Does One Size Fit All In Policy Reform? Cross-National Evidence and its Implications for Latin America

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1. Introduction

In an oft-cited article published in 1990, John Williamson (1990) coined the term "Washington Consensus" to refer to the lowest common denominator of policy advice being offered by the Washington-based institutions to Latin American countries at the end of the nineties. The list summarized 10 propositions that, Williamson argued, most of official Washington (a group in which he evidently includes himself) thought would be good for Latin America.[2]

Williamson's list provided a useful starting point for discussions on the merits of alternative economic reform programs. Discussions have centered upon what should be included or excluded from this list, both as a description of what Washington thinks as well as a normative statement regarding what countries should do. Critics such as Stiglitz (1999) have argued that the evidence of the nineties reveals a failure of the Washington Consensus and advocated drawing up a new list altogether, focusing on objectives of sustainable, egalitarian and democratic development and including concrete policies such as sound financial regulation. Williamson himself (2000) has expressed doubts about the role of interest rate liberalization and argued for a much more comprehensive view of financial liberalization. Naím (2001) has gone as far as to say that disagreements are so prevalent among economists and Washington institutions over issues such as the effectiveness of open trade policies and the need for an international financial architecture that no such consensus actually exists.

There is by now an emerging consensus that the results of Washington consensus reforms have, at best, been much weaker than expected (Loayza, Fajnzylber, and Calderón, 2002, Ocampo, 2005). The failure of the region to attain solid growth outcomes in the 1990s has coincided with growing difficulties in the consolidation and institutionalization of democratic regimes in the region. The high levels of political instability and fragility of countries like Ecuador and Venezuela are the most extreme examples of the way in which economic failures go hand in hand with political failures. Understanding the reasons for the disappointing performance of countries that followed Washington consensus growth strategies is thus a fundamental ingredient of any attempt to understand how the region can retake a path of consolidation of stable democratic institutions.

Underlying much of the discussion on the Washington consensus there appears to be implicit agreement that such a list exists, in the sense that there is a set of policy prescriptions that, if applied in any Latin American country, would generate at the very least the basic conditions necessary for sustained economic growth. Williamson has captured appropriately the state of thinking about policy reforms when he stated that "in practice there would probably not have been a lot of difference if I had undertaken a similar exercise for Africa or Asia" (2000, p. 255). In other words, underlying the discussion about the Washington Consensus there appears to be underlying agreement over the fact that there is sufficient similarity between all developing economies so as to permit applying the same thought exercise to all of them when thinking about development policies.

[2] The list consisted of: fiscal discipline, a redirection of public expenditure towards field offering high economic returns and the potential to improve income distribution, tax reform, interest rate liberalization, a competitive exchange rate, trade liberalization, liberalization of inflows o foreign direct investment, privatization, deregulation, and secure property rights.

This state of thinking contrasts starkly with the view of development economics shared by the structuralist and dependency schools (Prebisch, 1962, Furtado, 1961, Myrdal 1957). These authors argued for the specificity of the experience of developing countries and explicitly opposed attempts to use the same theories to think about broadly distinct institutional and structural settings. In this view of development, the effects of economic policies depended on historical and structural forces, so that policies that worked adequately in some settings would fail to do so in others.

Although many have by now forgotten the structuralist and dependency schools, there has been a recent rebirth of sots of their basic ideas in thinking about economic policies. Critical analysis of the growth experience of the 1990s has produced numerous observations of economies that have implemented different sets of policies and have shown widely divergent growth experiences. Although some of the highest growing countries in the world during the 1990s, such as Chile and Korea, whose growth rates of per capita GDP over the 1990-03 period were respectively 3.7 and 4.7%, had relatively open free market economies, others, such as Lebanon and Lesotho (growth rates of 6.1% and 4.8% over the same period), clearly did not. According to the Heritage Foundation (2005), Botswana and Mongolia had similar levels of economic freedom in the mid-nineties; the former grew at an average rate of 2.8% during the 1990-03 period; the latter at -3.0% (Rodríguez, 2006a). Deeper analysis of development experiences has led to the identification of country cases – such as that of El Salvador (Hausmann and Rodrik, 2005) - that have done "everything right" in terms of following the Washington Consensus, yet have not seen payoffs in terms of economic growth. In a recent comprehensive appraisal of the results of a decade of economic reforms published by the World Bank, the role of interactions between policies, institutions and economic structure is not only recognized but made to play a central role. In their words:

> "To sustain growth requires key functions to be fulfilled, but there is no unique combination of policies and institutions for fulfilling them…different polices can yield the same result, and the same policy can yield different results, depending on country institutional contexts and underlying growth strategies…Countries with remarkably different policy and institutional frameworks – Bangladesh, Botswana, Chile, China, Egypt, India, Lao PDR, Mauritius, Sri Lanka, Tunisia and Vietnam – have all sustained growth in GDP per capita incomes above the U.S. long-term growth rate of close to 2 percent a year." (World Bank, 2005, p. 12)

The academic literature has also seen renewed interest in understanding why similar economic policies appear to work differently in different countries. The importance of interactions among different dimensions of potential regressors has become the focus of recent attention in the academic literature. In a recent paper, Hausmann, Rodrik and Velasco (2004) point out that the Theorem of the Second Best would lead us to expect that the reduction of a particular distortion may have very different effects on welfare (and growth) depending on the initial levels of other distortions. Their theoretical examples illustrate the potentially complex interactions that can arise even in relatively simple models. They also present a discussion of a number of cases in which similar policies appear to have had very different growth effects and suggest that they may be due to the fact that the countries faced different binding constraints on economic growth.

This new (or renewed) vision of economic growth contrasts with the methodological tools currently used by growth economists to understand the effect of policies in developing countries. The standard empirical workhorse model of economic growth (the linear regression of growth on its determinants) assumes that the effect of a change in a policy is the same in all countries regardless of their structural or institutional characteristics. These growth regressions have become an ubiquitous form of policy analysis. Empirical work in this literature is often geared towards reaching (or rebutting) a conclusion that a certain variable of interest – say a particular economic policy or one of a variety of institutional arrangements – is harmful or beneficial for growth. It is not uncommon for research in this area to conclude with phrases such as "We find clear evidence that the institution and policy variables play a significant role in determining economic growth."[3] Even the widespread practice of inspection of partial scatter plots and correlations between growth and policies is, in essence, the use of a growth regression framework.

Such a vision rules out the existence of strong interactions between policies, institutions and economic structure. When such interactions are considered in the literature, the common approach is to explore non-linearities with respect to the variable of interest, assuming linearity in the remaining regressors (Barro, 1996, Banerjee and Duflo, 2003). A number of authors have introduced elements of non-parametric estimation to consider more general non-linearities (Liu and Stengos, 1999, Kalatzidakis et. al., 2001). However, the approach is usually concentrated on understanding the effects of non-linearity in a particular dimension rather than studying the implications of a more general breakdown of the assumption. An alternative approach has been to study models of parameter heterogeneity (Durlauf and Johnson, 1995, Durlauf, Kourtellos and Minkin, 2001) in which countries are characterized by different linear models.[4] Despite these explorations, the standard workhorse regression model is still that of the linear regression framework. For example, in Sala-i-Martin, Doppelhoffer and Miller's (2004) recent Bayesian exploration of robustness issues, all of the approximately 89 million regressions studied are linear.

The present paper steps into this debate by asking how we should use the cross-country data to carry out policy inferences when these non-linearities are important. We will make three basic points: (i) that the empirical estimates of growth effects of policies are severely biased if non-linearities are ignored (ii) that there is strong evidence that these non-linearities are important in the data (iii) that appropriate non-parametric tests on the cross-country data are generally inconclusive with respect to the effect of Washington-Consensus policies on economic growth.

These conclusions are highly relevant for policy analysis. They imply that development thinking should be specific to a country's institutional and structural characteristics and that thinking about a "list" of policy prescriptions to apply to a broad group of developing economies is methodologically erroneous. They also suggest the need to rely on local, case-specific knowledge for designing growth strategies. One size

[3] This particular phrase is taken from DeGregorio and Lee (2004).

[4] Parameter heterogeneity is often confused with non-linearity, but this is incorrect: parameter heterogeneity assumes that countries are characterized by different models, which happen to share the same functional form, while the assumption of non-linearity refers to countries sharing a common model which is different from the linear one.

does not fit all in terms of policy reform, and not recognizing this is likely to lead to frequent missteps in the search for economic growth

The rest of the paper proceeds as follows. Section 2 takes a first look at the data from Latin America as well as from a broader cross-section of countries and argues that there exist substantial differences in growth performances between countries that implemented similar policies. Section 3 lays the theoretical groundwork, discussing the theoretical underpinnings of the linear growth regression and the econometric effects of failure of these assumptions. Section 4 then shifts to empirical analysis, presenting the results of our tests of linearity, separability, and monotonicity. Section 5 presents some final reflections.

2. Does One Size Fit All? A First Look at the Data.

In many dimensions, Ecuador and Peru look remarkably alike. Both countries produce roughly one tenth of their value added in agriculture, three-tenths in industry, and six tenths in services. Both countries have a savings rate near 20%, and about two-thirds of their populations live in cities. Their debt service amounts to approximately one-fourth of their exports, and they devote roughly 10% of GDP to government consumption. During the nineties, both countries made ultimately successful efforts to stabilize their inflation rates: Perú through a monetary adjustment program in the early nineties, Ecuadros through its dollarization. Both countries belong to the Andean Community and have similar trade policies: their average tariff rate weighted by imports is approximately 11%. They are heavily dependent on natural resource exports, with fuel and mining exports making up four-tenths of their total exports. They have moderate current account deficits roughly under 2% of GDP.

Perú and Ecuador, however, are not similar in their economic performance. Since 1990, Perú has experienced a moderately high growth rate of 1.9% in its per capita GDP. Ecuador, in contrast, has stagnated and experienced a negative growth rate of -0.13% of GDP.

