Why We Learn Nothing from Regressing Economic Growth on Policies*

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Government use policy to achieve certain outcomes. Sometimes the desired ends are worthwhile, and sometimes they are pernicious. Cross-country regressions have been the tool of choice in assessing the effectiveness of policies and the empirical relevance of these two diametrically opposite views of government behavior. When government policy responds systematically to economic or political objectives, the standard growth regression in which economic growth (or any other performance indicator) is regressed on policy tells us nothing about the effectiveness of policy and whether government motives are good or bad.

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I. Introduction

Government use policy to achieve certain outcomes. Sometimes the desired ends are worthwhile, as is the case when policy is targeted on removing market failures. At other times, they are pernicious, as in the case when policies aim to create and distribute rents. Cross-country regressions have been the tool of choice to date in assessing the effectiveness of policies and the empirical relevance of these two diametrically opposite views of government behavior. This paper argues that such regressions are uninformative about the questions that motivate the analysis. The standard growth regression in which economic growth (or any other performance

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indicator) is regressed on policy tells us nothing about the effectiveness of policy and whether government motives are good or bad.

There is a voluminous empirical literature which attempts to estimate the effects of economic policy on growth. The typical cross-country growth regression takes the form

$$g_i = \alpha \ln y_{i0} + Z_i' \beta + \gamma s_i + \varepsilon_i$$

where s_i is a policy variable for country i, y_{i0} is initial income and Z_i is a vector of other covariates. Such growth regressions are sometimes specified in panel form, with growth and all left-hand side variables averaged over 5- or 10-year subperiods. The object of the exercise is to obtain an estimate of γ, the impact of policy intervention on growth. Regressions of this type are ubiquitous in academic research, as well as in policy work carried out by development agencies, where they are used to predict the effect of policy reforms.

The list of economic policies that have been included in cross-national regressions includes:

- fiscal policy (Easterly and Rebelo 1993)
- government consumption (Barro 1991)
- inflation (Fischer 1993)
- black market premia on foreign exchange (Sachs and Warner 1995)
- overvaluation of the exchange rate (Dollar 1992)
- financial liberalization (Eichengreen 2001)
- trade policy (Lee 1993)
- state ownership in industry or banking (La Porta, Lopez-de-Silanes, and Shleifer 2002)
- industrial policy (Ades and di Tella 1997)

While economic growth is the most frequently used measure of economic performance, sometimes other performance indicators such as productivity and investment are used as the dependent variable. Djankov *et al.* (2002) regress a variety of public goods (ranging from health outcomes to product quality standards) on regulations that restrict firm entry. Similar regressions are run also across industries or states/regions, regressing a performance variable on policies that apply at the relevant level. Besley and Burgess (2002), for example, analyze the impact of labor regulations on differential growth rates across Indian states.

As the empirical growth literature has grown, so has the critical eval-

uation of it. There is by now a wide-ranging discussion of the shortcomings of growth regressions, which focuses on problems relating to:

- parameter heterogeneity
- outliers
- omitted variables
- model uncertainty
- measurement error
- endogeneity

Temple (1999), Durlauf, Jonhson, and Temple (2005), and Easterly (2004) provide very useful recent critical surveys of the empirical growth literature. A dominant concern has been the lack of robustness. Levine and Renelt (1992) documented a while back that growth regressions are generally quite non-robust to variations in the set of conditioning variables. Sala-i-Martin, Doppelhofer, and Miller (2004) have tried to deal with this problem by Bayesian averaging of OLS estimates, to see which of the standard regressors are robustly correlated with growth. Easterly (2004) emphasizes that the large policy effects uncovered in growth regressions are typically driven by outliers — which represent instances of extremely "bad" policies.

The question analyzed here is how to interpret the estimated coefficients from such regressions when policies are not random but are used systematically by governments to achieve certain ends — whether good or bad. So the focus is on the endogeneity of the policy variables inserted on the right-hand side of the regression. Endogeneity problems are of course nothing new in growth regressions. But what is special here is that policy endogeneity is not just an econometric nuisance, but typically an integral part of the null hypothesis that is being tested. The supposition that governments are trying to achieve some economic or political objective is at the core of the theoretical framework that is subjected to empirical tests. In such a setting, treating policy as if it were exogenous or random is problematic not just from an econometric standpoint, but also conceptually.

