Some Historical, Theoretical, and Methodological Issues in Identifying Effects of Political Institutions

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Abstract

Recent research on the impact of institutions suffers from historical, theoretical, and methodological problems. The historical issue is whether the initially poorer countries became wealthier, as Acemoglou, Johnson, and Robinson (2002) would have us believe. The theoretical question is whether the institutions that should matter are those that protect property rights or those that coordindate investment or render rulers accountable. Finally, methodological issues include the definition of the dependent variable, the identifying assumption that institutions did not change, and the pitfalls of instrumental variables.

1   Introduction

An avalanche of recent papers places political institutions at the altar of economic development. Development, we are told, is due only superficially to the supply of productive factors or to progress in using them.

The central claim of "new institutionalism" is that institutions are the "primary" cause of economic development, "deeper" than features of the natural environment, "geography," and deeper than the supply of factors and the technologies for their use. The theoretical program has been laid out by North (1997: 224; italics supplied): "To make sense out of historical and contemporary evidence, we must rethink the whole process of economic growth.... The *primary source* of economic growth is the institutional/organizational structure of a political economy...." Specifically, we learn that "Third World countries are poor because the institutional constraints define a set of payoffs to political/economic activity that do not encourage productive activity." (1990: 110).

This program pervades recent research on development. Thus, in a paper entitled, "Institutions Rule: The Primacy of Institutions ....," Rodrik, Subramanian, and Trebbi (2002: 2; italics supplied) observe that "Growth theory has traditionally focused on physical or human capital accumulation, and, in its endogenous growth variant, on technological change. But accumulation and technological change are at best *proximate causes* of economic growth." Acemoglou (2003a; italics supplied) repeats: "poor countries ... often lack functioning markets, their populations are poorly educated, and their machinery and

technology are outdated or nonexistent. But these are only *proximate causes* of poverty...."

The main fact cited in favor of the institutionalist perspective is what Acemoglou, Johnson, and Robinson (2002; henceforth AJR) term "the reversal of fortunes." According to AJR, countries that were wealthier in 1500 (as measured by population density or urbanization rates) are the ones which are less developed now. This view is supported by Engerman and Sokoloff (1997, 2001; also Sokoloff 2000) with regard to the Americas, where the initial date is mid-eighteenth century. Finally, Banerjee and Iyer (2002), having gone back to mid-nineteenth century, found a reversal among districts within India.

The reversal occurred because relatively poor regions were sparsely populated, and this enabled or induced Europeans to settle in large numbers and develop institutions encouraging investment. In contrast, a large population and relative prosperity made extractive institutions more profitable for the colonizers. Areas that were originally wealthier adopted worse institutions. These institutions persisted, "blocking" the opportunity that was offered by industrialization (On the notion of "blocking," see Przeworski 2004b). As a result, the initially wealthier areas "fell behind," to use the phrase of Haber (1997).

The reversal motivates the institutional perspective. Thus, AJR (2002: 1233) observe: "The simplest version of the geography hypothesis emphasized the time-invariant effects of geographic variables, such as climate and disease, on work and productivity, and therefore predicts that nations and areas that were relatively rich in 1500 should also be relatively prosperous today. The

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reversal in relative incomes weighs against this simple version of the geography hypothesis." Banerjee and Iyer (2002: 1) also juxtapose these two views: "In the new institutionalist view, history matters because history shapes institutions and institutions shape the economy. By contrast, in what one might call the 'increasing returns' view, historical accidents put one country ahead in terms of aggregate wealth or human capital ... and this turns into bigger and bigger differences over time because of the increasing returns."

The historical, theoretical, and methodological issues raised by this perspective are innumerable. Below, I focus only on two that I see as central: (1) Was there a reversal? (2) Which institutions should we expect to matter? (3) How to identify their effects in the presence of endogeneity?

