

When Does Policy Reform Work? The Case of Central Bank Independence. Comments and Discussion

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Comments and Discussion

COMMENT BY

ALBERTO ALESINA This paper by Daron Acemoglu and his coauthors addresses an important topic. In general terms, the question is when and how policy reforms are successful or unsuccessful. More specifically, the paper focuses on central bank independence (CBI), and its goal is to shed some light on the inconclusive results in the literature regarding the benefits of CBI on inflation. I will focus relatively briefly on the authors' model and more extensively on the empirical evidence. I conclude that the model is not especially appropriate for studying this issue and that the empirical evidence is not robust.

THE MODEL. What models have been used in the literature to study the pros and cons of CBI? The first was the model of time inconsistency and inflation bias developed by Finn Kydland and Edward Prescott and later extended by Robert Barro and David Gordon.¹ The source of inflation in all these models is an interplay between wage setting behavior and the incentive of the central bank to "surprise" the economy with an inflation shock to reduce unemployment. Kenneth Rogoff pointed out how an inflation-averse and independent central banker can improve on the trade-off between rules and discretion.²

A second approach emphasizes the political influences on monetary policy, in particular, political business cycles, both "opportunistic" and "partisan." CBI can limit the inefficient fluctuations of inflation created by

1. Finn E. Kydland and Edward C. Prescott, "Rules Rather Than Discretion: The Inconsistency of Optimal Plans," *Journal of Political Economy* 85, no. 3 (1977): 473–91; Robert Barro and David Gordon, "Rules, Discretion, and Reputation in a Model of Monetary Policy," *Journal of Monetary Economics* 12, no. 1 (1983): 101–21.

2. Kenneth Rogoff, "The Optimal Degree of Commitment to an Intermediate Monetary Target," *Quarterly Journal of Economics* 100, no. 4 (1985): 1169–89.

political cycles. In this model the source of inflation is monetary policy that is excessively loose because of political influences. Alesina, Gerald Cohen, and Nouriel Roubini discuss this approach in detail.³

The third type of model focuses on biases toward fiscal deficit and the pressure on central banks to accommodate such deficits.⁴ In this context CBI is part of a package that ensures fiscal responsibility and avoids all excessive use of the inflation tax. Fiscal deficits are the source of inflation in this model.

The model of this paper is quite different. There is a lobby pushing for inflation, but the general public is against inflation. Not much is said in the paper about which lobby that would be in reality, nor does the paper explain why the lobby benefits from inflation. This is a good model for other issues, such as trade protection or regulation of certain sectors. And in fact, similar models have been widely used to discuss how policies that are inefficient for society as a whole are adopted to favor a specific, narrowly defined constituency. But this is not a good model for explaining why countries find themselves in a high-inflation equilibrium. Inflation is almost universally the result of some form of macroeconomic imbalance, not the effect of a small organized lobby pushing for it. The implications in terms of which governments are more likely to be successful and which will undertake reforms are different. The authors mention in passing that the model is meant to be illustrative, but I am not at all sure how their empirical implications would generalize to a more aggregate model of inflation along the lines sketched above.

An additional issue concerns the endogeneity of central bank reform. Many authors share the same problem that this paper has,⁵ namely, the assumption that central bank laws are considered an exogenous variable, an issue first raised by Adam Posen.⁶ However, I feel that this shortcoming is especially serious for this paper. In fact, it discusses interactions between political regimes and central bank reforms and the role of lobbies in pushing for more or less inflationary policies. The same political vari-

3. Alberto Alesina, Gerald Cohen, and Nouriel Roubini, *Political Cycles and the Macroeconomy* (MIT Press, November 1997).

4. For an extended discussion, see Torsten Persson and Guido Tabellini, *Political Economics: Explaining Economic Policy* (MIT Press, 2002).

5. For example, Alberto Alesina and Lawrence Summers, "Central Bank Independence and Macroeconomic Performance: Some Comparative Evidence," *Journal of Money, Credit and Banking* 25 (May 1993): 151–62.

