

# Forms of democracy, policy and economic development\*

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## Abstract

The paper combines insights from the recent research programs on constitutions and economic policy, and on history, institutions and growth. Drawing on cross-sectional as well as panel data, it presents new empirical results showing that the *form* of democracy (rather than democracy vs. non-democracy) has important consequences for the adoption of structural policies that promote long-run economic performance. Reforms into parliamentary (as opposed to presidential), proportional (as opposed to majoritarian) and permanent (as opposed to temporary) democracy appear to produce the most growth-promoting policies.

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# 1 Introduction

Research on constitutions and economic policymaking (see e.g., Persson and Tabellini, 2004, 2003) has drawn on earlier theoretical work to empirically uncover systematic and quantitatively important effects of both electoral rules and forms of government on fiscal policy and corruption. But, so far, this research has no more than scratched the surface when it comes to structural policies related to long-run economic performance. Moreover, questions remain regarding the causal interpretations of the empirical findings: the paucity of deep constitutional reforms within the set of democracies has led researchers to estimate the constitutional effects from the cross-sectional variation in the data.

Research on long-run economic development (see e.g., Hall and Jones, 1999, Acemoglu, Johnson, and Robinson, 2001, Rodrik, Subramanian, and Trebbi, 2004) has found clusters of structural policies in regulation and trade to be essential for economic performance; it has also found those structural policies to be systematically shaped by history and geography. (Many papers in this literature use the label “institutions”, where I use “structural policies”, see Section 2 for further discussion). While this work supports the common notion that “good institutions are necessary for successful development”, it remains obscure which deep societal institutions, if any, lead to the adoption of growth-promoting policies. Moreover, this research program, as well, has based its empirical estimates on the cross-sectional patterns in the data. Indeed, the findings have been criticized as ambiguous or fragile by Glaeser et al (2004), among others.

In this paper, I combine insights from these two recent research programs to extend the empirical analysis of the political economy of development. In terms of substance, my main claim is that specific political arrangements – the *form* of democracy, rather than democracy (vs. non-democracy) *per se* – may be one of the missing links between history, current policy and economic development. This claim, in turn, brings together two ideas based on the aforementioned results: (1) If constitutional arrangements indeed shape fiscal policy and corruption, they are likely to be reflected also in the structural policies fostering economic development, such as property-rights preserving regulations and non-protectionist trade policies. (2) If history and culture

indeed shape important societal institutions, they are likely to be reflected in the design of political institutions, such as the form of government or the electoral system.

In terms of methodology, I build further on an approach initiated in Persson (2004) to overcome the stumbling block that we observe few reforms among established democracies deep enough to change their broad constitutional features. By including democracies as well as non-democracies in the sample, I can separate the effects of democracy as such, from the effects of the form of democracy. Specifically, I exploit more than 130 observed switches in and out of democracy in the last forty years, which are associated with different forms of democracy. These reforms allow me to estimate various (multiple) treatment effects from the within-country variation in the outcomes of interest. Giavazzi and Tabellini (2004) also study the time variation around democratic reforms for a number of economic policy outcomes, but do not distinguish different forms of democracy.

The next section of the paper relies on a cross-sectional data set with average outcomes from the 1990s in about 100 countries. I start by describing the structural policy and performance data and reviewing recent work in the macro development literature by a replication – in spirit – of the results in Hall and Jones (1999), and Acemoglu, Johnson, and Robinson (2001). Next, I make an *a priori* case of why we should expect the form of democracy to influence structural policies. Against this backdrop, I introduce a number of constitutional measures as prospective determinants of structural policy. Then, I present a set of instrumental-variable estimates, showing that parliamentary (as opposed to presidential) democracy as well as the age of democracy, have a strong positive impact on economic performance through structural policy. Finally, I show that the estimates of the effects on structural policy hold up when I instrument the form of democracy by birth dates of the current (democratic) constitutions and by the Acemoglu et al measure of settler mortality.

The third section proceeds to a panel data set with annual observations during the period 1960-2000 in up to 140 countries. Here, I present difference-in-difference estimates of the effect on policy outcomes based on exits and entries into different forms of democracy (and a few reforms of existing democracies). Because the cross-sectional results in Section 2 show the age of democracy to be crucial, I distinguish permanent and temporary (reversed in the sample) democratic reforms; in general, permanent reforms have the stronger effect on policy. Reforms introducing parliamentary forms

of government, as in the cross-sectional data, positively affect the liberalization of trade and the protection of private property rights. Now, also reforms into proportional (as opposed to majoritarian) democratic elections have similar, but smaller, effects. Robust effects on economic performance are harder to find. A possible explanation is that reforms into parliamentary democracy (from presidential democracy or non-democracy) not only alter structural policies, but also significantly expand government spending. Indeed, the estimates I obtain from the time-series variation in government spending are close to the estimates obtained by Persson and Tabellini (2003, 2004) from the cross-sectional variation in 80 democracies.

Taken together, the results in the paper constitute a counterexample to the claim in Glaeser et al (2004) that it is hard to find tangible (i.e., rules-based) measures of institutions that are systematically correlated with structural policy measures. The coincidence of the cross-sectional and time-series results suggest that the estimated effects of parliamentary vs. presidential form of democracy reflect not only correlation, but a causal mechanism.

The paper ends with a few remarks on future research.

## **2 History, forms of democracy, structural policy and economic development**

In this section, I will argue that we should expect the form of democracy to influence growth-promoting policies, and that the (cross-sectional) data indeed seems to support this argument. Before doing so, however, I undertake a small exercise of replication to introduce the data on policy and performance, and to illustrate some results in the existing macro development literature. The results from this exercise also serve as a stepping stone for the main argument.

### **2.1 Replicating earlier results**

It is theoretically plausible and intuitively appealing to expect certain government undertakings to be particularly important for long-run economic performance. Those would include the design of a regulatory environment providing wide protection of property rights to promote accumulation of capital, human capital or productive knowledge, and the design of a non-protectionist trade regime permitting appropriate price signals to promote

efficient resource allocation. The recent literature in the macroeconomics of development has indeed focused precisely on such "institutions" (Hall and Jones 1999 use the term "social infrastructure", Acemoglu, Johnson and Robinson, 2004, refer to "economic institutions", while Rodrik, Subramanian and Trebbi, 2004, just say "institutions"). I much prefer the label "structural policy" to emphasize that regulatory and trade regimes result from purposeful collective choices under a set of more fundamental political arrangements (this is in line with the argument in Glaeser et al, 2004).

To make close contact with this recent development literature, I adopt several of the measures used there. Thus, I define a measure of *structural policy*, following Hall and Jones (1999) definition of "social infrastructure". As did these authors, I use an (unweighted) average of two indexes. One (collected by ICRG) refers to government anti-diversion policy, related to the protection of private property rights. The other index (originally constructed by Sachs and Warner, 1995) refers to the number of years with open borders since 1950. The two indexes, and thus the overall *structural policy* index, are normalized to take on values between 0 and 1.<sup>1</sup> Compared to Hall and Jones, however, I consider more recent data, computing an average for annual outcomes in the 1990s rather than a single year in the late 1980s. Furthermore, I do not impute data to replace missing observations. (The Data Appendix expands on sources and definitions of the variables.)

Economic performance is measured in three ways. I mainly consider (the log of) *output per worker*, but I also consider (the log of) *TFP*. The measurement of these variables again follow Hall and Jones, albeit with more recent data and no imputed observations. Finally, I consider *GDP per capita*, obtained directly from the Penn Tables.

The properties of these data might be best illustrated by a scatterplot, which will be familiar to many readers. *Figure 1* shows a strong positive partial correlation between *output per worker* and *structural policy*, when continental location and identity of colonial powers are held constant. The slope of the regression line in the diagram – which is just above 2 – coincides with the coefficient on structural policy in an OLS regression of output per capita (also including the set of continent and colonial history indicators). However, this positive correlation could reflect influences in each or both directions.

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<sup>1</sup>In the next subsection, I argue why it makes sense to look at a broad index of different policies rather than some single policy measure.

To get at the causal relationship between these variables, Hall and Jones (1999) launched the general idea that societies are more likely to pursue growth-promoting structural policies, the more strongly they have been exposed to Western European influence – for historical or geographical reasons. They suggested four instruments for a country’s structural policy: its latitude, its predicted trade from geographical and population characteristics, and its current population shares with a European language, and English, respectively, as their mother tongue.