2   Reversal?

AJR did not have income data for the early periods. They argued, however, that the degree of urbanization and population density are correlated with wealth. With this assumption, they regressed per capita income in 1995 on urbanization and on population density around 1500, and in both cases found negative slopes. This is their evidence for the reversal. In the meantime, however, Maddison (2003) completed his monumental project on *The World Economy,* generating income data for several countries and most regions going back at least to 1500. To avoid having to repeat the caveats, let me emphasize that some of these data consists of guesses, or more politely "conjectures," and there are no grounds to

be sure that Maddison's conjectures are more reliable than those on urbanization or population density, used by AJR. But these are the best conjectures about income we have to date. And it turns out that when per capita income in 2001 is regressed on that in 1500, the slope is slightly positive (while the coefficient is not significant):

*** Figure 1 here ***

The slope is also positive when we go back only to 1700 and even more positive and statistically significant when we go back to 1820. While these results cast doubt on the conclusions of AJR, the Maddison data for the two earlier dates include mostly countries that were highly developed by 2001. Hence, these results may suffer from selection bias in addition to the reliability issues. What does seem apparent is that no reversal occurred after 1820, but this fact is not under dispute.

*** Table 1 here ***

Now that we have income data, however, we do something AJR could not do, namely plot the time paths of incomes. Unfortunately, tropical countries are not only income- but also data-poor, so the numbers of observations are small. I take the data on the proportion of land area located in the tropical zone, from Gallup, Mellinguer, and Sachs (2003), and divide the countries into those that have no area in the tropics, some area in the tropics, and almost all (at least

93 percent) in the tropics.[1] The plot of incomes looks as follows (solid is for non-tropical, dashed for part tropical, dots for tropical):

*** Figure 2 here ***

The numbers are given in Table 2:

*** Table 2 here ***

Note that no observations for the tropical areas are available before 1820. But the non-tropical areas, which include Europe, were wealthier to begin with than the partly tropical countries (which include the United States) and remained wealthier. At least with these data, no reversal can be seen. Moreover, in accordance with Sachs, the gap opened up, already in 1820 for the tropical areas and after 1880 for the partly tropical ones. Hence, increasing returns are manifest.

3   Which Institutions?

The institutions that matter for development in the neo-institutionalist perspective are almost always those that "safeguard property rights." This idea goes back to North and Thomas (1973), indeed to Machiavelli, who observed that "everybody is eager to acquire such things and to obtain property, provided

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[1] This is because of the thirty-seven countries for which 1820 income data are available, twenty-three have no area in the tropics, eight have areas between 1.5 and 51.2 percent, Brazil has 93.1 percent, and five countries have their entire area in the tropics.

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that he be convinced that he will enjoy it when it has been acquired" (*Discourses on Livy. II.2*, cited after Holmes 2003). Thus, the definition of "good" institutions offered by AJRobinson (2002: 1262) goes as follows: "We take a good organization of society to correspond to a cluster of (political, economic, and social) institutions ensuring that a broad section of society has effective property rights."

What neo-institutionalists mean by secure property rights is protection against the risk of expropriation (of alienable productive assets or income) via the political process.[2] Indeed, almost all statistical papers use as an index of secure property rights the risk of expropriation as assessed by a Washington consulting firm, Political Risk Services.[3] Yet even if we assume that these are valid indicators of what they intend to measure,[4] the obvious question is whether they indicate that quality of institutions which is theoretically relevant. AJR (2002: 1270) are aware of this difference: they observe that their measure of institutions may "correspond poorly to the real concept that is relevant to development (which is likely to be a broad range of institutions, whereas we only have an index for a particular type of institutions)." So are Easterly and Levine (2002: 33): "Nor does the kind of general indicator of institutional quality we

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[2] Note that property can be made secure by might, rather than rights. For example, Hafer (2003) shows that a stable property system may be based on individually specific abilities to defend property by force. Acemoglou (2003b), in turn, argues that property can be defended by elevating barriers to entry, which is not a rights-based mechanism.

[3] Hall and Jones 1999; AJR, who also use "constraints on the chief executive" from Polity III; Easterly 2002 as well as Rodrik, Subramanian, and Trebbi 2002, who also use other indices generated by Kaufmann, Kraay, and Zoido-Lóbaton 1999.

[4] For doubts on this topic, see Aron (2000) who observes that indicators based on irreproducible judgements always predict performance, while those based on observable features of institutions rarely do. She also raises the possibility that such indicators may simply reflect recent performance.

use, ..., provide much guidance to officials making real laws and regulations."
Yet everyone relies on such indices.