6. Adam Posen, "Declarations Are Not Enough: Financial Sector Sources of Central Bank Independence," *NBER Macroeconomic Annual* 10 (1995): 253–74.

ables that make it more or less likely that a central bank reform will be successful are at the root of the question of whether central bank reform is adopted in the first place.

THE EMPIRICAL EVIDENCE. Here I will focus on some sensitivity analysis of the paper's two key tables, table 2 and table 4.⁷ First of all, the results as presented in the paper are not especially strong. The theory has implications for the differences between coefficients for three groups of countries defined in terms of the strength of executive constraints. In many cases, as the authors acknowledge, these coefficients, although different from zero, are not statistically different from *each other*.

My first sensitivity test examined to what extent the results were driven by the Latin American countries, since this is a region that has experienced much inflation and several central bank reforms. I found that, in table 2, dropping Argentina makes all the relevant coefficients insignificant in the full sample; similar patterns hold for table 4. The results are thus very sensitive to dropping one country.

Then I examined the groupings. The authors define their country groups as follows: the intermediate group includes all the countries within one standard deviation of the mean of their constraints index, and the strong and weak groups include those more than one standard deviation above and below the mean, respectively. The problem is that the distribution of countries is very asymmetric: 36.5 percent of the countries are bunched at the top of the distribution, with an index value of seven. Thus, the authors' distribution is as follows:

- strong constraints (index of 7): 36.5 percent of the countries
- intermediate constraints (index between 3.92 and 6.99): 48 percent
- weak constraints (index between 0 and 3.2): 15.5 percent (that is, a handful of countries).

The results hold only if the weak-constraints group is restricted to a very small set of countries. I tried two alternative groupings that seem more reasonable:⁸

- strong constraints: 36.5 percent
- weak constraints: 36.5 percent
- intermediate constraints: the rest,

7. I thank the authors for providing their data and codes, which made it very easy to replicate and examine their results.

8. Note that, because of the bunching at the top, one cannot quite split the sample evenly into thirds.

and

- strong constraints: 36.5 percent
- intermediate and weak constraints: split 50-50 between the remaining countries.

With either grouping, the results of the paper disappear: the weak-constraints group has a negative and significant coefficient, and the middle group has an insignificant and often positive coefficient. Thus even moving a handful of countries from the middle group to the weak group changes the results. Therefore the strategy of looking at the interaction of a quadratic function of the executive constraint variable with the CBI variable does not guarantee robustness to the grouping classification, contrary to the authors' claims.⁹

In conclusion, investigating the success of policy reforms in alternative institutional settings is a very good idea, but the paper does not use the most appropriate model to address the issue at hand, and thus it derives implications that may not be plausible. Moreover, the empirical evidence is not robust.

COMMENT BY

DAVID ROMER The thesis of this paper by Daron Acemoglu, Simon Johnson, Pablo Querubín, and James Robinson is simple but important. There has been a vast amount of work on the political economy of poor policies. Yet when it comes to thinking about the effects of policy reform, economists and policymakers too often still adopt the perspective of a benevolent social planner and assume that reforms will be implemented as intended. The basic insight of the paper is that in thinking about the effects of reform, it is a first-order mistake to overlook the forces that gave rise to the need for reform in the first place. Considering those forces can critically change what one should expect. Often the change is particularly stark: if a country is following poor policies because the rule of law is not respected, reforms that consist of passing new laws are unlikely to change anything at all. These core ideas more than repay the effort of reading the paper.

THE THEORY. The authors develop their thesis both theoretically and empirically, focusing on how central bank independence affects inflation.

9. The results of all these sensitivity tests are available from the author upon request.

The main result of their theoretical model is that central bank reform is unlikely to reduce inflation much either in countries with unconstrained leaders (because the reform will be ineffective) or in countries with highly constrained leaders (because inflation will already be low). The largest effect is likely to occur in countries with moderate levels of constraints on their leaders.

