

Distortions in Macroeconomics

Olivier Blanchard*

June 18 2017

After-dinner talks are the right places to test tentative ideas hoping for the indulgence of the audience. Mine will be in that spirit, and reflect my thoughts on what I see as a central macroeconomic question: What are the distortions that are central to understanding short-run macroeconomic evolutions?

I shall argue that, over the past 30 years, macroeconomics had, to an unhealthy extent, focused on a one-distortion (nominal rigidities) one-instrument (policy rate) view of the macro economy. As useful as the body of research that came out of this approach was, it was too reductive, and proved inadequate when the Great Financial crisis came. We need, even in our simplest models, to take into account more distortions. Having stated the general argument, I shall turn to a specific example and show how this richer approach modifies the way we should think about policy responses to the low neutral interest rates we observe in advanced economies today.

Let me develop this theme in more detail.

Back in my student days, i.e. the mid-1970s, much of macroeconomic research was focused on building larger and larger macroeconometric models, based on the integration of many partial equilibrium parts. Some researchers worked on explaining consumption, others on explaining investment, or asset demands, or price and wage setting. The empirical work was motivated by theoretical models, but these models were taken as guides rather than as tight constraints on the data. The estimated pieces were then put together in larger

*Talk, NBER Macroeconomics Annual Conference, April 2017. I thank Marty Eichenbaum, Jonathan Parker, and Adam Posen for comments.

models. The behavior captured in the estimated equations reflected in some ways both optimization and distortions, but the mapping was left, it was felt by necessity, implicit and somewhat vague. (I do not remember hearing the word “distortions” used in macro until the 1980s)

These large models were major achievements. But, for various reasons, researchers became disenchanted with them. Part of it was obscurity: the parts were reasonably clear, but the sum of the parts often had strange properties. Part of it was methodology: Identification of many equations was doubtful. Part of it was poor performance: The models did not do well during the oil crises of the 1970s. The result of disappointment was a desire to go back to basics.

For my generation of students, three papers played a central role. One was the paper by Robert Lucas (1973) on imperfect information. The other two were the papers by Stanley Fischer (1977) and by John Taylor (1980) on nominal rigidities. While the approaches were different, the methodology was similar: The focus was on the effects of one distortion: imperfect information leading to incomplete nominal adjustment in the case of Lucas, and explicit nominal rigidities, without staggering of decisions in Fischer, with staggering of decisions in Taylor. All other complications were cast aside, to focus on the issue at hand, the role of nominal rigidities and the implied non-neutrality of money.

Inspired by these models, further work then clarified the role of monopolistic competition, the role of menu costs, the role of different staggering structures, showing how each of them shaped the dynamic effects of nominal shocks. The natural next step was the re-integration of these nominal rigidities in a richer, micro-founded, general equilibrium model. The real business cycle model, developed by Kydland and Prescott (1982), provided the simplest and most convenient environment. Thus, was born the New-Keynesian (NK) model, a slightly odd marriage of the most neo-classical model and an ad-hoc distortion. But it was a marriage that has held together to this day.

In the hands of researchers like Woodford (2003) or Clarida, Gali, Gertler (1999), the model provided the basis, or at least the intellectual support, for the development of a new approach to monetary policy, i.e. inflation targeting, an approach adopted by most central banks around the world. It had a rich set of implications, with their origin deriving from the basic conceptual structure: One distortion, i.e. nominal rigidities in some form (often the convenient

Poisson form derived by Calvo), and one instrument, the nominal policy rate. The right use of the instrument could largely undo the distortion. Maintaining constant and low inflation would both minimize distortions and lead to the right level of output, a proposition Jordi Gali and I baptized, tongue in cheek, the Divine Coincidence (Blanchard and Gali 2007).