The comparison between Peru and Ecuador highlights an interesting pattern about economic growth both within the Latin America and Caribbean region and among developing countries: a very broad dispersion among the economic performances of countries that carried out similar economic policies, as well as broad variation in the economic policies that led to high levels of economic growth. These points are illustrated with the simple comparisons set out in Tables 2 and 3, which show the growth behavior and tariff rates for two sets of Latin American countries. While tariff rates are just one indicator of policy, and countries can differ substantially in other policy dimensions, they provide an useful starting point as they tend to be a reasonable indicator of how much governments are wiling to intervene in their economies. Certainly, countries that differ in their trade policy would appear to be more likely to differ in other policy dimensions. Table 2 shows the trade policies and growth rates of the eight economies in the region that had an annual per capita growth rate in excess of 2%. What is striking is that there is such dispersion in the economic policies that were compatible with high growth. For example, the three Caribbean island economies in this group, St. Kitts & Nevis, Trinidad & Tobago and Grenada all achieved per capita growth rates in excess of 2%, but ranged from having tariff rates of 4.5% in the case of

Trinidad & Tobago, substantially below the region (and the world) average, to 14.8% in the case of St. Kitts & Nevis, more than twice the region average.

Table 3 makes this point in another way: countries that shared the same policy strategy (in the dimension of trade policy) experienced very different growth performances, ranging from Paraguay's -0.5% average annual growth rate to Trinidad and Tobago's 2.9% annual rate. It appears that having similar policies is not a precondition for having similar growth experiences.

The comparisons that I have just presented are obviously extremely simplified. There are many policies other than trade policies. There are also structural and institutional factors that can account for differences in growth. One would expect that, after controlling for these factors, one would find that countries that adopt similar policies have similar growth performances.

In order to take these factors into account, Figures 1-2 and Table 4 display the results of a more complex exercise. In it I attempt to measure how similar countries are in terms of economic policies through the use of four indicators that are broadly available and that thus allow us to carry out cross-country comparisons using a large number of countries. These are the log of the black market premium (*bmp*), the log of 1 plus the inflation rate (*inf*), the ratio of government consumption to GDP (*govc*), and the average tariff rate (*tar*). We are interested in testing the hypothesis that countries with similar policies have similar growth outcomes. We can get at this issue by building all pairwise comparisons of countries and looking at how they differ in policies and how they differ in economic growth. More concretely, I estimate the relationship:

$$\left\|\gamma_i - \gamma_j\right\| = f\left(\left\|p_i - p_j\right\|\right) + \varepsilon_{ij} \qquad (1)$$

Where γ_i is country *i*'s growth rate, p_i =(*bmp,inf,govc,tar*) is our vector of policies and ε_{ij} is a random error term. We expect *f(.)* to be a monotonically increasing function. In other words, if two countries have very different policies then we would expect them to have different growth rates, but if they have similar policies we would expect them to have similar growth rates. Equation (1) thus provides a first way to test whether one size fits all in terms of policy formulation. It tests whether all countries can be seen as operating within the same model, so that the effects of policies are broadly similar for all of them.

Table 4 shows a first look at this data. In it we report the differences in the mean growth rates between pairs of countries split by two groups: those that have similar policies and those that have different policies. Our criteria for classifying two countries as similar is that the Euclidean distance between their policy vectors be less than the median of the sample. We present for the whole world sample as well as restricting to the Latin American countries. We also present a specification where we use the residuals of growth on a previous regression on initial GDP, total years of schooling, the rule of law, and the growth rate of population. From a statistical point of view, the results appear to confirm the idea that similar countries have similar growth rates: differences in growth rates are indeed systematically higher in all cases, and three out of four of them are statistically significant. But from an economic point of view, what is striking is how small the differences in growth rates are. They imply that changing from having very similar policies to having very different policies will cause a difference of between .16% and .45% in the average growth rate.

Figures 1 and 2 show the scatter plots of the relationship between the absolute differences in growth rates and the absolute distance between policy vectors for Latin America. In what will serve as a useful introduction to some of the discussion about formal treatment of nonlinearities that we take on below, we present both a linear estimate of the relationship (Figure 1) and a non-parametric estimate that can flexibly accommodate any functional form (Figure 2). In the next section we will explain the reasons why the nonparametric estimate is strictly preferable to the linear estimate when the functional form is unknown. Note that the nonparametric estimate in Figure 2 is strictly decreasing for low values of the distance between policies $\left\|p_i - p_j\right\|$, flat for intermediate values, and increasing only for very high values. In other words, there are large numbers of countries for which similar policies are not associated with similar economic performances. Note also that the difference between the linear estimate and the nonparametric estimate is striking. If we were to look only at the linear estimate, we would conclude that countries that are farther apart in terms of policies have dissimilar economic performances. The nonparametric estimate shows that this conclusion is highly misguided for most countries in the sample.

Figures 3 and 4 repeat this exercise after having purged the differences in growth rates from the effect of other potential structural determinants of economic growth. The dependent variable here is the residual of a regression of the growth rate on the log of initial GDP, total years of schooling, a rule of law indicator, and population growth rates. Note that now both the linear and nonlinear estimate are generally decreasing. It thus appears that, within Latin America, countries that follow similar policies don't have similar economic performances, even after controlling for some of their structural characteristics.

The inspection of Figures 1-4 also show that there exists broad variation in growth performances for countries with similar policies. Many pairs of countries appear to have similar policies and substantial differences in their growth rates, while there is also a substantial number of countries that have similar growth performances despite having similar policies.

There are a number of possible explanations for these results. In the first place, it could be that policies have none or little effect on growth. If policies were irrelevant for growth, then the slope of the function in (1) would be zero and one would expect to see no clear pattern arising from fitting such an equation. Granted that the slope of the estimated functions is not zero, but it is small when measured according to its economic significance. Another possibility is that the pattern of use of different policies served to systematically offset their differences. For example, if policy A is good for growth but policy B is bad for growth, then a country with high A and high B would have a similar growth performance to one with low A and low B, despite having very different policies. The problem with that argument is that it would require a special configuration of policy patterns, in which there are few countries with low A high B or high A and low B in the sample.

A third explanation is that this pattern could be due to non-linearities in the growth relationship. There are two ways in which a non-linear growth function could explain the behavior of the data. On the one hand, a non-linear function could imply that growth is a non-monotonic function of policies, so that different values of policies are compatible with the same growth outcome (Figure 5). Alternatively, non-linearities could imply the existence of relevant interactions, so that the marginal effect of policies on growth may depend on the values of other variables (Figure 6).

In the rest of the paper I will explore the case for non-linearities. I will show that the theoretical case for linearity of the growth function is tenuous and will argue that ignoring it can have damaging consequences. I then go on to show that the cross-country evidence shows strong evidence of non-linearities in the growth function.

3. Theoretical Framework

3.1. Is there a theoretical basis for the Linear Growth Regression?

In this section I discuss the theoretical basis for the linear growth regression. This regression, often referred to as a "Barro" regression because of the deep influence of Robert Barro's 1991 *Quarterly Journal of Economics* article, was proposed almost simultaneously by several other authors including Mankiw, Romer and Weil (1992) and Sala-i-Martin (1991). It consists of a growth regression that is linear in the log of initial GDP, some measures of investment or the stock of human capital, population growth and a set of "production function shifters" that commonly includes policy, institutional and structural controls. Formally, the specification often looks like:

$$\gamma_Y = \alpha_0 + \alpha_1 \ln y_{t-1} + \alpha_2 s_k + \alpha_3 H + \alpha_4 n + \beta Z \tag{2}$$

where γ_Y is the rate of per capita GDP growth, y_{t-1} is initial GDP, s_k refers to the savings rate , H is the stock of human capital, n is the rate of population growth and $\boldsymbol{Z}$ is a vector of potential production function shifters.

Given the ease of running this regression with readily available data sets and the obvious interest of exploring whether a particular set of policies, institutions or structural variables are harmful or beneficial for growth, the proliferation of applied work using equation (2) is not surprising. For obvious reasons, I will not discuss this voluminous literature here; the reader is referred to Aghion and Howitt (1999) and Temple (1999) as well as the articles in the recent *Handbook of Economic Growth* (Aghion and Durlauf, 2004) for exhaustive surveys. It suffices to note for our purposes that this analysis tends to take the form of varying the subset of variables included in Z and using conventional significance tests to evaluate the effect of potential determinants on economic growth.

Equation (2) is not a purely *ad-hoc* specification. Its analytical foundations were elaborated early on in the literature and, to my knowledge, were first presented systematically in Mankiw, Romer and Weil's (1991) augmented Solow model. These authors started out from a simple aggregate economy production function in which output Y_t was a multiplicative function of human and physical capital H_t and K_t as well as productivity A_t with constant exponents on the first two:

$$Y_t = A_t K^{\alpha}{}_t H^{\beta}{}_t \,. \tag{3}$$

This is the functional form known as the Cobb-Douglas production function because of the empirical estimation of it carried out by Charles Cobb and Paul Douglas

(1928).[5] It is useful to stop a minute to consider the implications of this equation. Production functions are, of course, a common staple of economics. At the firm level they can be thought of as a technological relationship between physical and human capital inputs on the one hand and output on the other hand. Even at the firm level, though, a production function is much more than a mathematical description of the production technology. A firm can derive less outputs, given a certain quantity of inputs, for a plethora of reasons ranging from high levels of worker conflict, low employee morale, poor management, lack of effective planning, and supply disruptions. All of these factors will be subsumed in the "technology" term A_t. Some of these reasons are so clearly *not* technological that many prefer economists prefer to use the more general and inclusive term "productivity."

What is true at the firm level is even truer at the aggregate economy-wide level. The reasons why a society may be able to produce much less output per worker than other societies, given the amount of inputs that it utilizes, can range from the quality of its institutions and its dependence on volatile primary goods industries to the stability of its macroeconomic policies, to name just a few. Indeed, many economists believe that A_t – and not K_t or H_t -is the fundamental source of cross-national differences in output per worker (Hall and Jones, 1999, Parente and Prescott, 2000).

