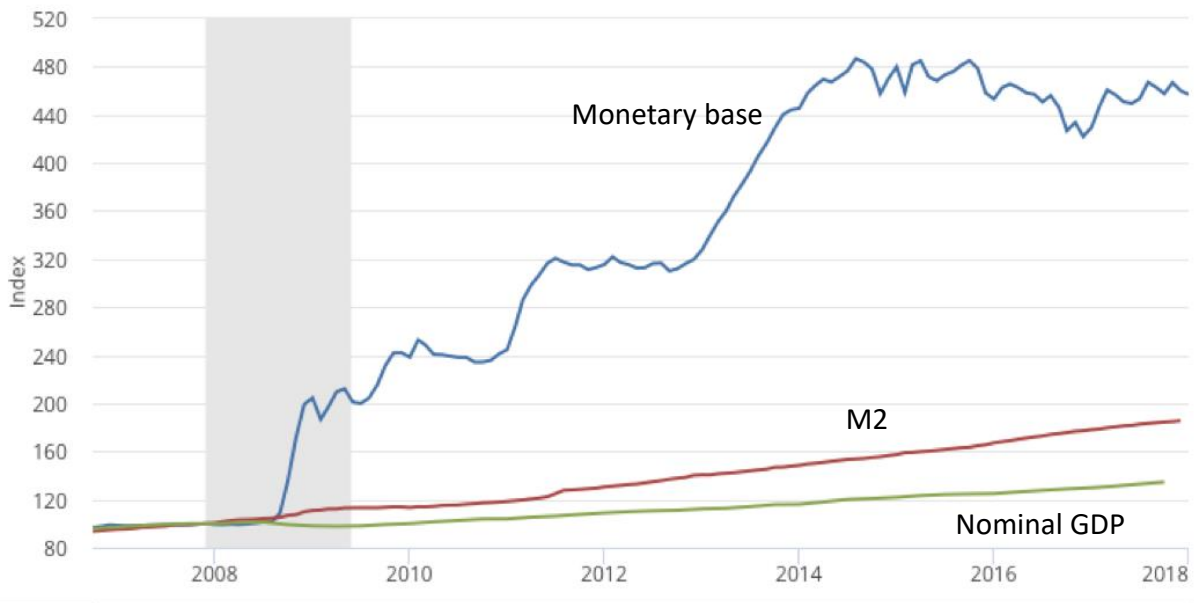


Figure 3

What we actually saw, as shown in Figure 3, was an enormous rise in the monetary base, far larger than anyone initially contemplated. Yet nominal GDP growth and inflation remained low.

The figure also shows strikingly little growth in M2; what growth there was probably reflected, at least in part, a shift of funds from shadow banking, not counted in the aggregate, to insured intermediaries.

All this was exactly as predicted, although some economists apparently didn't get the memo. For example, in 2013 Martin Feldstein described the failure of monetary expansion to produce inflation as "puzzling," and insisted that the decoupling was due to the Fed's decision to pay interest on excess reserves, which caused monetary base to simply accumulate in the banking system. As it happens, the Bank of Japan did not pay such interest during the quantitative easing episode shown in Figure 2, and saw a similar lack of results.