What I have described is obviously a caricature. First, but this is minor, there had to be at least another distortion: To talk about price setting, firms had to have some pricing power, and this led to a monopoly markup. Under Dixit-Stiglitz constant elasticity assumptions, the markup however was constant, and the effects of the distortion were largely irrelevant with respect to the effects of monetary policy. Second, some models had more than one nominal rigidity, for example rigidities in both wage and price setting as in Erceg, Henderson and Levin (2000); some models combined real and nominal rigidities, for example in my work with Gali (2007). Third, there was important work on credit, for example by Bernanke and Gertler (1989), and on liquidity, for example by Diamond and Dybvig (1983), or by Holmstrom and Tirole (1998). But, while these papers were well known and some of these mechanisms were integrated in DSGE models, they did not become part of the basic model. (I remember telling Bengt that, while I admired his work on liquidity with Jean, I was not sure how central it was to macro) Stable and low inflation as the target, and the use of the policy rate as the instrument, remained the basic approach to policy.

Even before the Great Financial Crisis, I felt some unease with two characteristics of the basic model and its larger DSGE cousins. (Blanchard, 2009) The first was that the deep reasons behind nominal rigidities, such as the costs of collecting information or of taking decisions were probably relevant beyond price or wage setting, and thus were relevant for consumption, investment, portfolio choices, with important but neglected implications for macroeconomic dynamics. The second that the models assumed much too much forward lookingness to agents. When combined with rational expectations, the implications of the Euler equation for consumption, or the interest parity condition for exchange rates, were simply counterfactual.

The financial crisis then made it clear that the basic model, and even its DSGE cousins, had other serious problems, that the financial sector was much more central to macroeconomics than had been assumed. Financial

markets were incomplete, raising issues of solvency and liquidity. The role and the importance of debt were central to understanding credit booms and busts. Bank runs were not just a historical footnote, but an essential aspect of maturity transformation. These distortions were at the core of the crisis; nominal rigidities may have made it worse; but even absent nominal rigidities, the financial crisis would likely have led to a large decrease in output.

Since the start of the crisis, DSGE models have been extended to allow for a richer financial sector, and integrate some of these distortions (for example, Gertler and Kiyotaki (2013)) But I feel we still do not have the right core model. Put another way, suppose that we were building a small macroeconomic model from scratch. What are, say, the three distortions we would deem essential to have in such a model, and, by implication, to have as the core of any DSGE model? What model should we teach at the start of the first year graduate course? ¹

I do not have the answer, but I have a few ideas. This is where my talk becomes even more tentative.

My first distortion would remain nominal rigidities. As much as I try, I just cannot interpret macroeconomic evolutions without relying on nominal rigidities. Proof of their relevance is in the ability of central banks to maintain their desired nominal and real interest rates over long periods of time, or in the dramatically different behavior of real exchange rates under fixed and flexible exchange rate systems (Mussa 1986).

My second distortion would be finite horizons. Not so much the finiteness which comes from death and the absence of operative bequest motives. But the finite horizon which comes from bounded rationality, from myopia, from the inability to think too far into the future.

My third distortion would be in the role of own funds in spending decisions, whether it is capital for banks, capital or collateral for firms or people. While it was only one of many distortions at play in the financial crisis, it can explain much of what happened, and how shocks affect financial intermediation.

How I would actually put them together in a basic model is a much harder question, the difference between a dinner talk and a serious paper. We have

¹This may a hopeless and misguided search. Maybe even the simplest characterization of fluctuations requires many more distortions. Maybe different distortions are important at different times. Maybe there is no simple model... I keep faith that there is.

off-the-shelf formalizations for nominal rigidities, for myopia, for capital constraints, for example Calvo for the first, Gabaix for the second, Holmstrom-Tirole (1997) for the third.² Each of them has its strengths and weaknesses, and whether they fit together conceptually is not obvious. (On this, I like the remarks by Cochrane (2016) on the potential misuse of the Gabaix formalization of myopia). In thinking about how to combine these or other formalizations, I still struggle between keeping strictly to micro foundations, or writing plausible characterizations more faithful to the empirical evidence but more loosely connected to theory (this is the old discussion between the pros and cons of the IS-LM versus the NK model, and whether there is a middle way). But this is a separate set of methodological issues, which I shall leave aside here.