The second set of assumptions of the Mankiw-Romer-Weil setup simply describe the evolution of the stocks of human and physical capital by the accumulation equations:

$$\frac{dK_t}{dt} = s_k Y_t - \delta K_t \tag{4}$$

$$\frac{dH_t}{dt} = s_h Y_t - \delta H_t \tag{5}$$

where s_k and s_h respectively denote the fractions of income devoted to savings in the form of physical and human capital and δ is the common depreciation rate for physical and human capital. In other words, both human and physical capital grow on net by the difference between the resources devoted to their accumulation and their depreciation. What Mankiw, Romer and Weil showed is that out of this extremely simple set up you could derive the following equation to describe the growth rate of the economy near its long-run equilibrium[6]:

$$\gamma_t = \frac{d\left(Y_t / L_t\right)}{Y_t / L_t} = \beta_0 + \beta_1 \ln\frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln\frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \beta_6 \ln A_0 + \lambda_{it}. \tag{6}$$

Where γ_t denotes the proportional growth rate of per capita GDP. The latter is defined simply as the ratio between output Y_t and the labor force L_t. n is the growth rate

[5] Although equation (3) may appear to be a somewhat arbitrary specification, many economists have thought that it is necessary to explain long-run trends in a number of macroeconomic aggregates, such as the apparent constancy over time and across countries of capital shares of GDP (Gollin, 2002). This evidence has recently been questioned by Ortega and Rodríguez (2006).

[6] In order to derive this equation, Mankiw, Romer and Weil approximate the differential equation for growth near the steady state (or long-run equilibrium). An implication of this is that another reason for the possible failure of this specification is that countries are far from their steady states. Dowrick (2004) analyzes this subject in detail.

of population $\frac{1}{L}\frac{dL_t}{dt}$ and g is the growth rate of technology $\frac{1}{A}\frac{dA_t}{dt}$. Recall that A_t denotes the level of productivity, so that A_o would be the level of productivity at the initial time period. λ_i is a country specific random effect.

There is a key additional assumption that Mankiw, Romer and Weil (1992) make that allows them to turn (6) into an equation that can be estimated econometrically. That is to assume that cross-national differences in the initial level of technology vary randomly according to:

$$\ln(A_0) = \ln(A) + \varepsilon_i \qquad (7)$$

with ε_i representing a country-specific random term. This is evidently an extremely simplifying assumption. Obviously we would expect cross-national differences in productivity to depend on cross-national differences in institutions, policies and economic structure – precisely the terms that growth empirics often studies and that we have grouped under the vector **Z**. We will return to this briefly. Substituting (7) in (6) gives us the linear regression:

$$\gamma_t = B_0 + \beta_1 \ln\frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln\frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \eta_{it}. \qquad (8)$$

where $B_0 = \beta_0 + \beta_6 \ln(A)$, which can be treated as a constant term, and $\eta_{it} = \lambda_{it} + \beta_6 \varepsilon_i$, which can be dealt with as a compound disturbance term. In other words, equation (8) can be estimated as a simple linear regression.[7] As long as we can ensure that η_{it} is uncorrelated with the rest of the independent variables in equation (8), it can be estimated by ordinary least squares; if we cannot make that assumption about the distribution of η_{it}, other methods such as instrumental variables techniques may be adequate.

Equation (8) gives us a well-defined theoretical prediction linking economic growth to specific observables – namely initial income, the savings rate, the stock of human capital per worker, and population growth. Indeed, Mankiw, Romer and Weil center on estimating precisely this equation. As we have seen, however, this approach sweeps under the rug one of the most interesting potential sources of variation in per capita income and growth: the effect that changes in variables like institutions, economic structure and economic policies may have on economic growth trough the aggregate productivity term A_t.

Equation (8) is not, however, the equation used by most researchers. The bulk of the literature on cross-national growth empirics uses some version of equation (2). There are two distinctions between (8) and (2). One is that (2) uses n as a control instead of $ln(n+g+\delta)$. This flaw can be – and often is – easily corrected.[8] The other, more serious problem, is that (8) does not (yet) depend on **Z**, the vector of policies,

[7] An additional important assumption

[8] Since g and δ are by assumption constant in this model it is simply a matter of estimating these and calculating $ln(n+g+\delta)$ as a control. We do not emphasize this flaw because, even though it is common, it is not difficult to handle and in practice many researchers do use the correct functional form.

institutions and structural variables that may affect the economy's aggregate productivity.

There are at least two important reasons to take **Z** into account when estimating a regression like (8). One is econometric. If differences across countries in the determinants of productivity are not simply randomly distributed, but rather ε_i is correlated with any of the regressors in (8), its omission would lead to omitted variable bias in our coefficient estimates of that regression. The other reason is more basic. Potentially the most important reason why we are interested in the empirical study of economic growth is because we care about how policy decisions, institutional reforms and structural economic changes can affect long-run growth, all of which we have characterized as belonging in **Z**. The effect that theses variables in **Z** may have on economic growth is potentially much more relevant than that of initial income, savings rates or population growth, all of which may be very difficult to change.

How do we go about putting variables like policies, institutions and struture in this framework? The most logical way would be by seeing their effect as affecting the efficiency with which society converts inputs like human and physical capital into outputs, that is, as affecting our productivity term A_t. That is precisely why a broad – as opposed to a strictly technological – concept of productivity is necessary to think about A_t. In that case equation (7) would need to be replaced with:

$$\ln(A_t) = h(\mathbf{Z}_t) + \varepsilon_{it}. \qquad (9)$$

which leads us to derive the following estimation form in place of equation (8):

$$\gamma_t = \beta_0 + \beta_1 \ln\frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln\frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \beta_6 h(\mathbf{Z}) + \eta_{it}. \qquad (10)$$

What we wish to point to is that there are two key characteristics of (10) in which it is distinguished from (8). One is that it is an inherently non-linear function of **Z**, as can be clearly seen from the non-linear term *h(**Z**)*. The second one is that even if we assume that the log of productivity is a linear function of **Z**, say:

$$\ln(A_t) = \sum_i \gamma_i Z_i \qquad (11)$$

, then equation (10) becomes:

$$\gamma_t = \beta_0 + \beta_1 \ln\frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln\frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \sum_i \beta_{6i} Z_i + \eta_{it}, \qquad (12)$$

where we have denoted $\beta_{6i} = \beta_6 \gamma_i$. This is **still** a non-linear function of **Z** since the growth rate of technology g that forms part of the nonlinear term $\ln(n+g+\delta)$ is:

$$g = \frac{1}{A}\frac{dA_t}{dt} = \sum_i \gamma \frac{dZ_i}{dt}. \qquad (13)$$

In other words, there is what we can call an **inescapable nonlinearity** in the growth function. Even if we assume that the effect of policies, institutions and structure on productivity is linear, it will still be the case that growth will be a non-linear function of these variables because it depends on them both through their direct effect through productivity and through their indirect effect that works through the $\ln(n+g+\delta)$ term.

An alternative way of putting this is by thinking about the conditions that would be necessary for growth to be a linear function of the variables in **Z**. Since $\ln(n+g+\delta)$ is a nonlinear function of **Z**, the only way for (12) to become linear is for g to be the same for all countries. In other words, we do not only need variables like institutions, structure and policies to affect productivity linearly, we also need them to do it in such a way that the growth rate of productivity will be the same in all countries. This will be possible only in very peculiar cases (e.g.: if all countries in the world have the same exact pace of economic reform).

While some of the assumptions of the Mankiw, Romer and Weil model have received considerable attention, the inescapable nonlinearity in production function shifters has been almost completely ignored in the applied literature.[9] One possible line of defense, taken by Mankiw, Romer and Weil, is to see g as capturing only the effects of technological change, which is assumed to be public and available to all countries, while $A(Z_0)$ is held to be fixed at its initial level. Research exploring the failure of this hypothesis often looks at varying rates of diffusion of technologies across countries (Coe and Helpman, 1995, Coe, Helpman and Hoffmaister 1997). This leaves unanswered the questions raised by the terms of Z that have no relation to technological diffusion. While the assumption that they are time-invariant may be adequate for thinking about some production function shifters such as economic geography and perhaps institutions, it is much less useful if one wants to understand the effect of variables like economic policies, institutional reform or structural change.

This paper will concentrate on empirically analyzing the validity of equation (11). I know of no systematic treatment of the effects of failure of the assumption that technology is a linear function of its determinants embodied in this equation. This is surprising, given that, unlike the other assumptions of the Mankiw-Romer-Weil model, this assumption is almost completely atheoretical. There is no reason why one would expect variables as diverse as economic policies, institutions and structural characteristics to have separable, linear effects on the log of the production function. Indeed, to the extent that one sees the "production-shifting" effect of the Z variables on the production function as reflecting the efficiency effects of relaxing different distortions, basic economic theory as captured by the Theorem of the Second-Best tells us that there is no reason to expect that relaxing one distortion would lead to an increase in efficiency when another distortion is present; in other words, it tells us that the effects of distortions on efficiency are unlikely to be separable.

Economics is, of course, full of simplifying assumptions, and there is by now a broad methodological consensus – at least among neoclassical economists - on some version of Milton Friedman's (1953) methodological postulate that an assumption should not be judged by its realism or lack of it but rather by its capacity to help explain reality. It is thus possible to develop a line of defense of the linearity hypothesis using this argument if we could show that the cross-country data is consistent with the predictions that emerge out of a model characterized by a linear growth function. In section 4 we turn to the discussion of how well the linearity hypothesis fares when posed against the data. Before that, however, it is useful to turn to an analysis of the consequences of erroneously assuming that the growth function is linear when it is not.

3.2. The Econometric Effects of Throwing In the Kitchen Sink

[9] Durlauf, Johnson and Temple's recent (2004) comprehensive survey of the empirical growth literature concurs with this assertion: "As far as we know, empirical work universally ignores the fact that $\log(n_i+g+\delta)$ should also be replaced with $\log(n_i+g_i+\delta)$" (2005, p. 580)

3.2.1. The Effects of Misspecification Bias

If our key contention is correct, then empirical work on economic growth has consistently attempted estimating a non-linear relationship through the use of linear methods. In slightly more technical jargon, they have estimated misspecified models. What type of problems arise from doing this?

The key problem arising from estimation of a misspecified regression is that it is unclear that the resulting coefficients can be given any meaningful interpretation. The reason is that estimating a non-linear regression through linear techniques is the same thing as omitting the non-linear term from the regression, and thus generates a bias which is formally identical to omitted variable bias.