The main point of Bardhan (2004) is that the new institutionalism got its institutions wrong. If "security of property rights" is the New Testament, we also have the Old Testament, drafted by Rosenstein-Rodan (1943; for a formal model see Murphy, Shleifer, and Vishny 1989) which says that institutions that matter are those that coordinate investment. In the literature of the 1960s, these were the institutions that force savings (Galenson 1959, DeSchweinitz 1959, Huntington 1968, Huntington and Dominguez 1975), while recent statistical studies in this vein typically emphasize the role of financial institutions (Beck, Levine, and Loyaza 2000; King and Levine 1993; Levine and Zervos 1998; Neusser and Kugler 1998; Rousseau and Wachtel 1998).

Finally, we can think that the institutions that matter for development are those that makes rulers accountable, those that provide information about government's actions and permit citizens to sanction bad behavior by throwing governments out of office. Such institutions should induce governments to limit rent extraction and to promote growth (Benhabib and Przeworski 2004).

Securing property rights, coordinating investment, and rendering the rulers accountable are second-order features of complex institutional frameworks. As such, they constitute consequences of specific institutions, such as patterns of separation of powers, the independence of the judiciary or of central banks, procedures for electing rulers, and the like. Hence, the first question is which specific institutional arrangements promote these second-order features, for ex-

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ample, whether it is true that subjecting rulers to periodic elections makes them accountable (Manin, Przeworski, and Stokes 1999). One cannot directly engineer accountability, only those institutional features one expects to promote accountability. Yet since institutions are complex systems, they have emergent, configurative, effects. Hence, in the end, it may be necessary to characterize institutional systems in terms of their effects on the security of property, coordination of investment, and political accountability.

4   Identifying Effects of Institutions

Following North (1980, 1990, 1997), the theory of "new institutionalism" consists of two propositions: (1) "Institutions matter": they influence norms, beliefs, and actions; therefore, they shape outcomes. (2) "Institutions are endogenous": their form and their functioning depend on the conditions under which they emerge and endure.

Yet if institutions constitute a primary cause, they cannot be caused by something else. This is why the two central axioms of new institutionalism do not easily cohabit the same theory. Following AJR (2002), statistical studies of the impact of institutions adopt the following procedure: (1) Regress per capita incomes for a recent date (or an average of recent dates) on recent institutions and some control variables, (2) Instrument recent institutions by instrumenting institutions at some time immemorial. In my view, this procedure is deeply flawed, for several reasons: (1) The divergence between the theoretically relevant

and the operational measures of institutions, discussed above. (2) The operationalization of the dependent variable, (3) The assumption that institutions do not change, (4) Problems with the use of instrumental variables estimator.

Economists are notoriously cavalier about taking the growth of *per capita income* as the dependent variable. The dynamic of population is ignored altogether in the "kitchen-sink" regressions, such as Barro's (1989), while population is assumed to grow at a constant exogenous rate in the theoretically motivated studies following Mankiw, Roemer, and Weil (1992). Yet the rate of growth of per capita income is a result of two, partly independent, processes: the growth of total output and the growth of population. And, very much to their surprise, Przeworski et al. (2000) found that political regimes systematically affect the rate of growth of population. The same holds for the Polity measure of institutions (Przeworski 2004c). It thus appears that if political institutions affect anything, it is demographic, rather than economic, performance.

AJR estimate a model in which *per capita* income at the most recent date depends on institutions, finding that good institutions are associated with higher incomes independently of conditions. This model implies that good institutions should generate higher growth rates during any period subperiod. Consider during the period 1950-1999 countries that were colonies as of 1945. It turns out that institutions affect demographic rather than economic performance: the rate of population growth ($POPG$) falls as the quality of institutions increases, the rate of growth of total output (YG) does not depend on institutions, so if the rate of growth of per capita income (G) does increase slightly with the quality

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of institutions it is only because of lower population growth. The table below shows means of these variables conditional on $XCONST$ as well as the OLS and 2SLS coefficients of $XCONST$.[5] :

*** Table 3 here ***

Hence, it looks like countries with better institutions had higher per capita incomes in the recent period because they had experienced lower population growth, not because their total output grew faster.