I have three main comments about the theory. First, it appears that nothing critical hinges on the paper's assumption that excessive inflation is the result of special-interest lobbying. For example, suppose inflation arises instead from dynamic inconsistency or the political business cycle. Central bank reform would be unlikely to reduce inflation if power in fact remained in the hands of politicians, but would be likely to do so if the reform genuinely put control of monetary policy in the hands of an independent central bank. The same is true if—to mention my favorite theory of high inflation—inflation occurs because politicians understand the short-run benefits of demand stimulus but not the costs of inflation or the difficulty of bringing it down. The fact that the theoretical results do not rest on a specific view of the source of excessive inflation has the advantage that the theory is not tied to the debatable view that inflation is the result of redistributive efforts. But it has the disadvantage of reducing the ability of tests based on the theory to discriminate among competing views of the reasons for poor policies.

Second, the prediction of an inverted U-shaped relationship between constraints on politicians and the effects of central bank reform on inflation seems unlikely to be robust. There are two competing effects. On the one hand, when constraints on politicians are weaker, the prereform situation is worse, so the room for improvement is greater. On the other, when constraints are stronger, the extent to which nominal reforms are likely to have force is greater. This suggests that the overall effect can go either way, but it does not suggest a strong reason for expecting an inverted U-shaped relationship in particular. It is not difficult to find changes in functional forms or other assumptions of the model that alter the prediction of an inverted U-shaped relationship. To give a simple example, just dropping the assumption in the paper's model that inflation cannot be negative changes the relationship from an inverted U to monotonically increasing.

Third, a prediction that is likely to be more robust is that if one controls for initial inflation, the amount that central bank reform reduces inflation will be monotonically increasing in the strength of political constraints. Controlling for initial inflation eliminates one of the two competing effects, and so leaves only the fact that nominal reforms are likely to have more force when politicians are more constrained.

The paper's model is not well suited to analyzing this issue, for two reasons. First, the prereform level of inflation is completely determined by the extent of constraints, so once one controls for initial inflation there is no remaining variation in political constraints. Second, the model's assumption that inflation cannot be negative means that inflation is sometimes at a corner solution. This is important to the model's implications, but it seems quite unrealistic and complicates the analysis.

I therefore consider a modest variation on the paper's model. First, I replace the paper's social welfare function with a conventional quadratic one,

$$(1') \quad u(\pi) = -\frac{b}{2}\pi^2, \quad b > 0,$$

where I have normalized the socially optimal level of inflation to zero. Second, I assume that the lobbyists' preferred inflation rate (or, more generally, the inflation rate that would prevail in the absence of constraints on politicians) varies across countries. Specifically, I assume that in the lobbyists' utility function, $w(\pi, t) = \alpha\pi - \frac{\beta}{2}\pi^2 - t$ (the paper's equation 3),

α may be heterogeneous. And third, I model central bank reform as an increase in the social cost of departures of inflation from its optimal level—that is, as an increase in b . Thus, I interpret central bank reform as an increased emphasis in the conventional (noncorrupted) policymaking arena on reaching the optimal level of inflation. The rest of the model is the same as the paper's. The politician maximizes $\lambda u(\pi) + (1 - \lambda)t$, where λ measures the strength of constraints on politicians and t is the transfer from the lobby, and the lobby makes a take-it-or-leave-it offer to the politician.

Analyzing the model along the lines of the paper yields an expression for equilibrium inflation:

$$(7') \quad \pi = \frac{\alpha}{\beta + \frac{\lambda}{1 - \lambda}b}.$$

This expression has two implications. First, it shows that my variant of the authors' model captures their idea that reform does not affect inflation if political constraints are either very low or very high. When λ is zero (politicians are unconstrained), inflation equals lobbyists' preferred level of α/β regardless of how much weight is given to achieving low inflation in the conventional policymaking process (b). And when λ is one (politi-

cians are completely constrained), lobbyists have no influence, and so inflation is zero for any level of b . More generally, equation 7' implies the inverted U-shaped relationship that the authors' emphasize.