The low real safe rate and macroeconomic policy

For better or for worse, simple conceptual frames such as the NK model strongly shape and limit our thinking. With the above discussion in mind, let me take an example, namely the potential policy implications of the very low level of the policy rate needed to maintain output at potential, the so-called neutral rate.³

Nearly all the discussion about policy implications has focused on monetary policy. In the one-distortion, one-instrument, view of the economy, so long as the policy rate does not hit the zero lower bound, the low neutral rate does not pose a particular problem: The central bank should simply choose a policy rate consistent with this low neutral rate. At the zero lower bound (or, to the extent that we now know that policy rates can be at least slightly negative, the “effective lower bound”), the issue becomes the degree to which financial assets are imperfect substitutes, and how the policy tool kit must be extended to allow for purchases of specific assets. This is indeed how, for the most part, both the policy discussion and policy actions have unfolded.

The figure below suggests however that the discussion should be more ambitious. It shows the evolution of the one-year real rate (constructed as the

²A fascinating question is why the Euler equation fails. One hypothesis is because of bounded rationality, for example a la Gabaix. Another is because of borrowing constraints, for example a la McKay et al (2016). The answer is probably both. Interestingly, both lead, at least to a close approximation, to a similar modified Euler equation.

³After giving the talk, I was made aware of an article by Davig and Gurkyanak (2015) which has a closely related theme.

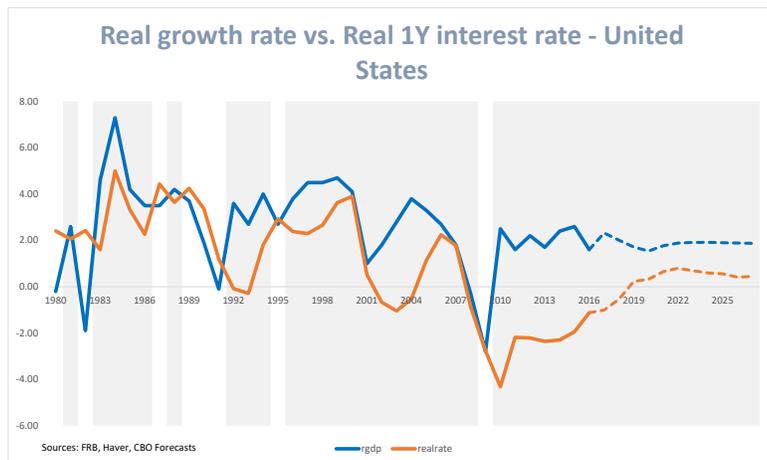


Figure 1:

difference between the one-year Treasury rate minus the corresponding CBO forecast of inflation) and the real growth rate in the United States since 1980. The one-year real rate has indeed come down since the early 1980s. And, interestingly, it is now substantially below the growth rate, and expected to be below it for the foreseeable future. This raises two interesting possibilities:

The first is that the low policy rate reflects a low marginal product of capital, and that the US economy has become dynamically inefficient. This could be the case if for example, consumers had finite horizons, either for physical reasons as in the overlapping generation model (should we call death a distortion?) or because of bounded rationality or borrowing constraints, as discussed above. If this were the case, the right policy tool would not be monetary policy, but rather policies aimed at decreasing saving. The right focus should be on fiscal policy. The right policy would be to increase public debt, and such a policy could be Pareto improving.

As exciting as this possibility would be, it does not appear however that this is the right explanation for the low safe rate. What matters for dynamic inefficiency is not the relation between the safe rate and the growth rate, but between the marginal product of capital and the growth rate. And the empirical evidence on the marginal product is that it has remained much higher than the growth rate.

This leads to the second hypothesis. That the difference between the marginal product and the safe rate has increased, leading to a low safe rate for a given marginal product. Put another way, it points to a large liquidity or risk premium. This in turn leads to a focus on the factors behind the premium, and the role of distortions in financial markets. Thinking of the premium as a risk premium, it takes us back to the equity premium puzzle identified by Mehra and Prescott (1985), and the various tentative resolutions to the puzzle. Thinking of the premium as a liquidity premium, it takes us to what is behind the demand for safe assets, along the lines of Caballero and Farhi (2014). It leads us to think about the role of financial regulations, and thus the role of regulatory policy. And if the high premium reflects, at least in part, distortions, the focus should then be on both fiscal and financial policies. If, for example, the safe rate is going to remain below the marginal product of capital, this implies that the government can borrow, never repay the debt, and still maintain a stable debt to GDP ratio. Should it do it? The fact that it can does not mean that it should. Or, to the extent that various distortions are behind the premium, should it instead remove them, even if this means a higher safer rate, and thus a higher cost of public borrowing?