Table 1 reports the results from similar two-stage estimates of the effect of *structural policy* on *output per worker* (estimating by GMM means allowing for heteroskedasticity of unknown form). When I control for continental location and identity of prior colonial powers, including latitude and the fraction of English speakers in the specification violates the conventional relevance or exogeneity test for instruments. As shown in the table, however, the share of European speakers and more favorable conditions for trade pass these validity tests: the F-statistic in column 1 rejects irrelevance, and the chi-2 statistic in column 2 does not reject exogeneity of one of these instruments. By the first-stage estimates in column 1, these variables are clearly positively associated with better structural policy. And by the second-stage estimates in column 2, structural policy has a large, positive, and precisely estimated effect on performance. According to these estimates, an improvement in structural policy corresponding to one standard deviation in the sample (in a country drawn at random) would raise long-run productivity by about 150%.

Acemoglu, Johnson and Robinson (2001) took this kind of argument further, by using data on settler mortality in 19th century colonies to measure the Western European influence. They argued that investment in “good institutions” was less intensive in places with a more dangerous disease environment, that good institutions are long-lasting, and that they are conducive to good policy (policy, in terms of my labeling conventions) today. What happens when we run the same specification as in columns 1 and 2 in the smaller (about 60 observations) cross section, where the settler mortality data is available? As columns 3 and 4 show, the results from the larger sample hold up, except that the predicted trade share has less influence of structural policies. If anything, the effect of structural policies on productivity is now larger. In columns 5 and 6 of Table 1 (the log of) *settler mortality* is used as the sole instrument for structural policy to more convincingly isolate exogenous variation in policy. The effect of structural policy on performance is

still precisely estimated, and even larger than before.

These kinds of results certainly strengthen the presumption that broad clusters of structural policies on regulation and trade do shape long-run economic performance. But a large question remains: under which specific political arrangements, if any, are we more likely to observe the adoption of more growth-promoting policies? This is the main issue of the present paper, to which I now turn.

## **2.2 Do forms of democracy shape structural policy and economic performance?**

**Preliminaries** Why should the nature of constitutional arrangements, i.e., the form of democracy, systematically influence the adoption of growth-promoting structural policies? From a theoretical perspective, the benefits of property rights regulation for different groups in society depends on the design and enforcement of the underlying legislation. But arguably, the regulation is more conducive to accumulation and growth when its protection of private property rights extends, in a relatively indiscriminating way, to broad groups of the population, rather than to small privileged groups or elites (see Acemoglu, Johnson and Robinson, 2004, for such an argument). A similar argument can be made for the trade regime. Protectionist trade policies support certain sectors, but sectors of production and regions of residence often correlate well, particularly in developing countries. Non-protectionist trade policies thus also tend to yield benefits to broad groups in the population, rather than small groups targeted by sector or region. In regulation as well as trade, stable policies are likely to have a larger effect than unstable policies on accumulation and allocation.

Given this characterization of growth-promoting structural policy in terms of the distribution of their benefits, we obtain some insights from the recent theoretical work on constitutions and fiscal policy. One is that we should expect certain arrangements to better promote policies with broad and stable benefits. Existing models of the form of government predict that the confidence requirement inherent in parliamentary systems helps produce spending programs better serving broad and stable majorities of voters than programs in presidential systems (see Persson, Roland and Tabellini, 2000). Models of alternative electoral rules predict proportional elections to produce policies better serving the interests of broad majorities than do majoritarian elec-

tions, either directly through incentives of politicians, or indirectly via party formation and the incidence of coalition government (see Lizzeri and Persico, 2001, Milesi-Ferretti, Perotti and Rostagno, 2002, Persson and Tabellini, 1999, 2000, and Persson, Roland and Tabellini, 2003). Another insight from the existing literature is that systematic effects of alternative democratic arrangements should not only show up in single programs, but in a number of policy programs. This means that it becomes natural to consider clusters of policies, like the structural policy index considered in the previous subsection.

Motivated by this discussion, I define binary indicators for democracy and its various forms, using the classifications in Persson and Tabellini (2003, 2004). If a country, on average, is coded as at least semi-free by the surveys of Freedom House (its Gastil index is lower than 5), I set the indicator *democracy* = 1.<sup>2</sup> Among democracies, countries are coded as parliamentary (*parliamentary democracy* = 1, *presidential democracy* = 0) if their executive is subject to a confidence agreement, presidential if it is not. And democracies are coded as majoritarian if their elections to the lower house of the legislature rely on plurality rule (*proportional democracy* = 0, *majoritarian democracy* = 1), proportional if they do not.

In addition to these binary indicator variables, I also follow Persson and Tabellini (2003) and measure the age of a country's democracy. The rationale for including this variable among the prospective policy determinants is that the stability argument above may not only apply to policy choices given a set of political institutions, but also to those institutions themselves. The variable *age of democracy* measures the number of years with uninterrupted democratic rule, going backwards from 2000 to 1800, divided by 200. It thus takes on values between 0 and 1 among democracies, and a value of 0 for all non-democracies.

I now ask whether these indicators help explain structural policy and performance. Persson and Tabellini (2003, ch. 7) took some steps towards answering this question. Here, I extend that analysis by using more recent observations, different specifications and estimation techniques and by adding non-democracies to the sample. In Section 3 below, I also consider panel data.

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<sup>2</sup>Another source for the classification of democracies and non-democracies, which I exploit in the panel data considered below, is the PolityIV data set. While the Polity data is better suited for the study of democratic reforms over time, the Freedom House data has broader coverage, as it also includes data on smaller countries not covered by the Polity data.



In the present section, I pose two questions to the cross-sectional data: (i) Are the indicators for constitutional rules significantly related to *structural policy*, while exogenous to *output per worker* (*TFP*, *GDP per capita*) in a statistical sense? (ii) Are the constitutional rules plausible links from history to current policy?

**Constitutions, policy and performance** To answer question (i), I add my constitutional measures as instruments, alongside the two Hall and Jones instruments, in the earlier two-stage specification. In other words, I adopt the specification illustrated in *Figure 2*, where the constitution affects performance only through its effect on structural policy (note that I am still controlling for continental location and identity of colonial rulers). The parliamentary form of government indicator and the age of democracy variable always turn out to be significant determinants of structural policy. As long as these two indicators are included, however, the indicator for presidential democracy (or democracy as such), never significantly influences structural policy. Neither do the indicators for alternative systems of democratic elections.

Results for the full 1990s cross section appear in the first portion of *Table 2*. Columns 1 and 2 show the first and second stage GMM estimates, when the *parliamentary democracy* and *age of democracy* variables are added to the corresponding first-stage specification in column 1 of *Table 1*. Several points about these results are worth noting. The two Hall and Jones instruments retain their explanatory power. But *parliamentary democracy* and *age of democracy* also influence *structural policy* in the expected direction – i.e., towards better policies. These variables also add enough explanatory power to the first stage for the F-statistic for the excluded instruments to remain at the same level as in column 1 of *Table 1* (10.50 vs. 11.01). The estimated effect of structural policy on output per worker in column 2 is also stable relative to its earlier estimate in column 2 of *Table 1* (3.59 vs. 3.81). Due to the overidentification, we can test the specific hypothesis that the two constitutional variables are exogenous to output per worker. The C-statistic (based on the difference between two Sargan statistics) at the bottom of column 2, is safely within the acceptance region.

According to the estimates, the constitutional effects are substantial in magnitude. Under the maintained exogeneity assumption, introducing parliamentary democracy in a non-democracy – or, equivalently (as the presi-

dential democracy indicator is insignificant), in a presidential democracy – improves *structural policy* so as to raise long-run productivity by almost 50%.

Columns 3 and 4 show that we obtain the same qualitative results if output per worker is replaced by *GDP per capita*, or by *TFP*. In the latter case, the estimated coefficient on structural policy is cut in half, which is not surprising given that the TFP measure controls for the accumulation of physical and human capital, whereas the other two do not.

Finally, columns 5 through 8 of the table show that the results hold up equally or more strongly in the smaller settler mortality sample. As in columns 1-2, the estimates in columns 5-6 are comparable to the corresponding estimates in Table 1.

**History, constitutions and policy** At a minimum, the estimates in Table 2 show that the form of democracy is systematically correlated with structural policies (and, indirectly with economic performance). They thus provide a clear counterexample to the argument in Glaeser et al (2004) that it is hard to find any measures of societal institutions, in the forms of tangible rules and procedures which are correlated with current measures of growth-promoting policies. The estimates also suggest that earlier failures to find robust effects of political regimes on economic development (see e.g., Przeworski and Limongi, 1993 for an overview) may have considered too crude measures, namely democracy vs. non-democracy.<sup>3</sup>

The results support the first main idea behind the paper, mentioned in the introduction, namely that the systematic effects on fiscal policy may also extend to other areas of policymaking. But can we interpret the estimates as reflecting a causal mechanism? For doing so, the constitutional variables should not be endogenous to structural policy or economic performance. The statistical arguments for exogeneity in the previous subsection may be reassuring, but they are clearly not enough. It is therefore useful to turn to the other main idea behind the paper, namely that historical forces may indeed exercise their impact on current policies through constitutional arrangements. To shed light on this possibility, I turn to an instrumental-variable strategy based on *a priori* arguments.