In order to fix ideas, let us think about a simple univariate non-linear function:

$$y = f(x) + \varepsilon_i \tag{14}$$

It is straightforward to rewrite this equation in order to split *f(x)* into its linear and non-linear components:

$$y = Ax + h(x) + \varepsilon_i \tag{15}$$

where $h(x) = f(x) - Ax$. Suppose then that we estimate the linear regression:

$$y = \alpha x + \eta_i \tag{16}$$

Estimating (16) when (15) is true is the same thing as throwing *h(x)* in the disturbance term, that is, omitting it from the regression. Recall that omitted variable bias affects coefficient estimates of the included variables whenever the is a correlation between the disturbance term and the right-hand side variables in the regression. In most cases it is very hard to know whether omitted variable bias is a serious problem unless we know whether the right-hand side variables in our regression are correlated with the excluded, often unobservable variables. In this case, in contrast, *h(x)* is by definition a function of our right-hand side variable *x*, and thus will generally be correlated with it.[10]

The crux of this is that $\hat{\alpha}$ will generally be a biased estimate of *A*. It is impossible to predict the sign of this bias unless we know the sign of the correlation of *h(x)* and *x*. We will generally not know this unless we know the functional form of *h(.)*, which, of course, we don't. Thus $\hat{\alpha}$ will not be an adequate estimator of the linear component of *f(x)*.

Is there a meaningful interpretation to the linear estimator? Some authors (see, e.g., Helpman, 2004, p. 73) have suggested that the linear estimator of a growth regression gives us the average effect of changing the explanatory variable over the sample of countries. If this were true, it would imply that the linear estimator may not be a poor guide to evaluating the expected effects of changes in policies or institutional and structural reforms: even if we cannot recover the expected effect of these changes for a given country, we may still be able to inform the policymaker of the expected effect of making such a change over all countries.

Regrettably, this interpretation is not correct. In Rodríguez(2006b), I establish the necessary conditions for a linear estimator to give us an unbiased estimate of the average partial derivative of a non-linear function. The conditions are highly restrictive,

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[10] One can come up with examples of non-linear functions that are not correlated with linear functions of their arguments, but this will only be true in very special cases.

and require, among other things, that the explanatory variables be distributed symmetrically. Anyone familiar with the cross-national data on institutions and policies will know that it is commonly characterized by large numbers of outliers characteristic of asymmetric distributions.[11]

Much of the recent literature in empirical growth analysis has been concerned with finding solutions for another type of misspecification problem: that of endogeneity or reverse causation. The state of the art for tackling endogeneity problems is the use of instrumental variables estimators. The basic intuition behind these is simple. Suppose that we are worried that reverse causation is contaminating the estimate of our variable of interest on growth. For concreteness, suppose we are attempting to estimate the effect of institutions on growth but think that part of the positive correlation displayed by the data comes form the fact that richer countries tend to develop better institutions. A simple solution would be to find a subset of events in which institutions changed for reasons that had nothing to do with growth. In a statisticians' ideal world, we would have controlled experiments in which we could be sure that institutions had varied randomly, much as explanatory variables change in real laboratory settings. It is not clear that such an experiment is feasible nor desirable for anyone except those completely obsessed with growth econometrics. However, there may be cases in which history or nature is able to give us this type of exogenous variation, so that there is a source of change in our variable of interest which is so clearly exogenous to the process under consideration that if we do observe that it is associated with changes in growth, those cannot reflect a process of reverse causation.

Some of the most relevant recent contributions in growth empirics are indeed ingenious applications of instrumental variables techniques to the study of the determinants of growth (see, for example, Frankel and Romer (2000) or Acemoglu, Johnson and Robinson (2001)). Regrettably, instrumental variables estimators are ill-equipped to handle the issue of non-linearities. The reason is that in order for the estimates derived from the use of instrumental variables to be unbiased, three key conditions have to be satisfied: (i) the instrument has to be exogenous, (ii) it has to be correlated with the potentially endogenous variable, and (iii) it has to be uncorrelated with the error term in the equation of interest. When there is an omitted nonlinearity, conditions (ii) and (iii) cannot be simultaneously satisfied: if an instrument is correlated with the variable x in equation (16), it will also be correlated with the nonlinear term $h(x)$ which is treated as part of the error term.

3.2.2. Non-linearities and the Curse of Dimensionality

What then is the appropriate method to estimate an inherently nonlinear function like (14)? The answer to this question depends on whether we believe that we know enough about the functional form of this equation so as to estimate it parametrically or non-parametrically. If we were to have sufficient information about the functional form of the growth function so as to be able to postulate a particular functional form, then the

[11] Easterly(2006) has indeed argued that most of the significant effects that the literature has found of policies on growth are caused precisely by these outliers,

adequate approach would be to estimate such functional form through a nonlinear technique such as nonlinear least squares.

Regrettably, it is hard to argue that we have such knowledge. Indeed, the prevalence of the use of the linearity assumption is in itself a manifestation of the lack of generally accepted theories about the functional form that the effect of the production function shifters that we have grouped in **Z** has on productivity. Therefore it seems appropriate to use methods that do not make *a priori* assumptions about functional forms. Non-parametric methods would appear to be appropriate in this case.

Regardless of whether we use parametric or non-parametric methods, estimating a non-linear function is always much more demanding in terms of data than estimating a linear function, because we must sample it at many more points to be certain of its shape. In the parametric case, if there were no sampling error, we would need only two observations to fit a linear function $y = \beta_0 + \beta_1 x$ to the data. However, if the function is a non-linear function of the form $f(x, \beta)$ with β a k-dimensional vector, then even in the absence of sampling error we will need at least *k* points to infer β.

In the non-parametric case this problem is exacerbated. If the function is unknown, then we will need much more information to sample it at many points if we want to keep the approximation error within reasonable bounds. Even more important is the fact that this problem becomes much greater when the dimensionality of the function grows. As shown by Yatchew (2004), if we sample a function at *n* equidistant points then the magnitude of the error of approximation will be proportional to the n-th root of the number of observations. In other words, having 100 observations to estimate a one-dimensional relationship is tantamount to having 10 observations ($100^{1/2}$) to estimate a two-dimensional specification and to having 4.64 observations ($100^{1/3}$) to estimate a three-dimensional specification. To be consistent, a researcher should place the same faith on a regression estimate of a general non-linear function in three dimensions that is run with 100 observations than she should put on a correctly specified linear regression that was run with 5 observations. This result is known as the *curse of dimensionality* in the literature on non-parametric econometrics and it underlines the difficulty in making appropriate inferences about unknown non-linear functions with few observations.[12]

One implication of the curse of dimensionality is that the confidence that we may have in the inferences that can be derived from non-parametric estimation will depend not so much on whether the function is non-linear but on whether it is separable or not. In other words, what is key is our ability to write:

$$y = f(x_1, \ldots x_n) = f_1(x_1) + \ldots + f_n(x_n) \qquad (17)$$

In which case we say that *f(.)* is additively separable. If this is the case, then the curse of dimensionality is not a problem because each of the subfunctions $f_i(.)$ varies along just one dimension.[13] Whether the curse becomes an obstacle to estimation will thus depend on whether we expect the effects of production-function shifters such as institutions, policies and economic structure to be independent of each other. To use a simple example, consider the effect of trade policy on growth. If we think that an

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[12] The result can be slightly attenuated if the optimal non-parametric estimator (which is not always easy to find) is used (Stone, 1980), but is still a significant impediment to making inferences with small data sets.

[13] This result was first established by Hastie and Tibshirani (1990).

increase of a certain magnitude – say 10 percentage points - in tariffs has the same marginal effect on growth regardless of the initial level of tariffs then we believe that the relationship between tariffs and growth is linear. If we believe that this effect varies with the initial level of tariffs – such that, for example, they are much larger for an increase from 0 to 10% than from 90 to 100%, then we say that it is non-linear. If we believe that it depends on the value of another variable – such that the effect of increasing tariffs will not be the same in a country with solid institutions than in one with rampant corruption – then we say that it is not separable. Estimation of the growth function through non-parametric techniques may be feasible if the curse of dimensionality is held in check by separability.

4. Empirical Evidence

I have until now argued that the theoretical basis for the standard linear "kitchen sink" regression commonly used in cross-national growth analysis is tenuous. Contrary to common opinion, this form does not naturally emerge from a simplified version of the Solow model, but is actually extremely hard to derive without very stringent assumptions that are themselves at odds with the type of variations that we are commonly interested in studying for the purposes of policy analysis. I have also argued that the real risk to the cross-country empirical project comes not only from failure of linearity but from the potential failure of separability: if the effects of potential production function shifters such as policies, institutions and economic structure are not separable from each other, then the amount of information often found in cross-national data sets may be insufficient to estimate the growth function with any degree of confidence.

Whether nonlinearities or non-separabilities are an empirical characteristic of the growth data is a question whose answer ultimately lies with the data itself. In this section I will present a brief summary of the empirical evidence regarding the issue of generalized non-linearities and non-separabilities in the growth data. We will concentrate on testing the assumption that production function shifters have a linear effect on productivity as captured by equation (11). As we have argued above, (11) is a necessary condition for linearity of the growth function. If we were to find evidence invalidating it, our results would shed doubts on the validity of the whole linear approach.[14]

The bulk of the tests discussed in what follows rely on non-parametric or semiparametric methods of estimation. The basic intuition behind these methods is that they do not make strong assumptions about the functional form underlying the estimated function but rather maintain sufficient flexibility so as to estimate any functional form. As explained above, this is the correct approach to take unless we are willing to make strong *a priori* assumptions about the underlying functional form of the growth function.

For the analysis that follows, I will use a cross-sectional data set of economy-wide measures of growth and its potential determinants for the 1975-00 period. The cross-sectional approach is the hallmark of the empirical growth literature and has dominated

[14] Recall that constancy of g is also necessary for linearity. Since failure of (11) is sufficient to invalidate linearity, we will not deal with this other assumption in detail. Were we to find confirmatory evidence of linearity – which we don't – it would also become necessary to evaluate this additional assumption.

cross-country growth analysis since the early nineties. Despite the recent interest in the use of panel data techniques, the cross-sectional approach is still broadly used and characterizes some of the most relevant recent contributions.[15] Furthermore, relevant methodological questions remain about the applicability of the panel data approach to study questions of long-term economic growth. For example, it is not clear that segmenting the data into ten or five-year intervals is appropriate when the phenomenon of interest is long-run growth, and most methods used require the introduction of fixed effects, impeding the analysis of the effect of potential growth determinants, such as institutions or geography, which exhibit little or no variation over time.[16] A natural extension of the work considered here, however, would explore its application in a panel data setting.