The second implication of equation 7' is that

$$\frac{\partial \pi}{\partial b} = -\left(\frac{\lambda}{1-\lambda}\right)\left(\frac{1}{\beta + \frac{\lambda}{1-\lambda}b}\right)\pi.$$

That is, at a given level of inflation, the amount by which inflation falls in response to central bank reform is monotonically increasing in the strength of political constraints. The intuition is exactly that described above: trying to correct a bad outcome through some kind of nominal reform is more effective when the rule of law is stronger.

THE EMPIRICAL WORK. Let me now turn to the empirical findings. One of the most interesting aspects of the results is the absence of a strong correlation between increases in central bank independence and decreases in inflation. This is most easily seen in the figures in appendix B, which are a marvelous feature of the paper. Over the paper's sample period, forty countries increased the independence of their central bank. But in thirty-four of those countries, inflation relative to the world average either showed no clear change from its behavior before the reform or, in a number of cases, rose modestly but clearly. In only six countries was inflation following the reform clearly lower than one would have expected based on its prior behavior. This is hardly a stunning endorsement of the anti-inflationary power of central bank independence.

One can also see from these figures why the authors obtain their result about the relationship between political constraints and the behavior of inflation following central bank reform. The six countries where inflation clearly fell following reform are Argentina, Mongolia, Nicaragua, Peru, Turkey, and Uruguay; all six are in the middle group in terms of political constraints. Since only slightly more than half of the forty countries that implemented reform are in this group, this pattern would be very unlikely to occur by chance.

Looking at these correlations is interesting, and the finding that increases in central bank independence are associated with falls in inflation only in countries with intermediate levels of political constraints is intriguing. At the same time, I do not think one should get overly excited about this result, for three reasons.

First, as the authors emphasize, in most specifications the effect is only moderately statistically significant.

Second, there is considerable room for improvement on the authors' measure of political constraints. The authors employ the average of the Polity IV measure of constraints on the executive over their full sample period, 1972–2004. This means that if a country reformed its central bank in, say, 1992, the authors' measure puts as much weight on political constraints twenty years before the reform, and ten years after the reform, as at the time of the reform. If another country had its reform in 2002, the paper's measure for this country goes back thirty years rather than twenty.

I think it is clear that, conceptually, one would like to know how constrained politicians were at the time of the reform. The extent of constraints at some point decades earlier or a decade later should not matter. What should matter for whether reform puts genuine control over monetary policy in the hands of a legally independent central bank is whether the rule of law is respected when the reform takes place. Because the Polity IV measure is not perfect, its lagged values surely convey some information about current constraints given its current value. But this hardly makes a case for the paper's approach of treating each year identically.

Putting much more weight on the measure of constraints near the time of the central bank reform changes one's views of many of the reforms. Consider the countries that the authors classify as having an intermediate level of constraints. Four of these countries (Bolivia, Spain, Turkey, and Uruguay) achieved the highest possible level of constraints a full decade before their reforms and kept them at that level through the end of the sample, and four more (Ecuador, Greece, Mongolia, and Portugal) attained the highest possible rating at least three years before their reforms and maintained it well afterward. In addition, Chile had an essentially unconstrained executive for fifteen years before its reform (although the constraints jumped to the highest possible level in the year of the reform), and Peru had a very low level of constraints from the year before its reform through six years afterward. Thus, a more appropriate measure of the constraints relevant to the effects of central bank reform would look quite different from the authors' measure.¹

1. The results based on an alternative measure of political constraints reported in table 3 of the paper are not reassuring in this regard, for two reasons. First, this measure also does not systematically attempt to measure constraints around the times of central bank reforms. Second, the results with this measure suggest that central bank reform reduces inflation by at least as much under weak political constraints as under moderate constraints, which contradicts one of the paper's main hypotheses.