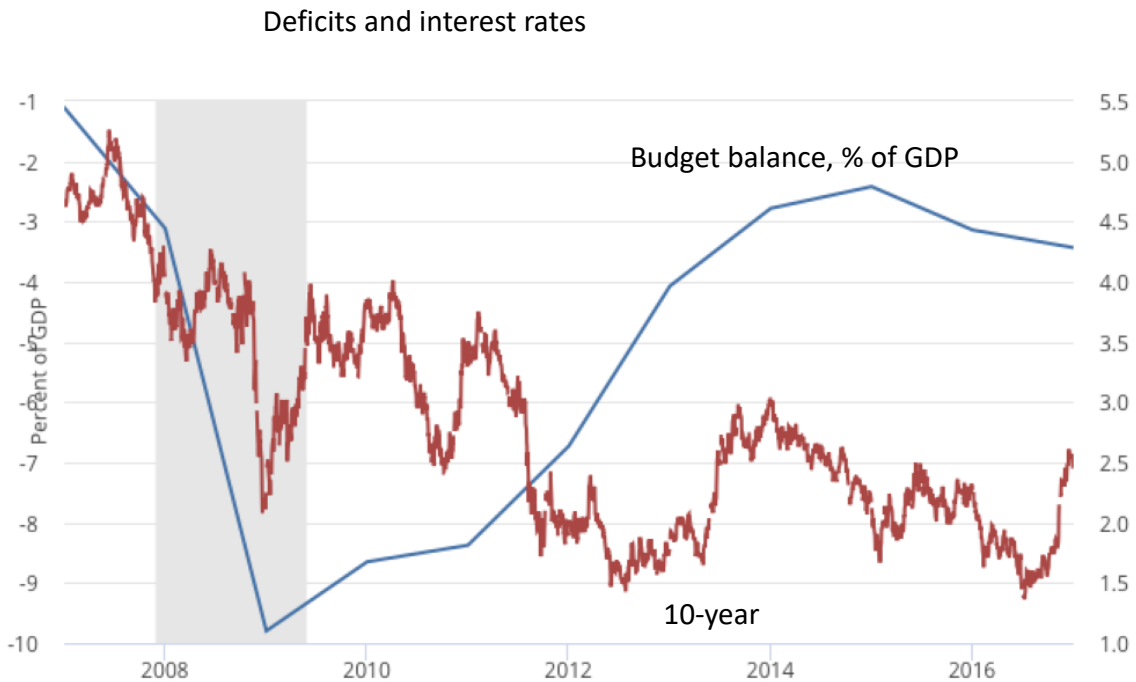


Figure 4

What about deficits and interest rates? As Figure 4 shows, huge U.S. deficits in 2009-12 were actually associated with interest rates that were low by historical standards, as liquidity-trap analyses predicted. This was very much not what many influential figures, including some economists, expected to happen. The Wall Street Journal seized on an uptick in long-term rates in early 2009 to publish an editorial titled “The bond vigilantes: The disciplinarians of U.S. policy makers return.” In 2010 Alan Greenspan, writing in the Financial Times, came out with one of my favorite economic pronouncements of all time (emphasis mine):

“Despite the surge in federal debt to the public during the past 18 months—to \$8.6 trillion from \$5.5 trillion—inflation and long-term interest rates, the typical symptoms of fiscal excess, have remained remarkably subdued. *This is regrettable*, because it is fostering a sense of complacency that can have dire consequences.”

Some historians of science tell us that the conventional view of how theories get accepted – that they are tested against evidence, and accepted if they pass – isn’t quite right. What matters is that a theory make *surprising* predictions, ones that run counter to conventional wisdom, and is proved right.

That seems to me to be a pretty good description of how liquidity-trap economics fared in the aftermath of the financial crisis. The theory made predictions about inflation, monetary aggregates, and interest rates that were very much at odds with what many people believed; those predictions were proved correct.

So far, however, I haven't gotten to the fourth prediction, large fiscal multipliers. That's because there's more controversy about what the evidence says. Clearly we didn't have runaway inflation or soaring interest rates. But evaluating the effect of fiscal policy takes a bit more parsing of the data. Skeptics of the liquidity-trap case, I'd argue, have misunderstood how to frame the issue. But let me take a moment first to point out that here, too, many influential voices, both among academic economists and among policymakers, vehemently disagreed with the proposition that fiscal multipliers would be large, or even positive.

Why? There were actually three distinct anti-Keynesian arguments, in each case made by people one would not normally have regarded as cranks.

First, one set of economists essentially resurrected Say's Law: they insisted that since income must be spent, any increase in public spending must, by definition, crowd out an equal amount of private spending. (See the discussion in Krugman 2009.)

Second, another set of economists managed to convince themselves, wrongly, that Ricardian equivalence implies a zero multiplier on government spending. (See Krugman 2011.) Actually, my original paper laid out a model with full Ricardian equivalence, in which the multiplier on short-term increases in government spending was 1.

Most consequentially for actual policy, many policymakers were persuaded by the arguments of Alesina and Ardagna (2010) that spending cuts would actually be expansionary, because they would improve confidence. Here's Jean-Claude Trichet in 2010:

“As regards the economy, the idea that austerity measures could trigger stagnation is incorrect ... In fact, in these circumstances, everything that helps to increase the confidence of households, firms and investors in the sustainability of public finances is good for the consolidation of growth and job creation. I firmly believe that in the current circumstances confidence-inspiring policies will foster and not hamper economic recovery, because confidence is the key factor today.”

Now, empirical analysis of fiscal policy is notoriously difficult, because of the endogeneity of both revenues and, to a lesser extent, spending. The raw correlation between budget deficits and output is negative, but everyone understands that this is because recessions cause revenues to shrink and spending on safety-net programs to grow.