My intention here was not to give the answers, but to show how much a richer view of the relevant distortions leads to a richer discussion of policy. To repeat and conclude: We must move from a dominant “one distortion/one instrument” to a “many distortions/ many instruments” view of the economy. In doing so, the way we think about the economy, and about the appropriate policies will be much more fertile

References

- [1] Ben Bernanke and Mark Gertler. Agency costs, net worth, and business fluctuations. *American Economic Review*, 79-1:14–31, March 1989.
- [2] Olivier Blanchard. The state of macro. *Annual Review of Economics*, 1:209–228, 2009.
- [3] Olivier Blanchard and Jordi Gali. Real rigidities and the New Keynesian model. *Journal of Money, Credit, and Banking*, pages 35–66, 2007.
- [4] Ricardo Caballero and Emmanuel Farhi. The safety trap. February 2014. NBER working paper 19927.
- [5] Richard Clarida, Jordi Gali, and Mark Gertler. The science of monetary policy: A New Keynesian perspective. *Journal of Economic Literature*, 37:1661–1707, December 1999.
- [6] John Cochrane. Comments on a behavioral New-Keynesian model by Xavier Gabaix. October 2016. mimeo, University of Chicago.
- [7] Troy Davig and Refet Gurkaynak. Is optimal monetary policy always optimal? *International Journal of Central Banking*, 11(S1), September 2015.
- [8] Douglas Diamond and Philip Dybvig. Bank runs, deposit insurance, and liquidity. *Journal of Political Economy*, 91(3):401–419, June 1983.
- [9] Christopher Erceg, Dale Henderson, and Andrew Levin. Optimal monetary policy with staggered wage and price contracts. *Journal of Monetary Economics*, 46-2:281–313, October 2000.
- [10] Stanley Fischer. Long term contracts, rational expectations and the optimal money supply rule. *Journal of Political Economy*, 85:191–205, 1977.
- [11] Xavier Gabaix. A behavioral New Keynesian model. December 2016. NBER working paper 22954.
- [12] Mark Gertler and Nobuhiro Kiyotaki. Banking, liquidity, and bank runs in an infinite-horizon economy. June 2013. NBER working paper 19129.

- [13] Bengt Holmstrom and Jean Tirole. Financial intermediation, loanable funds, and the real sector. *Quarterly Journal of Economics*, 112-3:663–691, August 1997.
- [14] Bengt Holmstrom and Jean Tirole. Private and public supply of liquidity. *Journal of Political Economy*, 106-1:1–40, February 1998.
- [15] Finn Kydland and Edward Prescott. Time to build and aggregate fluctuations. *Econometrica*, 50:1345–1370, 1982.
- [16] Robert Lucas. Some international evidence on the output-inflation trade-off. *American Economic Review*, 63(3):326–334, 1973.
- [17] Alisdair McKay, Emi Nakamura, and Jon Steinsson. The power of forward guidance revisited. *American Economic Review*, 106(10):3133–3158, 2016.
- [18] Rajnish Mehra and Edward Prescott. The equity premium. *Journal of Monetary Economics*, 15:145–161, 1985.
- [19] Michael Mussa. Nominal exchange rate regimes and the behavior of real exchange rates: Evidence and implications. *Carnegie-Rochester Conference Series on Public Policy*, 25:117–214, 1986.
- [20] John Taylor. Aggregate dynamics and staggered contracts. *Journal of Political Economy*, 88(1):1–24, 1980.
- [21] Michael Woodford. *Interest and Prices: Foundations of a Theory of Monetary Policy*. Princeton University Press, 2003.