Third, the paper looks only at the model's predictions about changes in inflation in response to changes in central bank independence, while the model makes a rich set of predictions about the level of inflation and how it responds to changes in both central bank independence and political constraints. For example, as I described, the model makes predictions about the strength of political constraints and the fall in inflation following central bank reform controlling for initial inflation. And it appears that at least one of the model's major predictions about those additional dimensions fails. The model predicts that with weak political constraints, inflation will be high both before and after central bank reform. But as the authors mention, this is not what the data show: in the eight countries in the weak-constraints group, inflation has been generally below the world average in seven.

Thus, as with many other valuable papers, this paper's contribution lies more in raising issues than in resolving them. The paper does not come close to settling the issue of how the political economy forces that give rise to poor policies affect the consequences of policy reform. But it makes a compelling case that those effects are likely to be large and important.

GENERAL DISCUSSION Gregory Mankiw wondered how best to take political economy constraints into consideration when doing policy analysis. If all outcomes are assumed to be the result of political constraints, analyzing the welfare implications of alternative policies becomes a purely academic exercise. This paper seemed to propose a compromise: sometimes policy can be altered to improve overall welfare, but other times it is determined by politics. Mankiw was not sure how helpful this perspective would be in practice.

Justin Wolfers suggested that some of the data used in the paper may be unreliable. For example, the data from Zimbabwe indicate that the central bank is independent, which, practically speaking, is probably not the case. The lesson to draw from this paper may be not that central bank reform is futile when political institutions are weak, but rather that central bank reform is futile when institutions are weak *and* the reforms are not actually being implemented. Conversely, some countries, such as Australia, have de facto central bank independence before they formalize it in law, so the formal policy change appears to have little effect. This should not lead one to conclude that central bank independence is ineffectual in such countries, however.

Michael Woodford questioned the relationship between constraints on the executive and the effect of policy reform on inflation. In countries with strong constraints, changes in central bank law might simply be less correlated with other important monetary policy changes. For example, the paper cites as pertinent policy changes the 1999 Riksbank Law in Sweden and the 1998 Bank of England Act in the United Kingdom. In fact, the most important anti-inflationary policy changes in both countries probably occurred in 1992, following the crisis in the European fixed exchange rate regime. Likewise, the 1998 Bank Act in the Netherlands is cited as an important policy change, but probably the most important anti-inflationary actions were those taken following the Maastricht Treaty at the beginning of the 1990s. The paper does not list any major reform in the United States during the sample period because there were no changes to the Federal Reserve Act; however, there was an important change in the character of U.S. monetary policy with the accession of Paul Volcker as Federal Reserve chairman at the beginning of the 1980s.

Benjamin Friedman raised another issue that appears in the literature on central bank independence, namely, the correlation between independence and the country's sacrifice ratio, a measure of the output cost of lowering inflation. If central bank independence delivered lower inflation by anchoring inflation expectations, this would imply that if some supply shock were to cause inflation to increase unexpectedly, a country that has an independent central bank should be able to return to low inflation at lower cost than one that does not. However, it turns out that the correlation goes in the other direction: countries with more independent central banks tend to have worse sacrifice ratios. This casts doubt on the notion that the relationship between central bank independence and low inflation is causal, and it suggests that these phenomena may be jointly caused by some third variable. Therefore Friedman proposed that the authors look at the relationship between the variables they examine and the countries' sacrifice ratios to see what this implies about their model.

Edward Glaeser also recommended that the authors examine the interaction between central bank independence and other country-level variables. The observed correlation between central bank independence and constraints on the executive may be caused by a third variable that jointly determines both, such as GDP.

Gary Becker observed that shifts in political power often facilitate policy changes. Examining such power shifts, in addition to the legal reforms studied in the paper, might be useful. Olivier Blanchard noted that the reasons for central bank reform are important in influencing outcomes and are

missing from the paper's argument. Some countries adopt central bank independence because of pressure from international organizations or from other countries. Christopher Sims added that, in some instances, central bank independence is simply a bad idea that economists have forced onto a complicated and unfavorable political environment. Without the institutions and fiscal policies necessary for an independent central bank to be effective, central bank reform can become little more than an excuse for the legislature to abdicate responsibility for inflation.