I use the Penn World Tables PPP-adjusted per capita GDP Growth Rates from Summers, Heston and Aten (2000) for the 1975-00 period as the dependent variables As right-hand side indicators of the potential production-function shifters $\boldsymbol{Z}$, I use twelve variables commonly used in empirical growth analysis as well as three summary indicators made up of subgroups of these. The sample attempts to cover the three key dimensions that have played relevant roles in the analysis of growth empirics: policies, institutions and economic structure. To measure policy distortions, I use government consumption as a percent of GDP, the average tax on imports and exports, the log of one plus the inflation rate and the log of the black market premium. To capture the role of institutions, I introduce four commonly used indicators: a measure of the rule of law, a measure of political instability, an index of economic freedom, and an index of the effectiveness of government spending. In the list of structural measures of the level of social development and economic modernization of nations, I use the share of primary exports in GDP in 1975, the rate of urbanization, the ratio of liquid liabilities to GDP, and the average years of life expectancy. I also use three summary indicators of each of these three dimensions, made up by simple normalized averages of the relevant indicators. A full description of the variables is provided in Table 5.

Estimation starts out from the semi-linear growth equation in (10):

$$\gamma_t = \beta_0 + \beta_1 \ln \frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln \frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \beta_6 h(\mathbf{Z}) + \eta_{it}. \qquad (10)$$

Our basic idea is to test the model embodied in equation (10) against two alternative models. One is the fully linear model of equation (12):

$$\gamma_t = \beta_0 + \beta_1 \ln \frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln \frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \sum_i \beta_{6i} Z_i + \eta_{it}. \qquad (12)$$

The second one is an intermediate additively separable model in which the effect of each of the production function shifters, while non-linear, is independent from each other. As noted above, this specification, were it to be valid, could considerably attenuate the effects of the curse of dimensionality:

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[15] Some examples are Frankel and Romer (1999), Acemoglu, Johnson and Robinson (2000), and Sala-i-Martin, Doppelhoffer and Miller (2004). The first two articles use a levels specification, whereas the third uses the growth specification that we reproduce here. For a recent critique of the levels approach, see Sachs (2005).

[16] Standard random effects estimators require the random effect to be uncorrelated with the residual, which is by construction not the case in a growth regression. See Durlauf, Johnson and Temple (2006) for a discussion.

$$\gamma_t = \beta_0 + \beta_1 \ln \frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln \frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \sum_i f_i(Z_i) + \eta_{it}. \qquad (18)$$

4.1 Linearity and Separability

The basic results of these tests are presented in Table 6.[17] We start out by evaluating the linearity hypothesis – that is, by testing (12) against (10). The linearity hypothesis is simple to test because its validity can be evaluated completely within a parametric framework. The reason is that, if the linear model is correct, then introducing nonlinear terms in the regression should not add to the explanatory capacity of the regression. Therefore one simple place to start the test of the linear model is by evaluating whether the added nonlinear terms corresponding to a Taylor expansion are significant once added to the linear specification.

Column 1 of Table 6 reports the results of these linearity tests. In order to make sure that our results are not driven by the specific choice of explanatory variables, we reran each specification using all possible combinations of our indicators of institutions, policies, and economic structure in which one variable from each group was included. This gives 125 (5^3) specifications. Table 6 reports the median F-statistic and the median p-value for the test of the null hypothesis of linearity. It also reports the number of regressions in which the test rejected the null hypothesis at a 5% level of significance. We see that in 114 of the 125 specifications (91.2%) the linearity hypothesis was rejected. This very high rejection rate indicates that the failure of linearity is not due to a specific variable or interaction but is rather a generalized phenomenon that turns up in many alternative specifications that use different indicators.

The decisiveness of these tests rejections has the additional implication that it is unnecessary to use nonparametric methods to evaluate the linearity hypothesis, given that it can already be rejected within the parametric setting. Adding to the flexibility of the non-linear terms only increases the explanatory power of the alternative hypothesis and thus the probability of rejecting the null of linearity. However, nonparametric tests are necessary to evaluate the hypothesis of separability. The reason is that when we evaluate separability both the null hypothesis, given by (18), and the alternative hypothesis, given by (11), are nonlinear. We want to be particularly flexible in our estimation of the additively separable form (18), in order to avoid the risk of rejecting it because we have imposed unreasonable restrictions on its functional form.

The rest of the columns of Table 6 report the results of four non-parametric tests of the separability hypothesis. The first one (column 2) is a Taylor polynomial test which is similar to the linearity test, but where in contrast we test for the joint significance only of the terms in the polynomial that contain interactions between the variables (e.g.: tariffs^2*primary exports). Since this is a subset of all the nonlinear terms, the results will obviously be somewhat weaker than those of the previous test where we evaluated the excludability of *all* the nonlinear terms. What we can clearly see, however, is that the rejection rate is still extremely high: in 75 of the 125 specifications (60.0%), we can reject the separability hypothesis.

[17] For a broader set of results, including several specifications of equation (10),(11), and (18), results using the World Bank (2004) *World Development Indicators* growth rate as the dependent variable, as well as technical explanations of the tests, see Rodríguez (2006b).

The rest of the columns of Table 6 report a set of other non-parametric terms of the separability hypothesis. These tests allow us to estimate both the separable and the non-separable specification with considerably more flexibility. The Fourier series expansion test (also known as the Hong-White test) relies on estimation of *f*(Z) by a flexible Fourier series approximation. This is a polynomial expansion in quadratic and trigonometric terms. There is an extensive econometric literature studying the properties of these estimators (Gallant, 1982, Geman and Huang, 1982 and Gallant, 1987). The basic benefit of a Fourier approximation is the greater flexibility of the trigonometric expansion to approximate highly non-linear functions. The residual regression and differencing tests both depend on direct estimation of the additively separable specification (18) by semi-parametric methods and analysis of its residuals. The residual regression test (Fan and Li, 1996) derives from the observation that if the separable specification is correct, then its residuals should be unrelated to any non-parametric function of the explanatory variables. Estimating a non-parametric regression of the residuals form the additively separable specification on the **Z** variables should thus allow us to evaluate whether the separability hypothesis is consistent with the data. The *differencing* test (Yatchew, 1988, 2003) compares the variance of the additively separable estimation with the variance of a full non-parametric specification. Its name derives from the fact that it uses information on the magnitude of the differences between the dependent variables of the observations that are nearby (in the sense of having similar values of the explanatory variables) to build an estimate of the variance of the full nonparametric specification.

The results of the last three columns of Table 6 confirm that, regardless of which non-parametric test we use, we find a rejection of the separability hypothesis in a preponderance of the tests. The rejection rates for the separability hypothesis range from a low of 68.8% in the residual regression tests to 88.0% in the Fourier series expansion tests to 98.4% in the differencing tests. All of the nonparametric tests therefore provide substantial evidence in favor of a non-separable specification.

4.2 What can the data say?

The results that we have just shown should be discouraging to those interested in using the growth regression framework to carry out policy analysis. They show that the cross-country data appear to be characterized by high-dimensional nonlinearities. Our discussion of underlying growth theory has highlighted the fact that theory is not much of a guide as to the way in which these variables may affect the growth function; our review of the empirical evidence has shown that neither is the empirical evidence.

These results contrast with the widespread use of empirical growth analysis to derive strong normative recommendations. Is there a way in which we can reconcile these two visions? In this section, we will use nonparametric tests to attempt to uncover the normative content of the cross-country data as regards the implications that can be drawn for the purposes of policy, institutional and structural reforms.

Within the linear cross-country empirical growth framework, it is common to use the results of conventional significance tests in order to draw policy implications. Therefore, the result that higher protection of property rights is associated with higher growth is often used to advocate recommendations for institutional reforms leading to greater protection of property rights. A logical question is what the counterpart of this type of analysis in the non-linear seting. In other words, what type of evidence would be necessary to find in the growth data to support a blanket recommendation to all countries to follow a specific course in terms of, for example, institutional reform?

We suggest that the appropriate concept in the nonlinear case is that of *monotonicity*. That is, a policy recommendation to follow a certain course of action should only be given if our evidence says that the growth function is monotonically increasing in the suggested course of action. An implication of this principle is that one should not recommend a policy is one knows that it may be damaging to the growth prospects of a country.

The monotonicity criterion may justly be perceived to be excessively restrictive, as it would imply that we should not recommend a policy if it were to harm just one country. An alternative possibility would be to adopt the criterion that one should not recommend a policy that is monotonically harmful for growth. In other words, if we find that the cross-country evidence shows that a policy hurts **all** countries, then we could not in good faith recommend such a policy.

For simplicity, assume that $h(\mathbf{Z})$ is a continuously differentiable function.[18] Estimation is based again on equation (10):

$$\gamma_t = \beta_0 + \beta_1 \ln \frac{Y_0}{L_0} + \beta_2 \ln s_k + \beta_3 \ln \frac{H_t}{L_t} + \beta_4 \ln(n+g+\delta) + \beta_6 h(\mathbf{Z}) + \eta_{it}. \qquad (10)$$

We will test (10) against two restrictive hypotheses. The first one is that growth is monotonically increasing in variable Z_i. This is the same as estimating (10) subject to the restriction that, for all possible values of the vector **Z**, the first derivative of the growth function with respect to **Z** is positive:

$$R_1 : \frac{\partial h(Z)}{\partial Z_i} > 0 \forall Z \qquad (19)$$

while the second one asserts that that derivative is negative (that the policy is harmful for growth):

$$R_2 : \frac{\partial h(Z)}{\partial Z_i} < 0 \forall Z \qquad (20)$$

Table 7 illustrates the possible configurations of results that could be obtained from these tests. For example, it is possible that the tests reject R_1 but do not reject R_2 (upper right corner). This evidence would be consistent with the idea that growth is monotonically decreasing in this variable, while at the same time rejecting the idea that the variable could be uniformly good for growth. The evidence would thus seem to give tentative support to a policy recommendation to decrease Z_i.. Likewise, it is possible for us to reject R_2 but be unable to reject R_1 (lower left corner). This would imply that we have found no evidence that the policy can be uniformly bad for growth but rather we have found support for the idea that it is conducive to higher growth. A policy recommendation to increase values of this variable would find support in the data.