My point is best made in the context of a specific application. Consider as an illustrative example an article by La Porta, Lopez-de-Silanes, and Shleifer (2002) in which the authors analyze the consequences of government ownership of banks around the world. The authors begin the article by distinguishing two perspectives on the role of government banks. The first perspective is a "developmental" one, which they attri-

bute to Alexander Gerschenkron. In this view, latecomers resort to state-ownership of the financial system to overcome market imperfections, mobilize resources, and catch up with advanced countries. The second perspective is a "political" one, in which government ownership allows politicians to transfer incomes to favored groups in return for their support. To discriminate between the two stories, La Porta *et al.* regress per-capita GDP and productivity growth on their measure of government ownership of banks (along with other standard regressors). This exercise reveals a robust negative relationship between government ownership and economic performance. The authors interpret this result as supportive of the political view, and inconsistent with the developmental view.

But there is a problem here. The cross-national variation we observe in government ownership is unlikely to be random by the very logic of the theories that are tested. Under the developmental perspective, this variation will be driven by the magnitude of the financial market failures that need to be addressed and the governments' capacity to do so effectively. Under the political motive, the variation will be generated by the degree of "honesty" or "corruption" of political leaders. I show in this paper that the cross-national association between performance and policy will have a very different interpretation depending on which of these fundamental drivers dominate. Unfortunately, none of these drivers is likely to be observable to the analyst.[1] In such a setting the estimated coefficient on state ownership is not informative about either the positive or the normative questions at stake. It cannot help us distinguish between the developmental and political views, because the estimated coefficient on government ownership will be negative in both cases. The intuition is straightforward: a government that cares about social welfare (and nothing else) will increase its policy intervention in response to larger market failures, but not so much as to completely insulate economic performance from their adverse consequence. A negative correlation between government ownership and growth might as well be taken as confirmation that governments are acting socially optimally! And under no circumstances can it tell us whether societies would be better or worse off if government ownership were legislated away (or, for that matter, made mandatory).

[1] That is why the problem cannot be treated as one of omitted variables (to be addressed by adding covariates to the specification) or parameter heterogeneity (to be addressed by splitting the sample). La Porta *et al.* (2002) follow both strategies.

A common defense of growth regressions is that despite all their problems they help us update our priors about the impact of certain types of policies. In the words of Wacziarg, "even simple or partial correlations can restrict the range of possible causal statements that can be made, and nowhere is this more the case than in the comparative growth literature, where causality is especially difficult to establish" (2002, p. 909). Consider an illustration from trade policy.[2] The estimated coefficient on import tariffs in growth regressions run for the contemporary period is typically negative (albeit insignificantly so) and rarely positive.[3] One frequently hears the argument that we can at least draw the conclusion from this fact that import protection cannot be beneficial to growth. But once again this and similar inferences are invalid. A negative partial correlation between growth and import tariffs is not only consistent with protection being growth-enhancing, it is actually an equilibrium consequence of trade protection being used in a socially optimal fashion.

The discussion on endogeneity in growth regressions has focused on outcome variables such as investment and trade ratios, where the concern has been that such outcome variables may be caused by (or jointly determined with) incomes. Surprisingly, there has been relatively little discussion of the consequences of policy endogeneity. The surveys by Temple (1999) and Easterly (2004), for example, barely pay lip service to this issue.[4] On the other hand, the microeconomic literature on policy evaluation has shown much more awareness of the biases introduced by policy endogeneity. For example, it is widely recognized that OLS estimates are unreliable when program placement is correlated with relevant features of a locality or determined optimally according to some objective function (*e.g.*, Rosenzweig and Wolpin 1986). The usual solutions to this problem are IV estimation and randomized trials. Neither of these two

[2] Wacziarg (2002) actually gives the example of corruption, arguing that the negative coefficient on corruption in growth regressions disproves the view, once held by some, that corruption could be a positive force for development. This logic suffers from the same problem discussed here. If indeed corruption is a second-best mechanism for getting around imperfections in the way that the economy operates — which is the argument that used to be made — the association between growth and corruption across countries will be negative.

[3] Yanikkaya (2003) is one exception.

[4] I have to confess that I am far from blameless here. In Rodrik (1998), I regressed growth on an indicator of capital-account liberalization to see if there was evidence that financial opening promotes growth. And in Rodriguez and Rodrik (2001), we critiqued a large body of literature which regresses growth on trade policy indicators without mentioning the problem analyzed in the present paper.

strategies is very promising when we are concerned with the impact of economic policies at the level of countries. So it is important to build some intuition about how interpretation can go astray when policies are selected endogenously.