Let us make the assumption that Western colonization affects current policies only (mainly) via the form of political institutions. Influence of

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<sup>3</sup>See, however, the recent paper by Papaioannou and Siourounis (2004) for more positive results.

Western colonization on a society should then be reflected in the political arrangements we observe post independence. Specifically, greater Western influence should show up in a greater probability of observing the same type of political arrangements in previous colonies as those observed in Western Europe, i.e., stable parliamentary democracies. Now suppose, in line with Acemoglu et al, that *settler mortality* is a good measure of Western influence. Under the identifying assumption that the influence on current policies operates only via the form of political institutions, *settler mortality* becomes a valid instrument for parliamentary democracy and/or age of democracy.

To achieve overidentification, I also use two additional historical instruments for constitutional arrangements. These are constructed and motivated in detail by Persson and Tabellini (2003, 2004). They exploit different worldwide frequencies in the adoption of alternative constitutional features during different historical periods. Specifically, the indicator *constitutional birth 51-80* is set to 1 if a country is democratic and its present electoral rule or form of government was introduced in the period 1951-80, and to 0 otherwise; *constitutional birth 21-50* is defined accordingly with regard to the period 1921-50.

Based on these arguments I adopt an estimation strategy that delves deeper than the first-stage estimates in Table 2, by treating political arrangements as endogenous when assessing their impact on current policies. The strategy is illustrated in Figure 3. I still control for the identity of the colonizing power and continental location, and allow the share of European speakers and the predicted trade share to influence both structural policy and constitutional arrangements.

The two-stage (GMM) estimates are displayed in Table 3. In the first two columns, I treat *parliamentary democracy* as endogenous, and *age of democracy* as exogenous. As expected a priori, the likelihood of parliamentary democracy is reduced by less Western influence, i.e., higher *settler mortality*, and raised by constitutional birth in the early post-war period. A change in the form of government is estimated to have about double the effect on structural policy as the one estimated in Table 2. Moreover, by the chi-2 statistic at the bottom of column 2, we cannot reject the assumption that *settler mortality* only affects structural policy indirectly, through *parliamentary democracy*. In columns 3 and 4, the roles of *age of democracy* and *parliamentary democracy* are reversed. The results are similar: more Western influence improves structural policy by raising the age of democracy, and we cannot reject that this indirect effect is the only effect. In the last

two columns of Table 3, I treat both constitutional features as endogenous. Here, I add the second of the constitutional birth variables to obtain separate identification of the two endogenous variables and to maintain overidentification. While this last specification asks quite a bit of the data, the effect on structural policy of parliamentary democracy appears robust.

The cross-sectional results in this section are certainly suggestive, but skeptics may still suspect them to be driven by unobservable (and thus omitted) country-characteristics, or some other statistical artefact. I will address some of these concerns in the next section by estimating how the form of democracy affects policy exclusively from the within-country variation associated with democratic reforms. The result that older democracies have better policies suggests that it may be important to distinguish democratic reforms not only by constitutional arrangement but also by duration.

### 3 Democratic reforms and policy changes

I now turn to a panel data set to estimate how different types of democratic reforms alter economic policy outcomes, and economic performance. Before going through the results, I describe this data set and my empirical strategy.

#### 3.1 Data and empirical strategy

**Reforms in the post-war panel** The data set collects annual observations of political and economic variables from 1960 to 2000 for as many countries in the world as possible. Due to data availability and the formation of new countries, the resulting panel is unbalanced – some of the estimates are still based on data from nearly 140 countries. As already mentioned in the introduction, and discussed at length in Persson and Tabellini (2003), reforms broad enough to change the main constitutional features are very rare among existing democracies (according to the definition below). This is evident from the list of such reforms in *Table 4 (a)*. In fact, only two countries in the panel changed status from a parliamentary to a presidential form of government, namely Cyprus in 1970 and Sri Lanka in 1978. Switches between majoritarian and proportional elections with maintained democratic status occur in about 15 cases, including France in 1986 and 1988, Japan in 1994, New Zealand in 1996, and South Africa in 1994. But these electoral reforms are still too few to permit meaningful estimation of the time variation

associated with reform.

Therefore, I also exploit switches of democratic status to study the effect of alternative forms of democracy. A first requirement is a precise classification of countries and years into democratic and non-democratic. In the panel, I rely on the Polity2 index, included in the 4th wave of the Polity data set, which is collected for all independent nations with more than 1/2 million inhabitants.<sup>4</sup> The Polity2 index takes on values from -10 to +10 depending on a variety of institutional features ranging from constraints on the executive to the openness of elections. I code a country as democratic in any year when this index takes on a (strictly) positive value: i.e., I set the binary indicator  $democracy = 1$ .

By this classification, a reform occurs in a year of the panel when a nation's *democracy* indicator flips between 0 and 1 or vice versa. (Note that a country that becomes independent within the sample period is coded as missing before independence, so a reform only occurs if democratic status changes at some point after the year of independence.) This definition of reform may appear quite arbitrary and based on marginal changes in democratic status. In fact, most reforms accord with conventional views on political history. They also appear to represent major institutional change: the reforms involve an average jump of 8.5 on the 21-step Polity2 scale (with a standard deviation of 4.1). To be meaningfully used in the before-after analysis to follow, I require that the outcome of interest be observed for at least two years before and after the reform.

Given this exclusion of the two first and last years, the maximal number of switches in and out of democracy I can exploit in the sample are 132. Missing observations of the variables of interest may cut the number further, but the typical regression below still incorporates well beyond 100 such reform episodes. Columns 1 and 2 of *Table 4* (b) lists the countries and years of usable reforms. As shown by column 3 of the table, 55 reforms represent exits out of democracy, while 77 represent entries into democracy. The next two columns classify the reforms with regard to the form of democracy, re-

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<sup>4</sup>The 1/2 million limit of the Polity data excludes some small nations included in the Freedom House data used in the previous section. On the other hand, different waves of the Polity data set update the entire data set (back to 1800) as definitions change, which makes it more suitable for comparisons over time than the Freedom House data (where there are no such updates from each yearly coding to the next).

lying on exactly the same definitions as in the previous section. Of the 131 episodes with available data (Benin in the early 1960s is hard to classify), 52 involve reforms of parliamentary democracies and 79 reforms of presidential democracies. With regard to the electoral system, 67 reforms concern proportional and 64 majoritarian democracies. As is evident from the table, some countries go through more than one reform. The form of democracy almost always stays constant across these intermittent periods of democracy, however, with the exception of six African countries (Ghana, Kenya, Nigeria, Sierra Leone, Sudan, and Uganda) that start out as parliamentary democracies after independence, and later on reappear as presidential democracies.

I am not only interested in the design of the political system, but also in the effects of reversible vs. irreversible reforms. To this end, I will define permanent democracy as the result of reforms within the sample period, which are not reversed before the end of the panel. Temporary democracy is instead defined as any state of democracy that ends in the sample. Of all the reform episodes 82 are associated with the beginning or end of temporary democracy, whereas 50 are associated with the beginning of permanent democracy.

*Table 5* compares the distribution of covariates in reform episodes across each of these classifications. For reforms with exits from (entries into) democracy, the table is based on the pre-reform (post-reform) classification. We see that slightly less than half (frequency 0.46) of the 132 reform country-years occur in the first twenty years of the panel. Reforms into (or out of) parliamentary vs. presidential democracy have basically the same distribution over time, as do reforms into proportional vs. majoritarian democracy. But the table also cautions us that the distribution of democratic reforms across continents may not be random. In particular, reforms involving presidential and proportional democracy are over-represented in Latin America<sup>5</sup>, reforms into majoritarian democracy are over-represented in Africa, while reforms into parliamentary and majoritarian democracy are over-represented in Asia.

The table also compares the reform episodes associated with permanent and temporary democracies. Naturally, early reforms are more common among episodes involving temporary democracy than among those involving

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<sup>5</sup>There are no valid reform episodes introducing parliamentary democracy in Latin America. Some Carabbean countries in the sample are parliamentary democracies, but they belong to the non-reforming group because they became solid democracies directly upon independence in the mid 1960s.

permanent democracy. To some degree, this reflects a right-censoring problem: some reforms classified as permanent will, in fact, be temporary (i.e., reversed after 2000). The incidence of permanent and temporary democracy is relatively evenly distributed, even though Africa and Asia are somewhat over-represented in temporary democracy, while Latin America and the rest of the world are under-represented.