The other two cases are trickier. Suppose we reject both hypotheses (upper left corner). Then the data would be telling us that growth is sometimes an increasing function and sometimes a decreasing function of policy Z_i. It would thus be difficult to give concrete policy recommendations because we are unsure of whether the policy will

[18] See Rodríguez(2006b) for a more general setup.

harm a country or not, as the data tell us that it is likely to harm some countries and not others. Alternatively, suppose we are in the lower right corner of Table 7. Here we find that the data can reject neither the positive monotonicity hypothesis nor the negative monotonicity hypothesis. Here the test is telling us that there is too little information in our data set to assert that either the variable is good for growth or bad for growth. In contrast to the upper left corner, where we know that the variable is good for growth in some countries and is bad for growth in others, here we cannot know whether it is uniformly good for growth, uniformly bad for growth, or neither.

The tests of the monotonicity hypothesis are shown in Table 8. These results show the outcome of tests carried out using Fourier expansions. We have coded the variables so that the positive monotonicity result (R_1) will always correspond to the "conventional wisdom" view. We refer to the conventional wisdom as that which is associated with Washington consensus recommendations: that openness is good for growth, black market premia are harmful, protection of the rule of law is beneficial, etc. We present the results of these tests for our indicators of economic policies, institutions, and economic structure. Again, we run all 125 combinations of specifications that combine one structural, one institutional, and one policy indicator.

Our results indicate that it is very difficult to reject the conventional wisdom hypothesis. Only in 4 (3.2%) of the specifications do we reject the conventional view of institutions, while the rejection rates of the conventional views of policies and institutions are equally low (7.2% and 8.0%). However, table 8 also shows that it is very difficult to reject the contrarian view of these variables – that the view of policies, institutions and structure held by the Washington consensus is completely wrong. Although the rejection rates of the contrarian view are higher, in no case are they above 50%, and in the case of policies – the centerpiece of the Washington consensus – they are as low as 35.2%. In other words, almost a full two thirds of estimated specifications (64.8%) do not allow us to reject the view that tariffs, black market premia, government consumption and inflation are uniformly **good** for growth. The results of other nonparametric tests (among them residual regression and differencing tests) are discussed at length in Rodríguez (2006b). These give even less support for the idea that one can draw policy conclusions from the data: the rejection rates for both contrarian and conventional view hypotheses are uniformly in the single digits.

I have chosen this "contrarian view" hypothesis to be deliberately outlandish. I do not imply to suggest that I believe there to be a reasonable argument that a country can achieve high growth by following the complete opposite of the Washington consensus view. What I claim is that the cross-country growth data is so uninformative that it does not even allow us to reject this extreme view of policies. The reason for this result can be traced back to the curse of dimensionality. It is nothing more than the expression of the difficulties that we have in attempting to estimate the growth function with any reasonable degree of precision. The curse of dimensionality tells us that we would need a very large number of observations to do this. Since we don't have that many observations, our methods can only estimate the growth function with very high imprecision, making it very difficult to meaningfully evaluate any hypotheses about the effect of potential determinants on growths.

A useful analogy can be drawn t the results of t-tests in standard linear regressions. When a coefficient fails to be significantly different from zero according to its t-statistic, this means that a confidence interval built around that estimator using our chosen level of significance will include both positive and negative values. Therefore, lack of significance implies that we cannot rule out the hypothesis that the coefficient is positive nor can we rule out the hypothesis that it is negative. Low t-statistics are common in regressions with very few observations, because in those cases the data has insufficient information to allow us to rule out potentially competing hypotheses.

A similar phenomenon operates here. Even though the number of observations used in these regressions (depending on the specification, they oscillate between 70 and 100) are generally enough to obtain significant coefficients in linear regressions, their informational content is much lower in a nonparametric multidimensional setting due to the curse of dimensionality. Thus we are generally incapable of decisively rejecting any hypothesis about the effect of these variables on growth.

In sum, cross-country growth data does not appear to be equipped to allow us to rule out competing visions about the effects of policies, institutions and economic structure on growth. It is, in contrast, consistent with many of these visions. The reason that conventional growth regressions reach another conclusion appears to be due to the fact that they invalidly restrict their estimated regressions to peculiar functional forms. These functional forms have neither a theoretical nor an empirical foundation.

5. Concluding Comments

It is useful to offer a brief summary of what our empirical analysis has – and has not – done. First, it has shown that there is decisive evidence against a linear view of the growth process. The data consistently reject linear specifications in favor of more general, nonlinear ones. Second, it has shown that there is also strong evidence against a view of the world in which interactions between different growth determinants are absent. Specifications that allow for these interactions perform much more strongly and a preponderance of tests prefer these complex specifications to the simpler, separable ones.

The third important result that emerges from our empirical analysis is that the growth data is generally unable to deliver strong conclusions about the sign of the growth effects of its possible determinants. The curse of dimensionality conspires to make it very difficult for us to reject even extreme, unconventional visions of the growth process. The idea of using growth empirics as a support for policy advice is an idea that is grounded in the assumptions of the linear framework, and whose validity becomes highly questionable once we realize that the growth process is characterized by high dimensional nonlinearities.

These conclusions can appear nihilistic. If we can't say anything about the effect of policies on growth, the reader may ask, then what is the use of this analysis? Is there any value of looking at the cross-country data? Or should we be resigned to the idea that we live in a world that we cannot understand.

I believe that a close reading of the results presented in this paper suggests a different interpretation. The results of the linearity and separability tests reported in Table 6, for example, are decisive in their rejection of these visions of the growth process. In other words, the data is telling us that we do not live in a simple world, where the effects of policies are approximately constant or can be separated from the values of other determinants of the growth process. The data has thus allowed us to distinguish between two competing visions of the world: one in which the growth process is approximately similar in the bulk of the world's countries, and another one where it is not. A nihilistic approach would not have been able to choose between these two visions; our approach has. I believe that further research along these lines may be able to uncover other interesting characteristics of the growth process that will be useful in the design of growth strategies.

I think that the main lesson that emerges from the exercise carried out in this paper is that growth empirics may have been asking the wrong set of questions. Growth empirics has attempted to garner evidence that can support policy recommendations that can be given to all developing countries. In this sense, it has acted as the academic counterpart of the "one size fits all" approach to policy design. The reason that the data is unable to come up with adequate answers to the questions posed by this paradigm is that in a world characterized by complexities and high-dimensional nonlinearities in the growth process, these questions stop making much sense. Once we recognize that the growth effects of a policy will depend on a country's structural and institutional characteristics, asking whether openness is good for growth makes as little sense as asking whether all undergraduates should major in physics or in history.

A complex vision of the growth process is necessary for us to make sense of the wide divergences that exist in the growth performances of countries that have carried out similar policies. It is also a vital part of understanding the phenomenon of countries that have found alternative pathways to high growth. I have argued that these varied experiences can be understood in the context of the existence of strong non-linearities and interactions in the growth process. Different policies can bring the same outcome because growth can be increasing in that policy over certain ranges but decreasing over others. Countries may experiment few growth effects of a policy reform because they don't have in place the institutional or structural conditions necessary to implement them.

If the results of this paper are correct, they have strong implications for the Washington Consensus approach to thinking about policies. Strong non-linearities in the growth process imply that it makes little sense to think about policy reforms abstracting from an economy's structural or institutional characteristics. The reforms that will work for a country may not work for others. Policy thinking should start from considering the country-specific characteristics that are likely to make certain policies work rather than trying to draw lists of reforms to be applied to large groups of countries.

One possible criticism of our analysis is that it subjects the field of growth empirics to an unfairly high standard. As pointed out by Jaime Ros in his comments to the conference version of this paper, linear specifications are prevalent in economics and in other social sciences. Thus it is possible that many other fields would be open to the same criticism that we have leveled.

My reaction to this comment is two-fold. While it is true that linear specifications are common in some fields of economics, it is also true that there are many other fields of the discipline where that is not the case. Many of the tests used in this paper were in fact developed in the field of production function estimation, where considerable attention has been paid to the issue of functional form and where tests for misspecification and omitted nonlinearities are common. My main contention is not that linear specifications are inadequate, but that they should be tested. The tests presented in this paper were able to reject the hypothesis of linearity decisively. This may or may not be the result of applying similar tests to other results in our discipline. What is certain is that we will never know this until we carry out the tests.

The results of this paper may be of particular interest to those involved in quantitative research in other social sciences. The problem of choosing a functional form to estimate the effect of potential determinants on, for example, political outcomes, is subject to the same type of problems faced by the determination of how potential production function shifters affect aggregate productivity and economic growth. Political theory can seldom provide concrete guidance as to the functional form that should be used to capture complex social processes. The adoption of linear specifications is a convenient expedient, but, as we have pointed out in this paper, erroneously adopting a linear specification can often lead to significantly distorted results. A logical step to take in this type of research would be to evaluate whether existing specifications are able to pass the type of linearity and separability tests that we propose in this paper. If they cannot, this result would invite deeper thinking about the feasibility of answering the type of questions that are commonly posed to this data.

Recognizing the true informational limitations of cross-national data sets to handle the study of complex social processes is likely to lead us to a reevaluation of the use of country-level evidence. There is a wealth of methods that can be used to attempt to understand the growth process at the level of specific economies. Detailed microeconomic studies can exploit the availability of information in labor and industrial surveys to help us understand the causes of productivity and human capital accumulation. Time-series studies of macroeconomic interactions can help us make sense of an economy's reaction to monetary and fiscal policy shocks. Historical and institutional analyses can help us understand the complex links between political alliances and economic policy design.

It is regrettably rare to see serious attempts at putting these different pieces of a country's growth puzzle together. In contrast, many fields of economics have developed methodologies to test hypotheses about aggregate economic performance at the country level. To take an example, the broad literature on the effects of trade on income inequality in the United States has relied almost completely on the application of methods that combine the use of theoretical knowledge with the analysis of country level evidence.[19] My hope is that this example can be followed within the field of empirical growth analysis in a way that allows us to evaluate competing hypotheses in a rigorous manner. The development of a *within-country growth empirics* that can help answer some of the questions that cross-country growth empirics have been unable to do represents one of the most exciting research projects in the study of economic development.[20]

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[19] See, for example, Katz and Autor (1999), Freeman (1995) and Feenstra and Hanson (2003).

[20] For a tentative attempt to develop and implement this type of analysis, see Hausmann and Rodríguez (2006).