The outline of the paper is as follows. The next section lays out a simple growth model that allows for policy to affect growth in both desirable and undesirable ways. The model is built around three unobservable parameters: (i) the honesty of the government; (ii) the extent of market imperfections; and (iii) the capacity of the government to intervene effectively. I next analyze the association between the level of policy intervention and growth under varying assumptions about which of these unobservables drives the cross-national variation in the data. The final section discusses some of the additional implications of the analysis.

II. Modeling the Sources of Cross-National Variation in Policy Interventions

We want to analyze the relationship between government policy and economic growth allowing for differences across countries in market imperfections, policy objectives, and government capabilities. We consider the simplest growth model, which takes the following linear form:

$$g = (1-\theta)A - \rho$$

where $\theta \in [0, 1]$ is an (unobservable) market-failure parameter, A is productivity, and ρ is impatience. (We fix the intertemporal elasticity of substitution to 1 to avoid carrying around extra parameters.) When $\theta > 0$, growth is reduced because of a wedge between social and private returns.

We suppose that the government has a policy tool at its disposal, denoted by s, that can increase private appropriability of the social returns. Let such policy intervention by government reduce market failure at agency cost (in growth terms) of $\phi\alpha(s)$, with $\alpha(0)=0$, $\alpha'(s)>0$, and $\alpha''(s)>0$. ϕ is just a shift parameter that allows us to vary "ability" or "policy effectiveness" across countries.

So the modified expression for growth can be written as

$$g(s, \theta, \phi) = (1-\theta(1-s))A - \phi\alpha(s) - \rho$$

For later reference, we define $g_s(s, \theta, \phi) = \theta A - \phi\alpha'(s)$. We denote the

growth-maximizing level of policy intervention by s^{**}, with s^{**} solving $g_s(s^{**}, \theta, \phi) = 0$.

A. The Politician's Problem

The politician can have both economic and political motives. We model this by assuming that the government cares about both growth (g) and the diversion of profits generated by policy intervention. Let the diversion function be given by concave and single-peaked function $\pi(s)$ (with $\pi'(s) > 0$ for small s, $\pi''(s) < 0$, $\pi(s^p) = 0$, and $s^p > s^{**}$). The relative weight placed on growth by the politician is λ (which is also unobservable, with $\lambda \in (0, \infty)$). The politician maximizes

$$\max_s u(s;\ \theta, \phi) = \lambda g(s, \theta, \phi) + \pi(s)$$

The first-order condition is:

$$\lambda g_s(s^*, \theta, \phi) + \pi'(s^*) = 0$$

The SOC condition is satisfied when $\alpha''(s) > 0$ and $\pi''(s) < 0$. Let the internal solution to this problem be given by $0 < s^* < 1$. We have $s^{**} < s^* < s^p$ as long as $\lambda \in (0, \infty)$. Therefore $\pi'(s^*) > 0$ and $g_s(s^*, \theta, \phi) < 0$. In words, a slight reduction in the equilibrium level of s would raise growth and increase political diversion.

B. Sources of Cross-National Variation

Countries can differ along many dimensions. We focus here on the variation in the following three parameters:

- λ: the degree to which the government cares about social welfare (the "honesty" of the government)
- θ: the extent to which markets are imperfect (the "need" for intervention)
- ϕ: the capacity of the government to intervene effectively (the "ability" of the government)

We will vary each in turn and analyze how the equilibrium levels of s and g respond.

C. Cross-National Variation Generated by λ

We want to know how s^* and g co-vary. So we check to see how changes in λ affect each of these endogenous variables.

$$\frac{ds^*}{d\lambda} = -\frac{g_s(s^*, \theta, \phi)}{\lambda g_{ss}(s^*, \theta, \phi) + \pi''(s^*)} < 0$$

Note that $g_s(s^*, \theta, \phi) < 0$ in equilibrium (the level of policy intervention is higher than the level that would maximize growth) as long as λ is finite. Further the SOC for the politician's problem ensures that the denominator is negative in the above expression. Similarly,

$$\frac{dg}{d\lambda} = g_s(s^*, \theta, \phi)\frac{ds^*}{d\lambda} > 0$$

Therefore,

$$\frac{dg/d\lambda}{ds^*/d\lambda} = g_s(s^*, \theta, \phi) = -\frac{\pi'(s^*)}{\lambda} < 0$$

So the cross-national association between the policy intervention and economic performance is negative (and zero in the limit as $\lambda \to \infty$).