Yet simply attempting to purge the cyclical component of the deficit, as Alesina and Ardagna did, is problematic. For one thing, statistical techniques for doing so don't seem to work very well: it was immediately obvious to many readers of the A-A work that their episodes of fiscal expansion and contraction didn't seem to correspond to known policy changes; an IMF analysis that used narrative evidence on actual policy reversed their main result. Even identifying the policy right, however, doesn't deal with the problem that stimulus programs are likely to be

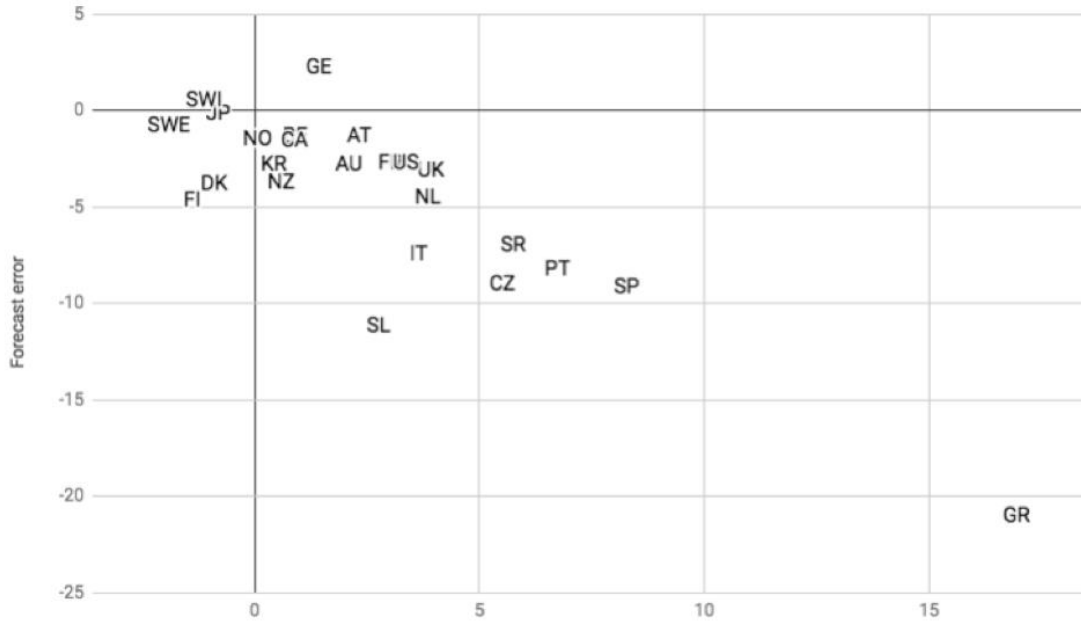
implemented when the economy is weak (as in the case of the Obama stimulus) and austerity programs often implemented, or at least delayed until, the economy is strong.

There are a variety of ways one might try to circumvent these problems, but as it happens the crisis itself provided something approximating a natural experiment: the debt panic that followed the 2009 Greek crisis. Following that crisis, some countries either chose or were forced into severe fiscal austerity despite high unemployment and the inability to pursue offsetting monetary expansion. Others were under less pressure.

This wasn't a perfect natural experiment, because arguably some of the countries forced into the harshest austerity were troubled in other ways. Still, as Blanchard and Leigh (2013) pointed out, one can at least partially deal with this issue by comparing economic performance with forecasts generated before austerity went into effect.

It's important to note, by the way, that this natural experiment took place over a limited period – 2009 to 2012 or 2013. Why? Because that's how long the debt panic lasted. Once Mario Draghi issued his famous pronouncement that the ECB would do "whatever it takes," bond spreads rapidly dropped and the pressure on governments to engage in extreme austerity policies faded. As a result, all the usual measurement and endogeneity issues apply to data from subsequent years.

Forecast error vs. Austerity



Change in structural balance 2009-13

Figure 5

Figure 5 therefore shows a Blanchard-Leigh type comparison for the period from 2009-2013. On the horizontal axis is the degree of austerity, as measured by the IMF's estimate of the change in the cyclically adjusted budget balance as a percentage of potential GDP. On the vertical axis is the deviation of growth from the IMF's forecast in the April 2010 World Economic Outlook.

As in Blanchard-Leigh, this comparison suggests multipliers substantially larger than those estimated from pre-liquidity-trap data, and indeed probably more than one. So the fourth prediction of the original liquidity-trap analysis is, I would argue, also supported by the evidence.

Overall, this is a pretty good record! An analysis made a decade before the global financial crisis, based on theory with a bit of historical evidence from the 1930s, ended up providing a good guide to monetary and fiscal policy post-2008 – a much better guide than the views held by many influential policymakers.