All in all, the incidence of different forms of democracy across continents does not appear to be random across continents. The estimation strategy should thus avoid confounding this continent-specific incidence of reforms and prospective continent-specific trends in the outcome variables of interest.<sup>6</sup>

To exploit these reforms in the analysis to follow, I use the information contained in Table 4 to define a new set of binary indicators, always splitting the country-years when *democracy* = 1 into two groups. To analyze reforms involving different democratic forms of government, I thus create two binary variables: *Parliamentary democracy* is coded to 1 if *democracy* = 1 and the form of government is parliamentary, and is coded to 0 otherwise. And *presidential democracy* is coded to 1 if *democracy* = 1 and the form of government is presidential, and to 0 otherwise. Binary indicators for *proportional democracy*, *majoritarian democracy*, *permanent democracy*, and *temporary democracy* are defined in an analogous manner.

**Econometric specification and concerns** Consider an outcome variable  $y$  in country  $i$  and year  $t$ ,  $y_{it}$ . A basic econometric specification can be written as:

$$y_{it} = \sum_{f=1}^F \beta^f D_{it}^f + \alpha_i + v_t + u_{it} , \quad (1)$$

where the  $D_{it}^f$  are the binary indicator variables for a subset of the different forms of democracy discussed above  $f = 1, \dots, F$ . Thus, the parameters

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<sup>6</sup>In the estimates reported below, countries that did not undergo a democratic reform in the sample period constitute a control group throughout the panel, whether they stayed democratic or non-democratic throughout. Compared to the countries undergoing at least one reform, the non-reforming countries on average are richer (but also display more income dispersion, 15-30% higher standard errors). Moreover, Latin America and Asia are over-represented in the reform group, whereas the Western world (Europe, North America, Oceania) is over-represented in the non-reform group. These differences in observable covariates indicate that non-observable features may also differ across the two groups; it is thus important to take account of fixed country characteristics in the analysis.

of interest are the coefficients  $\beta^f$ . Because the specification in (1) includes fixed country effects ( $\alpha_i$ ), these "treatment effects" are estimated only from the within-country variation around reforms (changes in  $D_{it}^f$  between 0 and 1, or vice versa). Because it also includes fixed year effects ( $v_t$ ), we obtain difference-in-difference estimates: i.e., for countries undertaking a particular type of reform, we compare the change in  $y$  (post-reform minus pre-reform) with the change in those countries that do not reform (those  $i$  that have either  $D_{it}^f = 1$  or  $D_{it}^f = 0$ ) over the same period.

This specification addresses some problems that may plague cross-sectional estimates, by holding constant unobserved sources of country variation in  $y$ , which are constant across time, and unobserved sources of time variation in  $y$ , which are common across countries. Nevertheless, the coefficients  $\beta^f$  do not identify the causal effect of different democratic reforms if countries in the various reform groups have trends in  $y$ , which are different from those in the non-reform group but unrelated to reforms.

To give a concrete example, suppose we have a regional component in measured trade policy, in the course of the sample say, a specific Latin American trend away from protection towards free trade. This would not be a major problem if the distribution of countries in the non-reforming group and the different reforms groups were regionally balanced. But, as we saw in Table 5, Latin-America is over-represented in the reforms involving presidential (and proportional) democracy. To avoid confounding such regional-specific incidence of reform and region-specific trends in policy or performance, I check that the estimates of  $\beta^f$  are robust to adding a set of continent-time interaction terms,  $\sum_c \gamma^c v_t$  (with  $\gamma^c$  denoting indicators for different continents) to the specification in (1).<sup>7</sup>

Another concern about identification also reflects selection into reform on observables, but of a different kind. Reform episodes may not be exogenous to structural policy or economic performance, because the latter systematically deteriorate (or improve) in the years just before observed reforms. In that case, we have a problem analogous to the so-called Ashenfelter's dip in the program evaluation literature. It is hard to predict the direction of this prospective bias *a priori*. While it is plausible that economic crises (and declining performance) may trigger political crises, these may lead to exits

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<sup>7</sup>In the regressions with economic performance measures as dependent variables, I also add an interaction term between years and socialist legal origin. The idea here is to avoid confounding democratic reforms and the output fall following the breakdown of the Soviet Union and the fall of the Iron curtain.



as well as entries with regard to democracy. Nevertheless, we may think that the policy environment is more unstable in countries introducing temporary rather than permanent democratic reforms. When it comes to the other classifications, it is hard to see *a priori* why we should expect reform episodes involving different forms of government or different electoral systems to be associated with different pre-reform changes in structural policies and performance. Anyway, I try to check for evidence of systematic pre-reform changes in the outcome variables amending the specification in (1) with the term  $\sum_f \alpha^f P_{it}^f$ , where  $P_{it}^f$  are indicators for (three) pre-reform years for different type of reforms,  $f = 1, \dots, F$ . As an additional diagnostic, I also study the dynamics before (and after) reforms by plots in the style of event studies (see further below).

A final econometric issue concerns inference, rather than identification. Typically, the policies and performance measures I consider below display quite strong autocorrelation. In these circumstances, the conventional standard errors associated with difference-in-difference estimates can be seriously understated (see Bertrand, Duflo, and Mullainathan, 2004). To guard against incorrect inference, I report not only report the conventional standard errors, but also those obtained by clustering at the country level, thus allowing arbitrary country-specific serial correlation. (This is, arguably, the most conservative way of dealing with the problem discussed by Bertrand et al, 2004.)

## 3.2 Reforms, policies and performance

**Trade liberalization** One of the two components of the *structural policy* index in Section 2 is the number of years since 1950 with an open economy (in the sense of low tariffs, few non-tariff barriers, small black-market premium, few state monopolies and a non-socialist economy). I now consider the annual observations of this openness index, which are available for a large number of countries for the period 1960-2000 thanks to Wacziarg and Welch's (2004) update of Sachs and Warner's (1995) original data set. For a given country  $i$  and year  $t$ , the binary indicator *open* is thus set equal to 1 or 0, depending on whether the five criteria for an open economy are met.

*Table 6* shows the difference-in-difference estimates of reforms into different forms of democracy for this aspect of structural policy. (Estimating the effect of reforms into democracy, as such, generally yields a positive coefficient, but the significance of this effect is not robust to the checks performed in the table.) These estimates are based on data from 132 countries; due to

the formation of new countries (mostly) and missing data, the length of the average panel is 35 years.

Column 1 distinguishes reforms out of non-democracy into (or out of) democracy with a parliamentary and presidential form of government, respectively. Underneath the estimated coefficients are two sets of standard errors and the implied confidence levels: conventional ones in parentheses and clustered (by country) ones in square brackets. The coefficients on both sets of reforms are positive, but that on presidential democracy is lower and only borderline significant. Clearly, it matters which way the standard errors are computed: the clustered errors are three to four times higher than the conventional ones.

Column 2 reports on a specification with the robustness checks discussed in the previous subsection, i.e., continent-year interactions and indicators for the three years preceding each type of reform. Clearly, the positive effect of parliamentary reforms is robust to continent-specific trends in *open*. The estimated effect is also quantitatively important: introducing a parliamentary democracy (in a non-democracy or a presidential democracy) raises the probability of opening the economy by about 25%. There is no robust evidence of pre-reform changes driving the results.

Figure 4 illustrates the estimates in the form of an event-study plot. This figure relies on the residuals from the regression underlying column 2, but leaving out the democracy indicators. Part (a) of the figure plots the (average) residuals associated with entries into and exits out of parliamentary democracy, five years before and after reform. There is no sign of any pre-reform changes in liberalization. Moreover, the positive estimates in the table seem to derive from liberalizations following entries into parliamentary democracy, rather than de-liberalizations following exits from parliamentary democracy. Countries opening up their economies within five years after their entry into parliamentary democracy include not only some of the former communistic states in Eastern Europe (Albania, Bulgaria, Hungary, Poland and Romania), but also Bangladesh, Ethiopia and Nepal. Figure 4 (b) shows a corresponding plot for reforms of presidential democracies. Evidently, the effects of the presidential reforms are smaller and much less systematic.

Columns 3 and 4 of Table 6 report on the same kind of estimates, when reforms are distinguished by electoral rule. Here, the introduction of democracy with proportional elections (or a reform from majoritarian to proportional elections) raises the probability of an opening of the economy, whereas the introduction of majoritarian elections does not. Once more, we see no

evidence of significant pre-reform changes in the trade policy index. The positive effect of proportional elections on openness is about half the estimated effect of a parliamentary form of government – still quite sizable. Because the parliamentary-presidential distinction is independent of the proportional-majoritarian distinction, the specifications in columns 1-4 really assume the effects of the form of government and the electoral rule to be additive. A formal test – based on indicators for the four possible types of democracy – does not reject the hypothesis of additivity (results not shown).