A central problem in identifying econometric models of the impact of institutions is to find instruments for the endogenous variables. It is this difficulty that led AJR and their followers to go back far in history. Yet since institutions changed in the meantime, and they did, the strategy of skipping centuries fails. Here is the evidence:

*** Table 4 here ***

The crosstab shows a lot of volatility. Of the fifty-eight countries that entered the world with worst possible institutions, twenty-six ended up with good institutions (5 or more), while eleven out of forty-six countries went from best possible to bad institutions (3 or less). The correlation between entry and exist institutions is only 0.26. This correlation is higher, 0.55, when we consider only

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[5] The first-stage equation for 2SLS includes per capita income, number of past transitions to democracy, the proportion of other countries in the world that are democracies during a given year, and a dummy for British colonies. Controlling the estimates for per capita income makes no difference except for POPG in 2SLS, which is reported with this control and AR1. Standard errors in parentheses.

those countries that were not independent as of 1945, but this is still only 28 percent of variance.

The problem with the use of instrumental variables goes even deeper. As argued by Djankov et al. (2003), in those areas where European settlers brought in good institutions, they also brought themselves, that is, their human capital. Indeed, Glaeser et al. (2004) discovered that the initial level of human capital has an effect independent of institutions. Since the impact of the instrument – settler mortality – on development is not exhausted by their impact on institutions, the instrument is correlated with the error in the growth equation and the estimates are still biased. Finally, for reasons outlined by Heckman (1997, 2004), one should not rely exclusively on instrumental variables.

Consider a schematic representation of the possible directions of causality:

Geography —> Institutions —> Forces —> Growth
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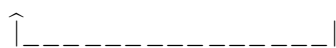
The invariant background conditions, "geography,"[6] determine the initial institutions, which in turn determine the supply of factors and the technologies that exploit them. Hence, what in the marxist language would be "forces of production" (Cohen 1978) and in the neoclassical language "physical or human capital accumulation and technological change" (see the quote from Rodrik, Subramanian, and Trebbi at the beginning) are only "proximate" causes. But

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[6] While the view that geography plays an important role in shaping the patterns and the rhythm of development dates back to Montesquieu (1748), this perspective has been recently revitalized under the influence of Diamond (1997) and applied to explain long-term patterns of economic growth by Sachs (2001) and his collaborators (Gallup, Sachs, and Mellinger 1998).

institutions are still caused by something else, not only by the invariant background conditions but also by the development they generate.

The new institutionalism does recognize that institutions are endogenous. As already North and Thomas (1973: 6) observed, "new institutional arrangements will not be set up unless the private benefits of their creation promise to exceed the costs."[7] And the embarrassingly obvious thought is that if endogeneity is sufficiently strong, institutions cannot have a causal efficacy of their own. Imagine that only those institutions that generate some specific outcomes, say those that perpetuate the power of the otherwise powerful, are viable under the given conditions.[8] Then institutions have no autonomous role to play. Conditions shape institutions and institutions only transmit the causal effects of these conditions. And if this is true, if institutions are endogenous, it may be that the "institutional constraints define a set of payoffs to political/economic activity that do not encourage productive activity" precisely in those countries where returns to productive activity are relatively low. When returns to producing are low, those who populate political institutions prefer to engage in rent seeking (Acemoglou 1995, Murphy, Shleifer, and Vishny 1993). The institutionalist explanation of the poverty of the Third World is then circular.

To evaluate the impact of institutions we must use the observed world to

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[7] There is nothing new about the claim that institutions are endogenous. Montesquieu as well as Rousseau, the latter in his folkloric description of Poland, claimed that particular institutions can function only if they correspond to cultures, mores, religions, or geographic conditions. J.S. Mill considered the issue of endogeneity in the first chapter of *Considerations*, entitled "To What Extent Forms of Government are a Matter of Choice."