Note that in this case, the observed correlation between s and g does tell us what we are interested in knowing, namely $\partial g/\partial s = g_s(s^*, \theta, \phi)$. It yields the correct answer to the question, how would a small increase in s affect g locally? Even though policy is endogenous, we recover the partial derivative that is of interest.

However, we are far from being home free. How we actually use this information for policy purposes is not entirely clear, since we have assumed policy is actually endogenous.

Suppose the "policy" question is of the following kind. Countries can choose between two policy regimes: (i) regime A where s is set equal to zero, and (ii) regime B where s is determined in the way presented above. Does the empirical finding that $\partial g/\partial s < 0$ allow us to say that regime A is preferable to regime B? Obviously not, since it is entirely possible that $g(s^*, \theta, \phi) > g(0, \theta, \phi)$ even though the politician is politically-motivated and puts less than infinite weight on growth (see Figure 1). To be sure that regime A is desirable, we need not only $\partial g/\partial s$ to be negative but for it to be "sufficiently" negative.

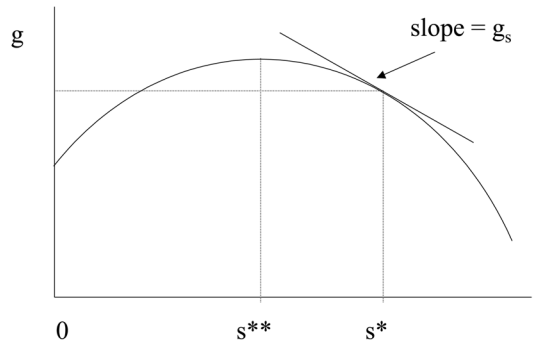

FIGURE 1

D. Cross-National Variation Generated by θ

Now assume countries differ along a different dimension, the size of the market imperfection and the need for policy intervention (captured by θ). We do the same exercise, with variation in θ this time:

$$\frac{ds^*}{d\theta} = \frac{\lambda A}{\lambda\phi\alpha''(s^*) - \pi''(s^*)} > 0$$

as long as $\lambda > 0$ (politician cares at least *a* little about growth).

$$\frac{dg}{d\theta} = -(1-s^*)A - \frac{A\pi'(s^*)}{\lambda\phi\alpha''(s^*) - \pi''(s^*)} < 0$$

Therefore

$$\frac{dg/d\theta}{ds^*/d\theta} = -(1-s^*)\phi\alpha''(s^*) + \frac{(1-s^*)\pi''(s^*) - \pi'(s^*)}{\lambda} < 0$$

We still get a negative relationship between s and g, even if λ is very large (that is, even if the politician cares mostly or exclusively about social welfare). In the limit, when the politician puts infinite weight on growth ($\lambda \to \infty$), this expression simplifies to

$$\lim_{\lambda\to\infty} \frac{dg/d\theta}{ds^*/d\theta} = -(1-s^*)\phi\alpha''(s^*) < 0$$

which is still unambiguously negative. The intuition is the following: when markets become more imperfect, the optimal policy response is to increase the level of intervention, but no so much as to fully insulate growth from the impact of the increased market imperfection. Consequently, higher levels of policy intervention are associated with lower growth rates, even though the politician is motivated purely by economic factors.

Therefore, we should actually expect to observe a negative relationship between policy interventions and economic performance under the null hypothesis that what governments are doing is to respond, in a welfare-maximizing way, to market imperfections. Or to put it differently, when we observe a negative correlation between interventions and performance we cannot distinguish between two diametrically opposed views of the world — one in which governments are driven by desirable economic motives and one in which they are driven by economically harmful, political motives. This result applies to all cross-sectional regressions, whether run at the level of countries, regions, or industries.

E. Cross-National Variation Generated by ϕ

We assume now it is ϕ that varies across countries. Look first at the effect on the "optimal" intervention:

$$\frac{ds^*}{d\phi} = -\frac{\lambda \alpha'(s^*)}{\lambda \phi \alpha''(s^*) - \pi''(s^*)} < 0$$

If policy is less effective, there is less intervention in equilibrium. The effect on growth is given by:

$$\frac{dg}{d\phi} = -\alpha(s^*) + g_s(s^*, \theta, \phi)\frac{ds^*}{d\phi}$$

which is of ambiguous sign since $\alpha(s^*) > 0$, $g_s(s^*, \theta, \phi) < 0$ (as long as λ is less than infinite), and $ds^*/d\phi < 0$. Intuitively, there are two offsetting effects here. The direct effect (captured by the first term) is negative, since a decrease in the effectiveness of the policy intervention reduces growth. But there is also an indirect effect that arises from the induced change in s^*. An increase in ϕ reduces s^*, which gives a positive boost to growth since s is too high from a growth maximizing standpoint in the first place. If we are in an equilibrium where political motivations leave us with too high a level of interventions, an increase in the cost

of intervention (a decrease in the ability of the government to intervene) can increase growth.