Finally, columns 5 and 6 consider the distinction between permanent and temporary reform. As expected, permanent democratizations significantly raise the probability of liberalizing the economy, whereas temporary democratizations do not. If anything, the latter appear to reduce the probability of an open economy, but the negative estimate is not statistically robust.

**Protection of property rights and *structural policy*** The second component of the structural policy measure in Section 2 is the 1990s average of an index for the protection of property rights, called *GADP*. What are the effect of democratic reforms on regulatory policy in broad terms, as measured by the annual value of this index? The *GADP* data is only available for a mere 16 years, however, namely between 1982 and 1997. Given this short sample period, the distinction between permanent and temporary reforms is not very meaningful, so that the estimates reported in *Table 7* focus on alternative forms of government and electoral rules rather than the reversibility of reform.

Columns 1 and 2 distinguish between parliamentary and presidential democracies. As in the case of trade liberalization, the effect of parliamentary democracy is estimated to be positive, but when evaluated at the clustered standard errors the coefficient is not significantly different from zero. Columns 3 and 4 suggest no discernible effect of the electoral rule.

In columns 5-8 of the table, I carry out the analog of the cross-sectional specification in Section 2. That is, I study the effect of reforms on a broad Hall-Jones-style measure of *structural policy*, defined as the unweighted average of *open* and *GADP*, for the years and countries where both measures are available.<sup>8</sup> Reforms into parliamentary democracy, as well as proportional democracy, have strong significant positive effects on this broad index

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<sup>8</sup>Because of the shorter time span and the stricter data requirements, the number of reforms underlying the regressions in *Table 7* is only about 50.

of structural policy. Evidently, the two dimensions of policy reinforce each other: the estimated coefficients for parliamentary democracy and proportional democracy are higher than the average of the individual coefficients on *open* (in Table 6) and *GADP* (in the first half of Table 7). Interestingly, the difference-in-difference estimate in Table 7 of the effect of parliamentary democracy on *structural policy* just below 0.3. This estimate is close to the IV estimates in Table 3, where the same effect is entirely identified from the cross-country variation, rather than the within-country variation in the data. This coincidence of the findings suggests that the estimates indeed pick up a causal effect of the form of democracy on structural policy.

**Economic performance** I now consider the effects of reforms on economic performance. Table 8 displays estimates of the reduced form effects on *output per worker*; thus, I do not try to identify the policy channels whereby such effects – if any – may come about. The table has the same structure as Tables 6 and 7, beginning with distinctions between alternative forms of government, proceeding via alternative electoral rules to reversibility of reforms. Some of the estimates in the first four columns indicate effects on economic performance in line with the estimated effects on structural policy; i.e., positive effects of reforms introducing parliamentary and proportional democracies. But these effects are not statistically robust. As we include continent-year interactions, consider clustered standard errors, or both, they are no longer significantly different from zero. Repeating the same exercise with *GDP per capita* or *TFP* as outcome variables produces similar half-baked results (not shown).

How can we reconcile these findings with the panel results for structural policy and the overall cross-sectional results? I see two possibilities. One is that the effects on economic performance may only appear with a considerable lag. Not only may the effects of a democratic reform on structural policy take time. The effects of more growth-promoting policies on productivity may also take time, because they operate through better incentives for accumulation and efficient allocation of factors. Since the average democratic reform in the panel occurs after 1980 (recall Table 5), the effects on performance may just not have shown up yet, or at least not shown up fully. The cross-sectional data used in Section 2 may thus allow us to better estimate the long-run effect of democratic reforms on performance, given that many of the democracies in the sample were created before 1960; recall also

the positive effects of the age of democracy on policy and performance. In the difference-in-difference analysis in this section, these older democracies always belong to the control group of the panel.

The second possibility is that the form of democracy shapes not only structural policies but also other policies, which affect economic performance in the opposite direction. In particular, the literature on constitutions and fiscal policy discussed earlier in the paper has found strong support for the theoretical prediction that parliamentary democracies have larger governments than presidential democracies. If a large government sector has a negative effect on economic performance, this may explain the inconclusive effect on output per worker of parliamentary democracy, despite a favorable effect on structural policy. Before concluding the paper, I briefly turn to this possibility.

**Government spending** For the time period 1960-2000 and the broad set of countries in the panel, the most widely available measure of government spending is probably total government consumption (expressed as a percentage of GDP) from the Penn World Tables. *Table 9* displays estimates of how democratic reforms of different forms impinge on the size of government, measured in that way. As columns 3-6 show, neither the electoral rule nor the reversibility of reform appear to systematically affect government consumption. The estimates in columns 1 and 2 are more intriguing, however. A reform that establishes parliamentary democracy in a non-democracy raises government spending by about 2.5% of GDP and this effect is quite precisely estimated. As illustrated by the event-study plot in *Figure 5*, this effect reflects hikes in spending following entry into parliamentary democracy, as well as drops in spending following exits from parliamentary democracy. Additional results (not shown) similarly indicate that this form of democracy creates pressure for more spending: analogous difference-in-difference estimates show that reforms introducing parliamentary democracy are also followed by systematic increases in government deficits and inflation.

It is instructive to compare these estimates with those in Persson and Tabellini (2003, 2004), who only considered cross-sectional data for a set of democracies and used a very different data source (IFS data from the IMF for total expenditures by central governments). They estimated that a reform from presidential to parliamentary democracy raises spending by about 5% of GDP. This is not far from the estimate of 3.3% obtained here,

when subtracting the coefficient on presidential democracy from that on parliamentary democracy in the most general specification of column 2. This coincidence of estimates is remarkable in its own right. But from the viewpoint of the present paper, it is consistent with the second suggestion why we may fail to find a robust positive effect of parliamentary democracy on economic performance.

## 4 Final remarks

The empirical results in this paper suggest that political arrangements constitute a link in the chain from history to current policy and performance. In particular, cross-sectional as well as panel data show the form of democracy to be critical for the design of trade and regulatory regimes. Reforms of authoritarian political regimes into parliamentary, proportional and permanent democracies seem to foster the adoption of more growth-promoting structural policies, whereas reforms into presidential, majoritarian and temporary democracy do not.

Further theoretical work should model the adoption of structural policy under alternative forms of democracy not only to rationalize the empirical findings, but also to generate more insight into the interplay between democratic arrangements, policy and economic performance. Further empirical work should take advantage of longer-term data, exploring whether the results of pre-1960 democratic reforms corroborate those of post-1960 reforms. More generally, building a bridge between the recent work in political economics and development economics, may teach us important lessons about growth-enhancing institutional reforms.

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## Data Appendix

Variables are assembled for all independent countries on an annual basis during the period 1960 to 2000, whenever available. Unless stated otherwise, the panel data set just collects the annual observations for each country, while the cross-sectional data set collects their average values in the 1990s for each country.

### **Economic performance**

*GDP per capita*: Log of real GDP per capita. Source: Penn World Tables 6.1

*Output per worker*: As in Hall and Jones (1999), (the log of ) GDP divided by the workforce. Source: Penn World Tables 6.1.

*TFP*: Log of total factor productivity. Constructed as in Hall and Jones (1999), by adjusting (the log of) *output per worker* for physical capital (constructed from investment data by perpetual inventory method and imposing common depreciation and capital shares) and human capital (years of schooling in the population above 25, interpolating between 5-year observations). Sources: Penn World Tables 6.1 and Barro-Lee data set.

### **Economic policy**

*GADP*: Average of five different subjective perception indexes concerning (i) repudiation of government contracts, (ii) expropriation risk, (iii) corruption, (iv) rule of law, and (v) bureaucratic quality. Normalized between 0 and 1. Source: IRIS-3 data set.

*Open*: Binary (0,1) indicator for openness of the economy in a given year. Coded open only if all of Sachs and Warner's (1995) five criteria are fulfilled, namely (i) average tariffs below 40%, (ii) non-tariff barriers in less than 40% of sectors, (iii) black market premium of the exchange rate less than 20%, (iv) no state monopoly on major exports, (v) socialist economic system. Sources: Easterly, Levine and Roodman (2004) and Wacziarg and Welch (2004).

*Years open*: Average of *open* during 1950-1998.

*Structural policy*: in the panel data set this is an unweighted average of *GADP* and *open*, year by year; in the cross-sectional data set it is unweighted average of *years open* and the 1990s average of *GADP*.

*Government consumption*: total government consumption as a percentage of GDP. Source: Penn World Tables 6.1

## Forms of democracy and political institutions

*Democracy*: Binary (0,1) indicator of democratic rule.