3. Can monetary policy be effective in a liquidity trap?

In 1998 I argued that while a monetary expansion perceived as temporary would be ineffective in a liquidity trap, an expansion perceived as permanent would work – because it would raise the expected rate of inflation, and hence reduce real interest rates. So I declared that the Bank of Japan needed to “credibly promise to be irresponsible,” making clear its willingness to tolerate higher inflation.

A more diplomatic way to put this prescription, and the way many economists now do put it, is to say that when nominal rates are near zero, gaining monetary traction requires convincing markets that there has been a monetary regime change. So what have we learned about the usefulness of this prescription since 2008?

The first thing we’ve learned, disappointingly, is how hard it is to get central bankers to consider a regime change.

Since the 1990s there has been a wide consensus that responsible monetary policy, with a due concern for price stability, means having an inflation target of 2 percent. Why 2, as opposed to 1 or 3? Well, that's a funny story. Partly it's New Zealand's fault: as the first central bank to explicitly adopt an inflation target, their choice has had an effect utterly disproportionate to their economic weight or population (even if you include the sheep.)

But 2 percent also seemed to be a good compromise among different factions in the economic policy community. Those who believed that "price stability" should mean zero inflation were willing to accept 2 percent on the grounds that given unmeasured gains from technological progress, 2 might really be zero. Meanwhile, as mentioned before, those who worried about hitting the zero lower bound believed that 2 was high enough to largely eliminate that concern. And 2 percent inflation also seemed high enough to eliminate most problems associated with downward nominal wage rigidity.

We now know, however, that the assumptions underlying the 2 percent solution were all wrong. Despite a 2 percent inflation target, the U.S. spent 7 years at the zero lower bound, and the euro area is still there. Downward nominal wage rigidity turned out to be a significant issue in the United States, and a huge issue in southern Europe, where it greatly increased the difficulty of achieving internal devaluation in Spain and especially Portugal.

Given the way experience has undermined much of the original case for a 2 percent inflation target, and given the severity of the economic crisis, you might therefore have expected some

revision – a rise in the inflation target, or a shift to some other kind of targeting – price level or nominal GDP targeting. But that hasn't happened. Even though a 2 percent inflation target is an essentially arbitrary number, it has become a focal point, a sort of token of respectability that almost no central bankers are willing to meddle with. (In this sense it resembles the role once played by the gold standard.)

This is quite remarkable. If the worst economic crisis since the 1930s, one that cumulatively cost advanced nations something on the order of 20 percent of GDP in foregone output, wasn't enough to provoke a monetary regime change, it's hard to imagine what will.

This in turn might seem to suggest that while monetary policy could in principle offer a solution to the problem of the zero lower bound, fiscal policy ends up being the only realistic tool. Unfortunately, fiscal responses were pretty bad too: a brief, modest turn toward stimulus at the beginning of the crisis was followed by austerity in the midst of high unemployment and impotent monetary policy.

Aside from the economic and human costs of this failure to act, the unwillingness and/or inability of central bankers to even attempt regime change means that we don't have much evidence on whether the monetary policy prescriptions from liquidity-trap analysis could work in practice. There is, however, one exception: the place where it all began, Japan.

When he became Prime Minister in 2012, Shinzo Abe – heretofore known as a conservative on many issues – surprised the world by endorsing a fairly radical monetary experiment.

“Abenomics” was supposed to contain three “arrows” – fiscal stimulus and structural reform as well as monetary expansion. In practice, however, fiscal policy has if anything tightened slightly, while structural reform, as often happens, is in the eye of the beholder. There has, however, been a very visible shift not just in the Bank of Japan’s actions but in its underlying attitude: while it still professes the conventional 2 percent target, it gives every indication of being willing to be far more adventurous than in the past in its efforts to achieve that target.