The manner in which g and s covary is given by

$$\frac{dg/d\phi}{ds^*/d\phi} = \alpha(s^*)\frac{\lambda\phi\alpha''(s^*) - \pi''(s^*)}{\lambda\alpha'(s^*)} - \frac{\pi'(s^*)}{\lambda}$$

which is of ambiguous sign for the reason described above.

F. Comparisons

Comparing the three expressions we have obtained on how g and s covary under different assumptions about how countries differ, it can be shown that:

$$\frac{dg/d\theta}{ds^*/d\theta} < \frac{dg/d\lambda}{ds^*/d\lambda} < \frac{dg/d\phi}{ds^*/d\phi}$$

Note that the cross-national variation between g and s is more negative when what varies in the background (and differentiates countries from each other) is market imperfections than when it is the honesty/corruption of governments. So we are more likely to uncover a strong negative correlation between policy intervention and performance precisely when policies across countries differ for economic (*i.e.*, good) rather than political (*i.e.*, bad) reasons.

III. Results

I summarize some of the main results of the analysis here:

- An increase in s is rarely accompanied by a rise in g. The only exception is the case where the main variation across countries is in the "ability" of governments and governments are not too "dishonest."
- This is true even when politician is a social-welfare maximizer and is not motivated at all by diversion ($\lambda \to \infty$).
- No matter how "honest" the government is and how significant the "need" for policy interventions, the cross-national relationship between s and g will be negative as long as the main source of variation across countries is the variation in θ ("need") and λ ("honesty").

Therefore, as long as policy interventions are not random and their presence responds to unobservables, regressing an economic performance variable on policy s is uninformative about the degree to which market failures exist, the extent to which policy interventions are targeted on them, the effectiveness with which government policies are deployed, or the extent to which policy interventions are used to create and divert rents for political purposes.

IV. Where Next?

Is there a way out of the conundrum? Should we give up on estimating the impact of national economic policies? We can safely presume that policy randomization across countries is an unpromising avenue to pursue: national governments are unlikely to want to be experimented upon. What about an instrumental-variable (IV) strategy? The trouble with IV is twofold. First, in this area of inquiry it is genuinely hard to find credible instruments which satisfy both the exogeneity and exclusion requirements. As Durlauf, Johnson, and Temple (2005) point out, growth theory is so broad and encompassing that it is always possible to find a story about why an exogenous variable belongs as a regressor in the second-stage of the estimation (therefore making it invalid as an instrument). Plausible instruments are very few indeed. But an equally important limitation on IV is that what we are typically interested in knowing is the impact of *purposeful* policy action. We want the answer to the question: when governments have tried to achieve this or that objective, how successful have they been at it? The exogenous component of policy, even if excludable from the second-stage, can help us answer a different question — what has been the impact of policy interventions that governments did not adopt purposefully — but it does not answer that particular question.

A first step in the right direction is to take the theories that motivate our empirical analyses more seriously. Our failure to undertake meaningful tests often derives from a failure to fully specify the theoretical model(s) being put to the test. For example, if we are testing the null hypothesis that governments are acting in the public interest, we need to specify a model in which governments do precisely that, come clean about what we assume is and is not observable, and inquire whether the empirical implications of such a model are consistent with the data. If we are testing this view against the alternative that they are motivated

by private/political interests, we need to be clear about the distinct predictions the two models make for the data. Furthermore, whether or not the alternative theories generate different predictions, we need to look for direct evidence about the channels through which policies are hypothesized to operate.[5] These seem like standard good practice, but it is clear that the bulk of the cross-national growth literature proceeds in a different manner, assuming that it is enough to plug a policy variable in a regression (while perhaps making an honest attempt at instrumenting it) in order to answer a whole series of questions about the effectiveness of policy and the motives of governments.

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[5] This is in the spirit of the approach taken in Rajan and Zingales (1998). However, note that being able to control for country and industry fixed effects, as in Rajan and Zingales, does not eliminate the central problem of policy endogeneity.

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