[8] Note that this is what most of us tended to believe some thirty years ago. See Przeworski (2004a).

make inferences about a hypothetical one. If history had generously produced natural experiments[9], we could assess causality simply by matching the observed institutions and comparing their outcomes. But what can we say about causality when institutions are endogenous? The issue concerns the status of subjunctive conditionals in which the antecedent cannot or at least is unlikely to be realized.[10] To qualify as causes, the particular variables must be capable of assuming different values under the same conditions, that is, they must be "manipulable" (Holland 1986). What distinguishes causality from correlation is manipulability: "Associational inference involves the joint or conditional distributions of values of Y and A, and causal inference concerns the values $Y|x, u - Y|x\prime, u$ on individual units." (Holland 1986: 948; A stands for factors that cannot be manipulated, "attributes"; u stands for background conditions.) In the end, "causes are only those things that could, in principle, be treatments in experiments" (1986: 954). Hence, all econometric strategies for identifying the role of institutions (X) rely on exploiting their variation under the background conditions (U) to construct counterfactuals and then matching, one way or another, the observations with their counterfactual pairs.

Yet inferences based on non-experimental evidence are subject to several biases. Let one set of institutions constitute the "treatment," while others serve as "controls":

(1) *Baseline difference.* The units observed as treated may have had different

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[9] A beautiful example where it did is by Banerjee and Iyer (2002).
[10] On causality in a probabilistic framework, see Rubin (1974), Rosenberg (2002), and Heckman (2004).

performance as controls than those that were in fact controls or vice-versa.

(2) *Effect of the treatment on the treated.* The units observed as controls may have performed differently under treatment than those that were actually observed as treated. This will be true of the choice of treatment is even in part driven by its consequences. Notably, under such conditions, the instrumental variables estimator fails to correct the bias (Heckman 1997).

(3) *Post-treatment effect.* I am using here the terminology of King and Zeng (2001), even though I think it is a misnomer. The problem here is that changing just the treatment and nothing else that affects performance may be impossible. This is the gist of the Glaeser et al. critique of AJR. The bias arises because, as Lebow (2000: 5757) puts it, "'Surgical' counterfactuals are no more realistic than surgical air strikes." In the real world, change of one state is associated with a change of others. Hence, perhaps this is a "non-local" effect.

(4) *Distance effect.* King and Zeng (2001) show that the distance between the conditions under which the actual cases were observed and their closest observed matches under alternative institutions is also a source of bias. Extrapolating outside the range of observations is less reliable than interpolating within it. But even the distances within the observed range matter, particularly in the presence of non-linearities.

(5) *Aggregate effect.* This is a violation of the "stable unit treatment value assumption" (SUTVA), namely, that observations are independent, which implies that realizations of counterfactuals do not alter the values actually observed. All countries cannot be net exporters: had Latin American countries adopted

export-orientation strategy earlier, the performance of the Far Eastern countries that did so would have not been the same.

No single estimator corrects for all the biases. As Dawid (2000) argues, all inferences involving counterfactuals are metaphysical: in addition to observations, inferences about them must rely on untestable assumptions. In Heckman's (2004: 51) words, "There is no assumption-free method of causal inference." The reason is that even if we observe the marginal distributions of outcomes separately under different institutions, by construction we cannot observe their joint distribution for each set of background conditions, some of which may be unobservable. Hence, different assumptions are needed to correct for each of these biases. We do have a number of statistical ways to cope with them: instrumental variables, Heckman selection models, differences-in-differences, as well as various forms of propensity matching.[11] But since each estimator invokes different assumptions, we must be concerned about the robustness of the conclusions. The question raised thirty years ago by Alasdair MacIntyre (1972) – "Is the science of comparative politics possible?" – is not rhetorical.

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[11] Rosenberg (2002) presents an exceptionally clear discussion of the issues involved. Winship and Morgan (1999) is a good review of the alternative statistical methods.