In the cross-sectional data set, *democracy* =1 requires an average value in the 1990s less than 5 of the Gastil index. This is an average of indexes for civil liberties and political rights, where each index is measured on one-to-seven scale with one representing the highest degree of freedom and seven the lowest. Countries with scores between 1.0 and 2.5 are designated "free", between 3.0 and 5.5 "partly free" and between 5.5 and 7.0 "not free". Source: Freedom House, Annual Survey of Freedom Country Ratings.

In the panel data set, *democracy* =1 in a given year requires a (strictly) positive value of the Polity2 index. This index adds a number of indicators regarding the selection of and checks and balances on the executive, and the openness and competitiveness of elections; it ranges from +10 (strongly democratic) to -10 (strongly autocratic). Source: Polity IV Project (<http://www.cidcm.umd.edu/inscr/polity/index.htm>).

*Age of democracy*: Defined (in the cross section) as  $age\ of\ democracy = (2000 - dem\_age)/200$ , where *dem\_age* is the first year of democratic rule, corresponding to the first year of an uninterrupted string of positive yearly values of the Polity2 index until the end of the sample, given that the country was also an independent nation (foreign occupation during WWII not counted as an interruption of democracy). Sources: PolityIV Project and Persson and Tabellini (2003).

*Parliamentary democracy*: Binary indicator of parliamentary form of government among countries with *democracy* = 1. Coded as 1 if the confidence of the legislative assembly is necessary for the survival of the executive (even if an elected president is chief executive). Source: Persson and Tabellini (2003).

*Presidential democracy*: Binary indicator for presidential form of government. Coded as 1 if *democracy* = 1 and *parliamentary democracy* = 0. Source: Persson and Tabellini (2003).

*Proportional democracy*: Binary indicator of proportional elections among countries with *democracy* =1. Coded as 1 if the legislative assembly (lower house) is not elected with plurality or majority rule. Source: Persson and Tabellini (2003).

*Majoritarian democracy*: Binary indicator for majoritarian elections. Coded as 1 if *democracy* = 1 and *proportional democracy* = 0. Source: Persson and Tabellini (2003).

*Permanent democracy*: Binary indicator (in the panel only) for a year that belongs to a democratic spell ( $democracy = 1$ ) beginning strictly after 1960 (or strictly after the date of independence, if after 1960) and continuing without interruption until 2000.

*Temporary democracy*: Binary indicator (in the panel only) for a year that belongs to a democratic spell ending before 2000.

*Constitutional birth 1921-50 and 1951-80*: Binary indicators (cross section only) for the age of the current constitution among democracies. Coded as 1 if  $democracy = 1$  and the present electoral system or form of government was established in the periods between 1921 and 1950, or 1951 and 1980, respectively. Source: Persson and Tabellini (2003).

### **Other country characteristics**

*Continental location*: Binary indicators for (East) Asia, Africa and Latin America. Used directly in the cross-sectional specifications and interacted with year dummies in the panel specifications.

*Colonial origin*: Binary indicators for British, French Spanish-Portuguese, and Other colonizers. In cross-sectional specifications, these binary indicators are discounted by the factor  $(250 - \textit{years since independence})/250$ . In the panel specifications they are interacted with year dummies. Source: Persson and Tabellini (2003)

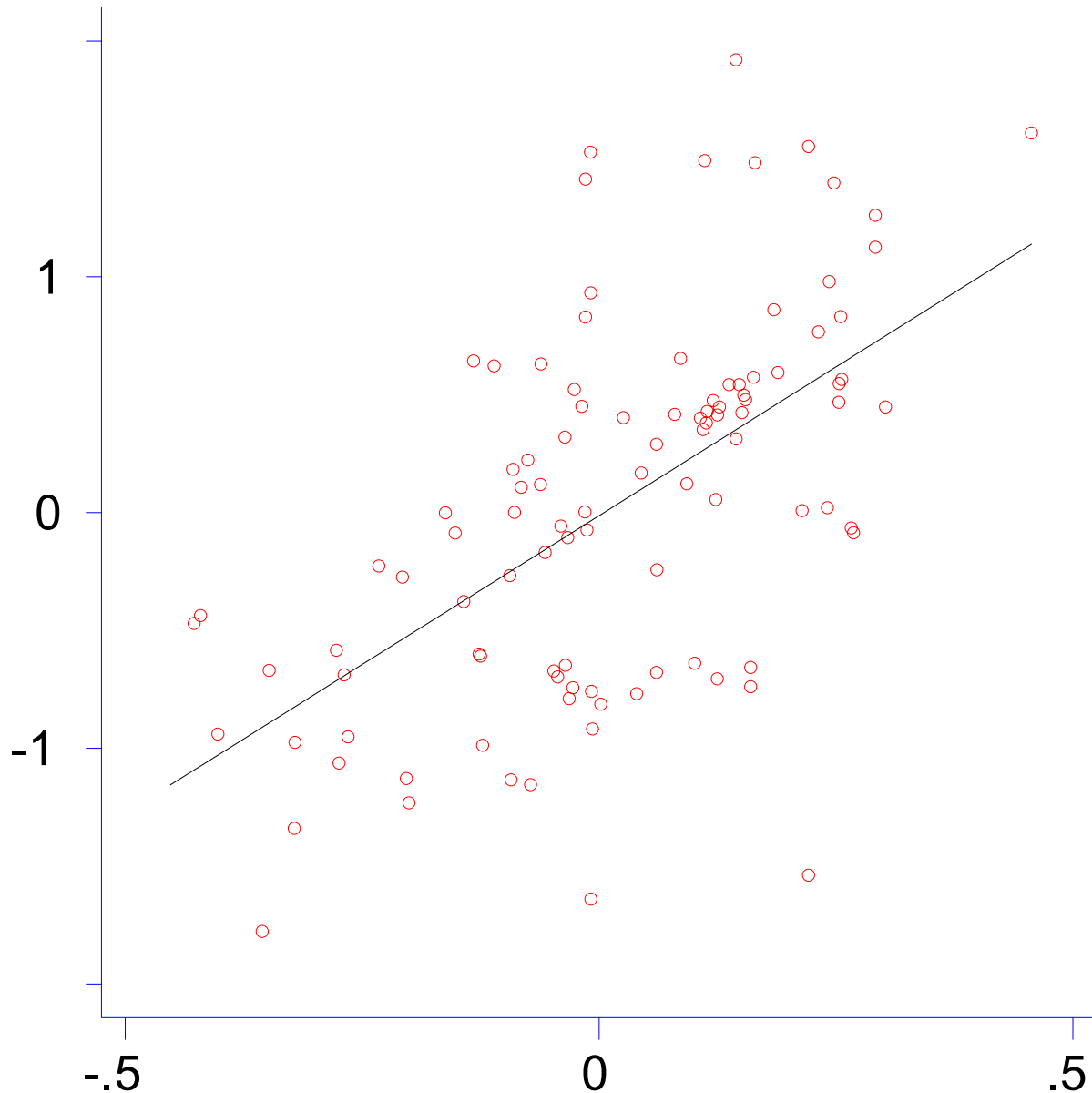
*Socialist legal origin*: Binary indicator for socialist legal origin, interacted with year dummies in some of the panel specifications. Source: La Porta et al (1998).

*Share of European speakers*: The fraction of the population (in the cross-section only) speaking one of the major languages of Western Europe: English, French, German, Portuguese, or Spanish. Source: Hall and Jones (1999).