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Table 1: A Tale of Two Countries

	Ecuador	Perú
Agriculture/GDP	7.7	10.3
Industry /GDP	28.7	29.3
Services/GDP	63.6	60.4
Savings/GNP	23.7	17.5
Urbanization Rate	64	74
Inflation	7.9	2.3
Average Tariff	11.29	11.23
Fuel and Mining Exports/Total Exports	40.67	48.1
Current Account Surplus/Deficit	-1.75	-1.67
Government Consumption/GDP	9.5	10.1
Fiscal Surplus/GDP	1.9	-1.8
Growth Rate, 1990-2003	-0.13	1.92

Source: World Bank (2005). All Data from 2005 except for the Urbanization Rate, which reflects the earliest available, and the growth rate, which is for 1990-03.

Table 2: Countries exceeding 2% Annual Growth in the Region, 1990-2003				
Rank	Country	Growth Rate, 1990-2003	Average Tariff Rate	
1	Chile	3.9%	11.1 %(*)	
2	St. Kitts and Nevis	3.6%	14.8%	
3	Belize	3.6%	17.5%	
4	Dominican Rep.	3.5%	13.3%	
5	Trinidad & Tobago	2.9%	4.5%	
6	Panama	2.8%	3.1%	
7	Costa Rica	2.6%	5.1%	
8	Grenada	2.3%	7.4%	
	Region Average	1.2%	7.2%	

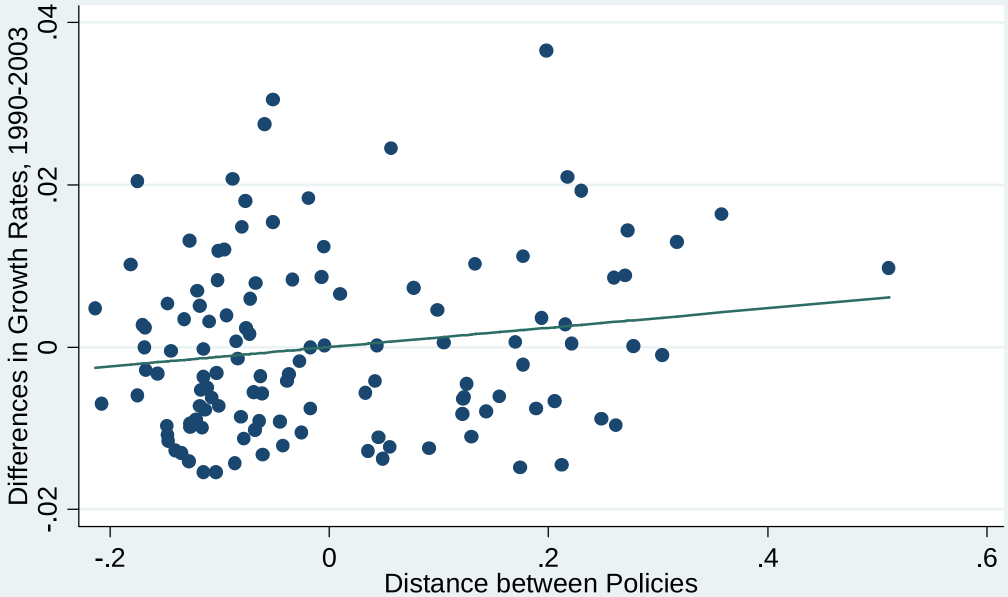
Source: World Bank (2005). (*)- MFN tariff in 1997 from 1997 WTO Trade Policy Review (WTO, 1997)

Table 3: Growth Performance of 10 Most Liberalized Economies in the Region (Tariff Criteria), 1990-2003			
Rank	Country	Growth Rate, 1990-2003	Average Tariff Rate
1	El Salvador	1.7%	0.3%
2	Panama	2.8%	3.1%
3	Bolivia	1.1%	3.1%
4	Mexico	1.1%	3.4%
5	Trinidad & Tobago	2.9%	4.5%
6	Paraguay	-0.5%	4.6%
7	Jamaica	0.0%	4.7%
8	Colombia	0.5%	4.8%
9	Costa Rica	2.6%	5.1%
10	Nicaragua	0.3%	5.4%
	Region Average	1.33%	6.28%

Source: World Bank (2005)

Figure 1: Linear OLS estimates, no controls

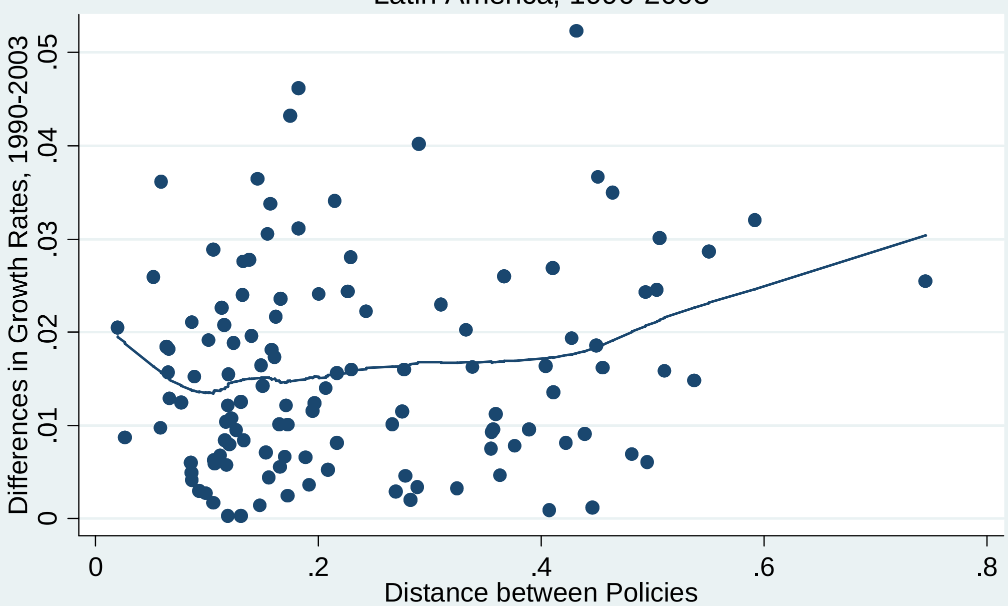
Latin America, 1990-2003



coef = .01191172, se = .00469997, t = 2.53

Figure 2: Nonparametric estimates, no controls

Latin America, 1990-2003



bandwidth = .8

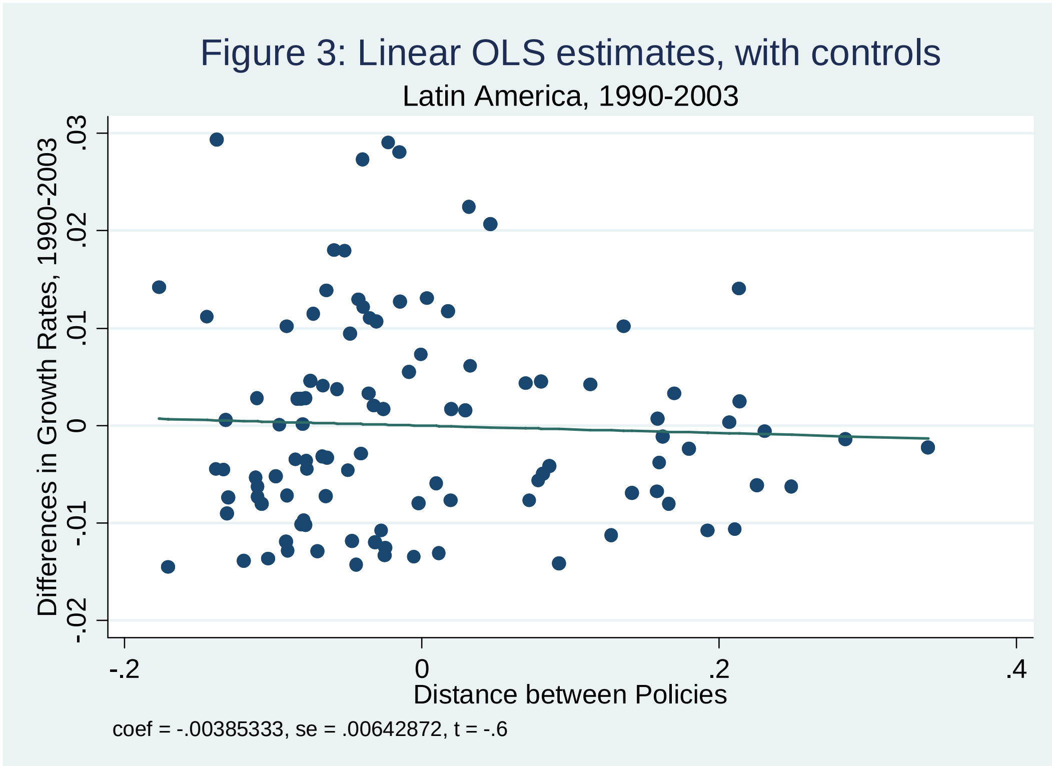


Figure 3: Linear OLS estimates, with controls
Latin America, 1990-2003
Differences in Growth Rates, 1990-2003
-.02
-.01
0
.01
.02
.03
-.2
0
.2
.4
Distance between Policies
coef = -.00385333, se = .00642872, t = -.6