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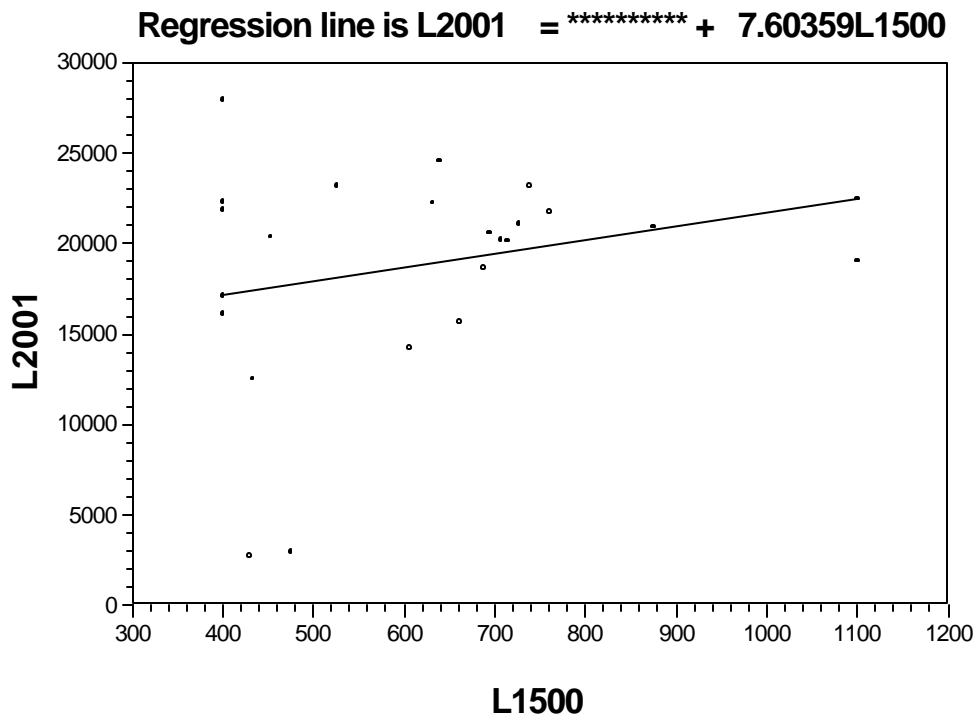
Figure 1: gdp/cap in 2001 as a function of 1500

Table 1: Per capita income in 1999 as a function of incomes at different dates.

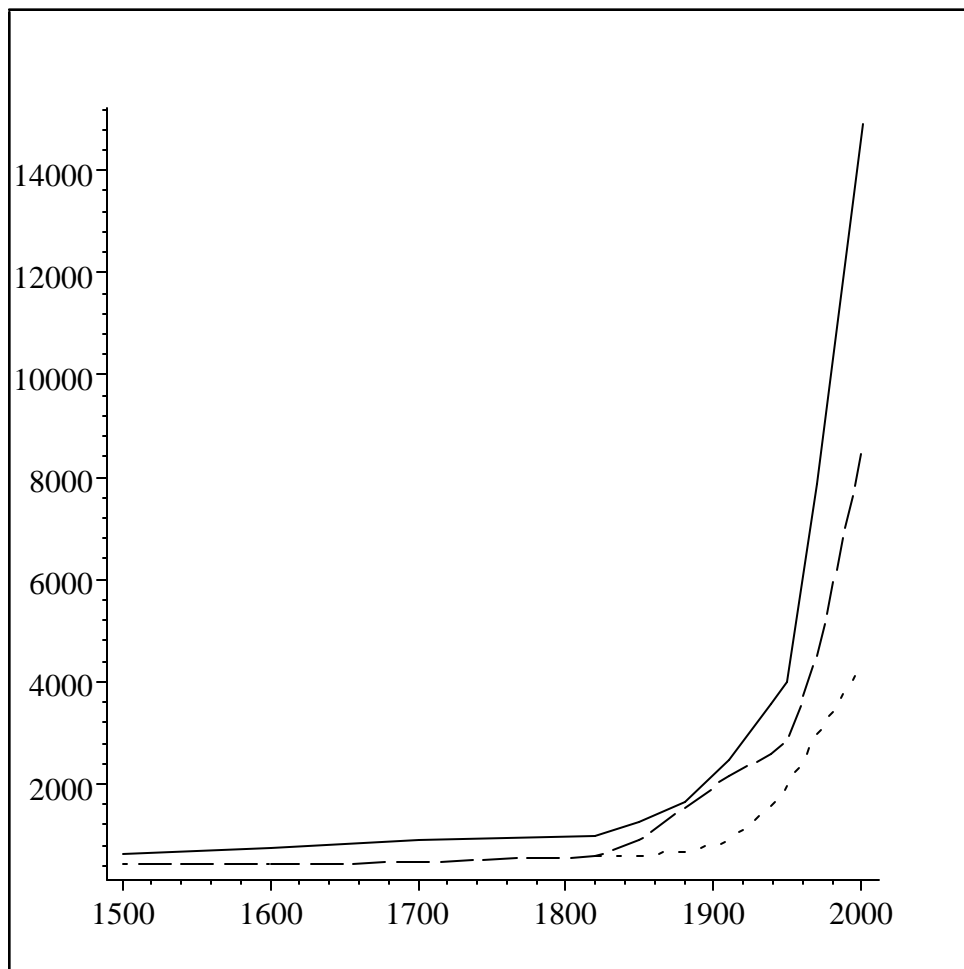|	1500	1700	1820
$Constant$	14090	14538	592
	(4727)	(3093)	(2481)
gdp/cap	7.60	5.01	15.96
	(7.42)	(3.36)	(2.73)
R^2	0.0024	0.0553	0.4803
N	22	22	59