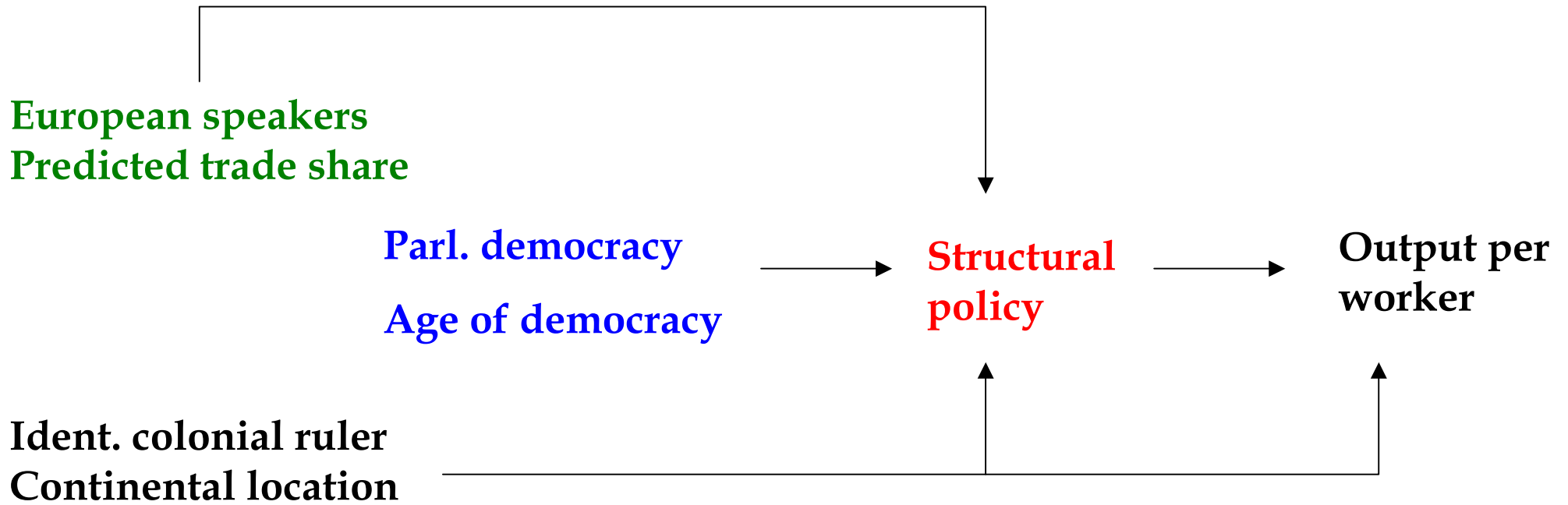
*Predicted trade share*: Log of the Frankel-Romer forecasted trade share (in the cross-section only), derived from a gravity model of international trade that only takes into account country population and geographical features. Source: Hall and Jones (1999)

*Settler mortality*: Log of mortality rate among non-military settlers in Western European colonies in the early 1800s. Source: Acemoglu, Johnson and Robinson (2001).

Residuals of *output/worker*



Residuals of *structural policy*

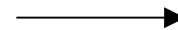


**Figure 2**  
**Do constitutions shape structural policy ?**

Settler mortality  
Constitutional birth



Parl. democracy  
Age of democracy



Structural  
policy

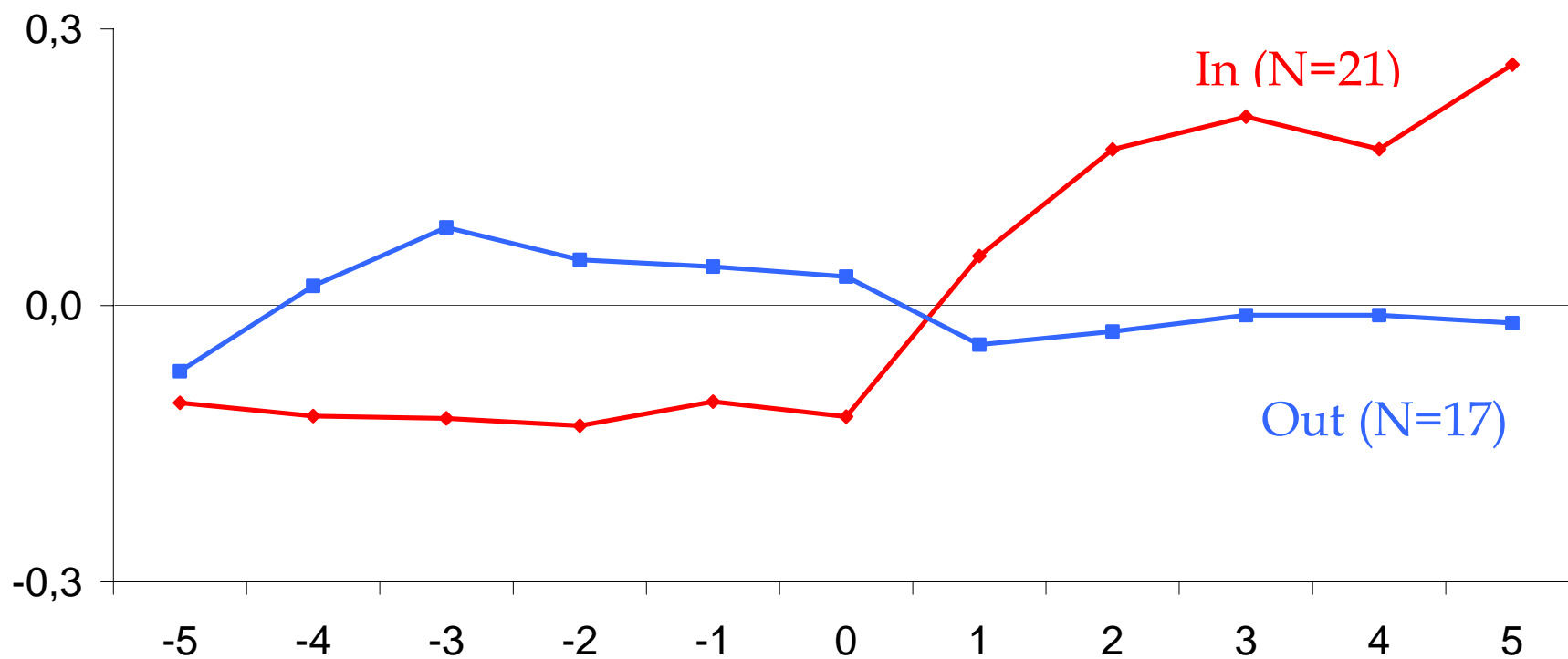
Ident. colonial ruler  
Continental location  
European speakers  
Predicted trade share



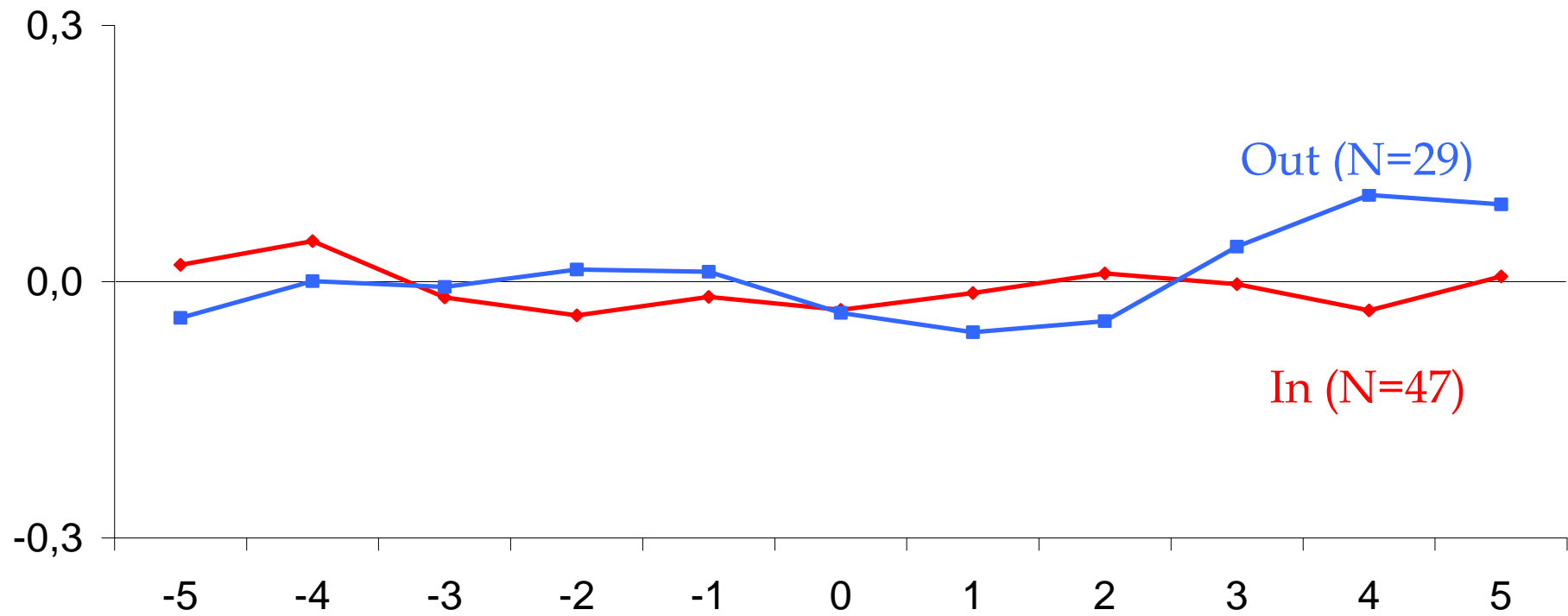
Figure 3

Do constitutions link history to current policy ?