Kurodanomics

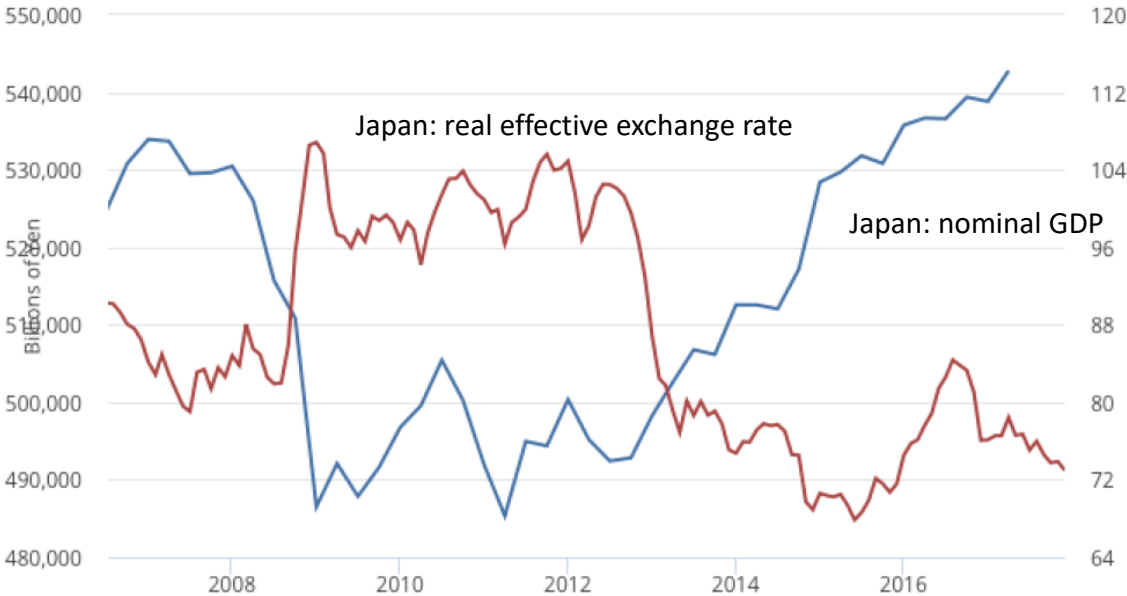


Figure 6

So how is Kurodanomics – a much better description in practice than Abenomics – working?

Figure 6 shows two indicators, nominal GDP and the real effective exchange rate. Despite being

at (or slightly below) the zero lower bound, the Bank of Japan evidently managed to achieve considerable traction. It has not so far managed to achieve the inflation target, but at least the Japanese experiment suggests some support for the view that monetary regime change can be effective even at the zero lower bound. Credibly promising to be irresponsible makes a difference; the problem is that central bankers won't do it.

Before pronouncing Kurodanomics a vindication of the 1998 view, however, I would raise a question that worries me both about Japan and about the role of monetary policy in general: what happens to the analysis if we really do face a future of secular stagnation?

Since there still seems to be a lot of confusion – willful confusion? – about what secular stagnation means, let's be clear: it has nothing to do with the economy's growth rate, certainly not in the short run and maybe not even in the long run. Instead, it's the proposition that for whatever reason the natural rate of interest, the interest rate consistent with full employment, will on average be negative for a long time. This in turn means that liquidity-trap conditions will become the norm, not the exception – rather than an economy that goes through brief episodes at the zero lower bound in the aftermath of exceptional busts, we will have an economy in which the ZLB is binding most of the time, except during exceptional booms/bubbles.

This was *not* the way I modeled the liquidity trap in 1998, or how Eggertsson and Woodford modeled it in 2003. In these papers the assumption was that after a period of depressed

demand, the economy would return to a regime with a positive natural rate of interest. Once the economy returned to “normal”, conventional monetary policy would regain traction. So what the central bank needed to do was commit to a higher price level in this future period when it would have regained its normal superpowers.

But if a negative natural interest rate is the new normal, how can the central bank gain traction? The answer seems to be that it must create a self-fulfilling prophecy of higher inflation: it must convince the market that it will achieve inflation; this higher expected inflation reduces real interest rates; and lower real interest rates create an economic boom that generates the expected inflation.

This is obviously even harder than convincing the market that there has been a monetary regime change. And it also raises the prospect of what I’ve called the timidity trap: if the inflation target is set too low, it won’t generate the required economic boom even if markets believe the central bank will hit it. I find this especially worrisome for Japan, where demographic factors – a rapidly shrinking working-age population – suggest that the conventional 2 percent target might well be too low to achieve economic escape velocity.

But let me not end on a down note. I began this paper by framing it as an assessment of how modern liquidity-trap analysis, brought into being two decades ago in an attempt to make sense of Japan’s problems, has fared in the aftermath of a global crisis that produced Japan-like conditions in many countries. And the answer, I’d argue, is that it has done very well.

Put it this way: If economists were like natural scientists, we'd be celebrating the success of our standard model. Confronted with conditions very different from those encountered in the past, the model made predictions very much at odds with the expectations of many policymakers and market participants. And those predictions proved correct.

Now, it's true that policymakers by and large ignored this successful analysis, with ugly results for the real world. It's also true that essentially nobody who was wrong has admitted error, or changed his views. But aside from that, this is basically a happy story.

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