Source: Maddison (2003).

Figure 2: Paths of per capita incomes in non-tropical (solid line), semi-tropical (dashed), and tropical areas (dots).

Table 2: Per capita income by period and land area in the tropics.

Year	Not tropical	Part tropical	Tropical
1500	641(19)	425(3)	*n.a.* (0)
1600	767(19)	425(3)	*n.a.* (0)
1700	898(19)	467(3)	*n.a.* (0)
1820	982(23)	590(8)	602(7)
1850	1269(22)	905(8)	612(7)
1880	1668(26)	1545(8)	680(7)
1910	2481(28)	2136(11)	877(11)
1939	3577(28)	2587(11)	1570(17)
1950	3976(30)	2840(13)	1952(16)
1970	7870(30)	4512(13)	2974(16)
2001	14915(30)	8553(13)	4324(16)

Source: % Land area in geographical tropics, in equal area projection, from Gallup, Mellinguer, and Sachs (2003). Per capita income from Maddison (2003). Numbers of observations in parentheses.

Table 3: Rate of growth of total income, of population, and of per capita income, by quality of institutions.

XCONST	YG	POPG	G	N
1	4.12	2.88	1.25	763
2	5.24	2.66	2.58	316
3	4.13	2.95	1.27	557
4	6.45	2.55	3.89	33
5	4.03	2.33	1.70	192
6	5.37	1.45	3.62	55
7	3.95	2.07	1.87	415
OLS	−0.0431	−0.1446	0.1020	2331
	(0.0809)	(0.0191)	(0.0816)	
2SLS	0.1083	−0.1814	0.1728	2331
	(0.1824)	(0.0539)	(0.1838)	

Note: XCONST from Polity IV. Income and population from PWT5.6, extended by Easterly: www.nyu.edu/fas/institute/dri/Easterly/Research.html.

Table 4: Quality of Institutions By Entry and Exit Years

Entry/Exit	1	2	3	4	5	6	7	All
1	10	5	14	3	5	6	15	58
2	1	3	2	1	0	1	0	8
3	6	5	11	3	8	11	7	51
4	1	0	0	1	1	0	0	3
5	0	2	6	0	6	1	3	18
6	0	0	0	0	0	0	2	2
7	3	3	5	1	4	1	29	46
All	21	18	38	9	24	20	56	186

Note: Quality is measured by XCONST from Polity IV.

(1) *Baseline difference.* The units observed as treated may have had different performance as controls than those that were in fact controls or vice-versa.

(2) *Effect of the treatment on the treated.* The units observed as controls may have performed differently under treatment than those that were actually observed as treated. This will be true of the choice of treatment is even in part driven by its consequences.

(3) *Post-treatment effect.* The problem here is that changing just the treatment and nothing else that affects performance may be impossible.

(4) *Distance effect.* The distance between the conditions under which the actual cases were observed and their closest observed matches under alternative institutions is also a source of bias. Extrapolating outside the range of observations is less reliable than interpolating within it. But even the distances within the observed range matter, particularly in the presence of non-linearities.

(5) *Aggregate effect.* This is a violation of the "stable unit treatment value assumption" (SUTVA), namely, that observations are independent, which implies that realizations of counterfactuals do not alter the values actually observed.