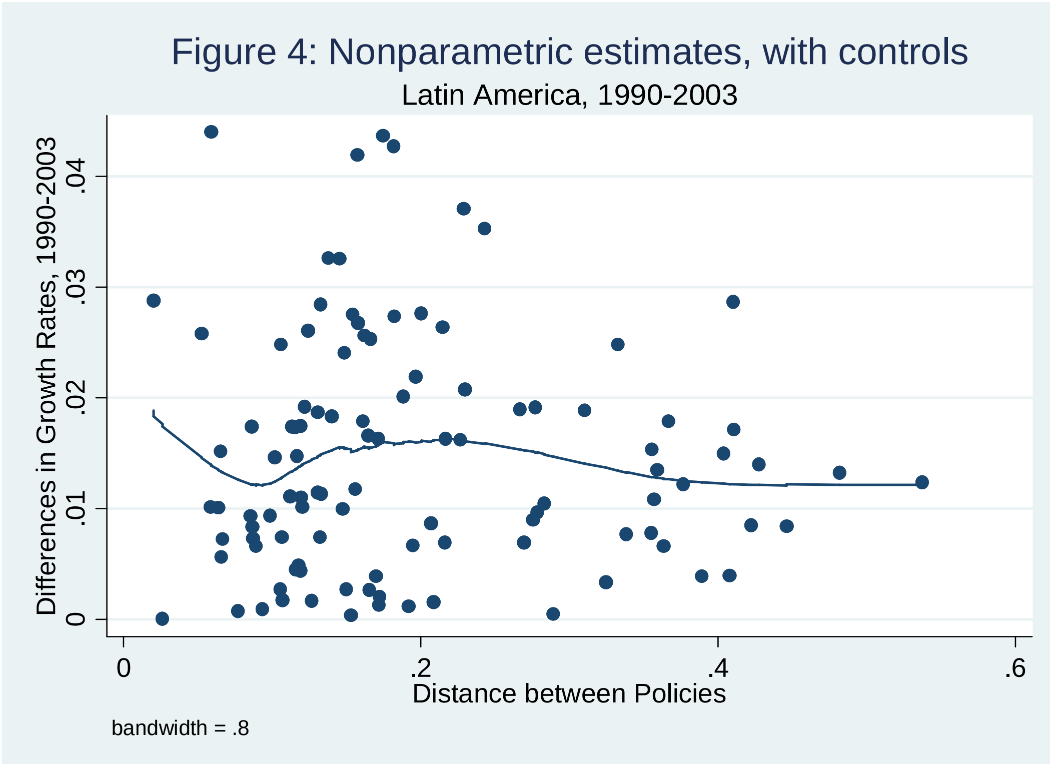


Table 4: Mean absolute value of differences in growth performances between pairs of economies						
		Euclidean Distance Between Policy Vectors				
		Below median	Above median	Difference	t	n
World	No Controls	1.98%	2.35%	0.36%	7.75***	7306
	With Controls	1.85%	2.30%	0.45%	7.67***	4028
Latin America	No Controls	1.40%	1.75%	0.35%	2.50**	238
	With Controls	1.39%	1.55%	0.16%	1.09	208

Figure 5: Non-linear example 1

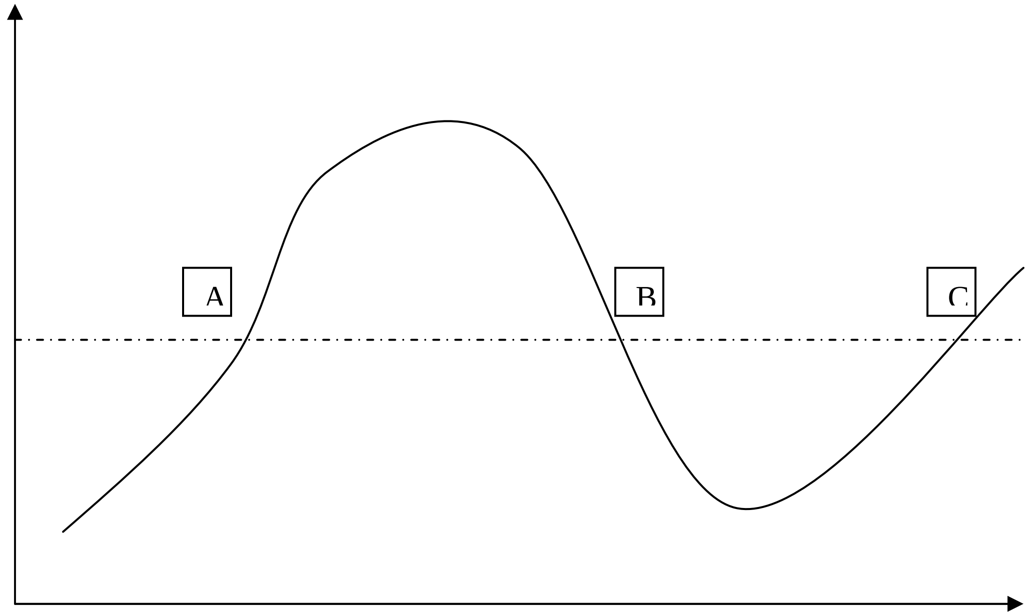


Figure 6: Non-linear example 2

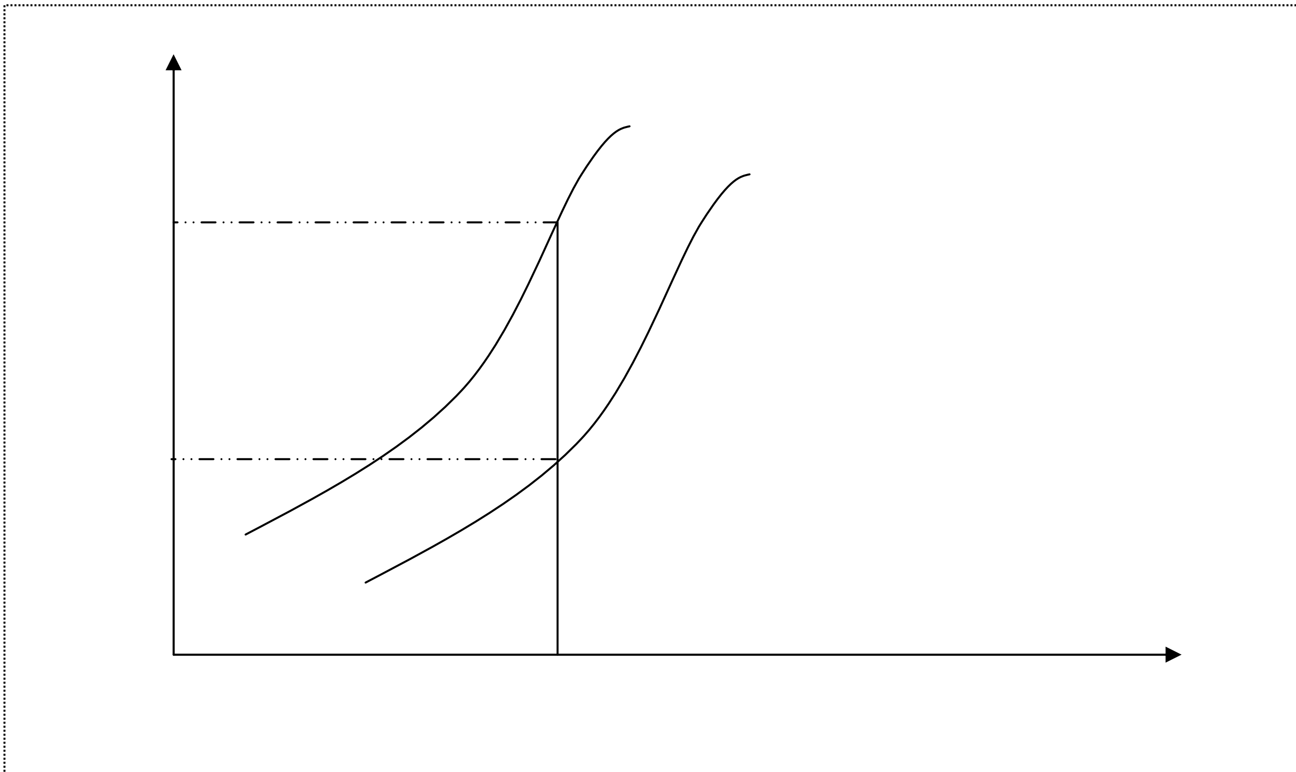


Table 5: Variable Descriptions

Policy Indicators	
1. Trade Policy Openness	$(1+t_m)(1+t_e)-1$, with t_m (t_e) the ratio of import (export) tax revenue in total imports (exports); Data from World Bank (2004)
2. Log of Black Market Premium	Dollar and Kraay (2002)
3. Government Consumption as a Percentage of GDP	World Bank (2004)
4. Log of (1+Inflation Rate)	World Bank (2004)
5.Summary Policy Indicator	Sum of 1-4, normalized over the unit interval
Institutional Indicators	
6. Rule of Law	Dollar and Kraay (2002)
7. Political Instability	Average Variation in POLITY variable, Polity IV Data Set.
8. Effectiveness of Government Spending	Glaeser et al. (2004)
9. Economic Freedom Index	Heritage Foundation
10. Summary Institutions Indicator	Sum of 6-9, normalized over the unit interval
Economic Structure Indicators	
11. Share of Primary Exports in Total Exports	World Bank (2004)
12. Urbanization Rate	World Bank (2004)
13. Share of liquid liabilities in GDP	International Monetary Fund (2004)
14, Life Expectancy	World Bank (2004)
15. Summary Structure Indicator	Sum of 10-14, normalized over the unit interval

Table 6: Linearity and Separability Tests, Penn World Tables 1975-00

Equation	*Linearity*	Separability			
Controls	*Taylor Polynomial*	*Taylor Polynomial*	*Fourier Series Expansion*	*Residual Regression*	*Differencing*
Median F-Statistic	5.01	2.40	5.38	2.63	33.27
Median P-Value	0.00	0.02	0.00	0.00	0.00
Number significant (/125)	114	75	110	86	123
Percent Significant	91.2%	60.0%	88.0%	68.8%	98.4%

Table 7: Possible results of monotonicity tests and implications

	Reject R_2	*Cannot Reject R_2*
Reject R_1	• *Nonlinear function that has both increasing and decreasing segments.* • *No general policy recommendations can be given*	• *Evidence is consistent with Z_i being harmful for growth.* • *Recommendation of increasing Z_i can be given.*
Cannot Reject R_1	• *Evidence is consistent with Z_i being beneficial for growth.* • *Recommendation of decreasing Z_i can be given.*	• *Evidence is uninformative about the form of the growth function* • *No general policy recommendations can be given.*

Table 8: Fourier Expansion Tests of Monotonicity (PWT Data)

	Conventional Wisdom			**Contrarian View**		
	Policies	**Institutions**	**Structure**	**Policies**	**Institutions**	**Structure**
Median F-Statistic	-1.62	-1.91	-2.09	0.76	0.87	1.44
Median P-Value	0.95	0.97	0.98	0.22	0.19	0.08
Number significant (/125)	9	4	10	44	52	62
Percent Significant	7.2%	3.2%	8.0%	35.2%	41.6%	49.6%