**Figure 4**  
**(a) Parliamentary reforms and openness**  
**Residuals 5 years before/after reform**

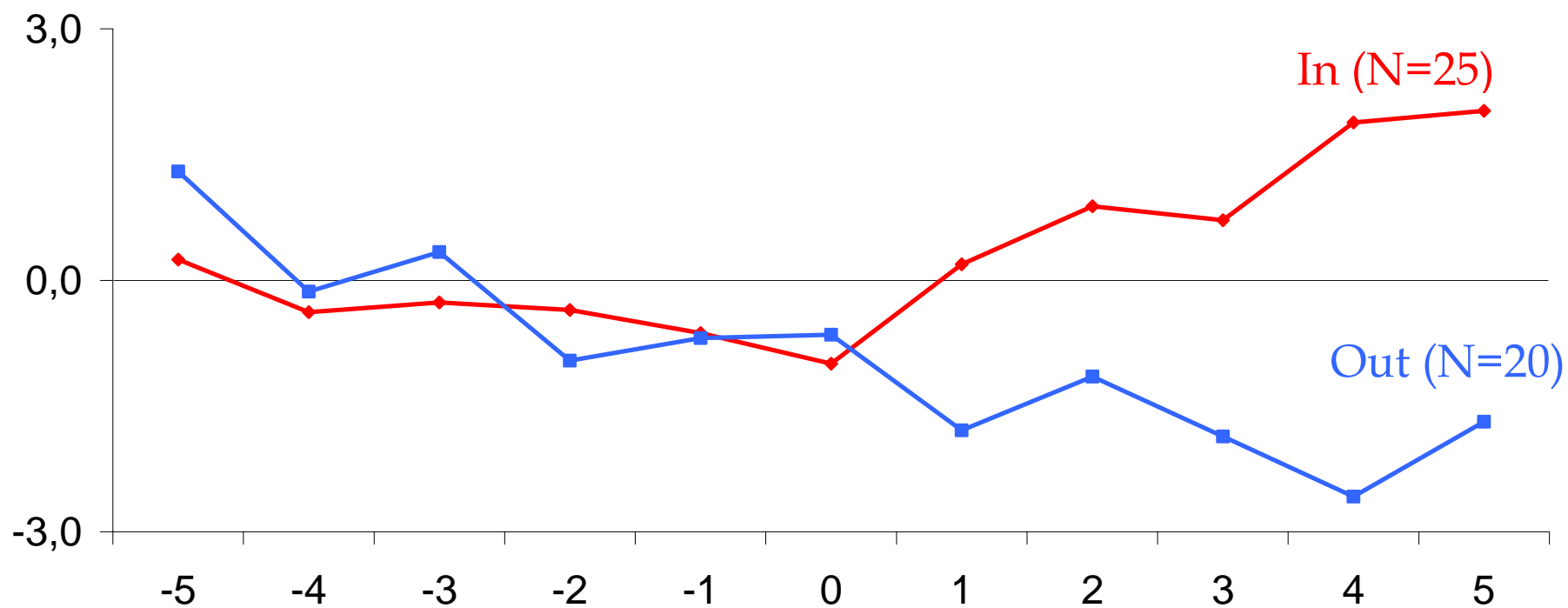


**Figure 4**  
**(b) Presidential reforms and openness**  
**Residuals 5 years before/after reform**





**Figure 5**  
**Parliamentary reforms and government consumption**  
**Residuals 5 years before/after reform**



**Table 1 History, structural policy and economic performance**

Cross section	Full data set		Settler mortality data set			
Dependent variable	<i>structural policy</i>	<i>output per worker</i>	<i>structural policy</i>	<i>output per worker</i>	<i>structural policy</i>	<i>output per worker</i>
<i>structural policy</i>		3.81*** (0.66)		5.17*** (1.06)		9.00*** (2.57)
<i>European speakers</i>	0.21*** (0.06)		0.33*** (0.09)			
<i>predicted trade share</i>	0.09*** (0.03)		0.05 (0.04)			
<i>settler mortality</i>					-0.06** (0.02)	
Excluded instruments F	11.01***		6.82***		5.98**	
Over-identification chi-2 (df)		2.39(1)		0.61(1)		
Number of observations	98	98	58	58	59	59

Instrumental variable estimation by GMM, allowing for heteroskedasticity of unknown form.

Robust standard errors in brackets; \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10%.

Second stage includes *structural policy* (endogenous) and indicators for earlier colonial powers (Britain, France, Spain-Portugal, Other) and continents (Latin Am., Africa, Asia). First stage includes the same seven indicators, and instruments as shown in cols 1, 3 and 5.

Excluded instruments F is the test statistic of the hypothesis that the instruments do not belong to the first-stage regression.

Over-identification chi-2(df) is the Hansen J statistic of the over-identifying restriction that one of the instruments does not influence *output per worker*, other than through *structural policy*; critical value (cols 2 and 4) at 5% is 3.84.

**Table 2 Forms of democracy, structural policy and economic performance**

Cross section	Full data set				Settler mortality data set			
Dependent variable	<i>structural policy</i>	<i>output per worker</i>	<i>GDP per capita</i>	<i>TFP</i>	<i>structural policy</i>	<i>output per worker</i>	<i>GDP per capita</i>	<i>TFP</i>
<i>structural policy</i>		3.59*** (0.64)	3.91*** (0.70)	1.88*** (0.39)		4.78*** (0.86)	5.22*** (0.85)	2.05*** (0.56)
<i>parliamentary democracy</i>	0.10** (0.05)				0.17** (0.08)			
<i>age of democracy</i>	0.32*** (0.09)				0.34** (0.15)			
<i>European speakers</i>	0.13** (0.06)				0.20* (0.08)			
<i>predicted trade share</i>	0.08*** (0.02)				0.01 (0.03)			
Excluded instruments F	10.50***			12.68***	5.88***			5.11**
Over-identification chi-2(df)		0.68(2)	0.89(2)	1.34(2)		1.39(2)	1.64(2)	0.98(2)
Number of observations	97	97	97	86	57	57	57	48

Instrumental variable estimation by GMM, allowing for heteroskedasticity of unknown form.

Robust standard errors in brackets; \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10%.

Second stage in cols 2-4 and 6-8 includes *structural policy* (endogenous) and indicators for earlier colonial powers (Britain, France, Spain-Portugal, Other) and continents (Latin Am., Africa, Asia).. First stage includes exogenous second-stage variables and instruments as shown in cols 1, and 4.

Excluded instruments F is the test statistic of the hypothesis that the instruments do not belong to the first-stage regression.

Over-identification chi-2(df) is the C statistic for the over-identifying restriction for the two constitutional instruments; critical value at 5% is 5.99.

**Table 3 History, forms of democracy and structural policy**

Dependent variable	<i>parliamentary democracy</i>	<i>structural policy</i>	<i>age of democracy</i>	<i>structural policy</i>	<i>parliamentary democracy</i>	<i>age of democracy</i>	<i>structural policy</i>
<i>parliamentary democracy</i>		0.31** (0.16)		0.18** (0.08)			0.49*** (0.19)
<i>age of democracy</i>		0.38*** (0.14)		0.64** (0.30)			-0.05 (0.25)
<i>settler mortality</i>	-0.10** (0.04)		-0.05** (0.02)		-0.08** (0.04)	-0.04* (0.02)	
<i>constitutional birth 51-80</i>	0.25** (0.11)		0.15*** (0.06)		0.23** (0.11)	0.07 (0.05)	
<i>constitutional birth 21-50</i>					-0.26 (0.17)	0.26*** (0.09)	
Excluded instruments F	4.75**		4.73**		2.96**	5.61***	
Over-identification chi-2(df)		0.31(1)		0.58(1)			0.03(1)
Number of observations	57	57	57	57	57	57	57

Instrumental variable estimation by GMM, allowing for heteroskedasticity of unknown form.

Robust standard errors in brackets; \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10%.

Second stage in cols 2, 4 and 7 includes *structural policy* (endogenous) and indicators for earlier colonial powers (Britain, France, Spain-Portugal, Other) and continents (Latin Am., Africa, Asia) plus *European speakers* and *predicted trade share*. First stage includes exogenous second-stage variables and instruments as shown in cols 1, 3, 5 and 6. Excluded instruments F is the test statistic of the hypothesis that the instruments do not belong to the first-stage regression.

Over-identification chi-2(df) is the J statistic for the over-identifying restriction that settler mortality affects structural policy only through the form of democracy; critical value at 5% is 3.84.

## Table 4 Reform episodes 1962-1998

### (a) Reforms in existing democracies

<i>Country</i>	<i>Reform</i>	<i>Type of reform</i>
Albania	1992	Elections: majoritarian to proportional
Armenia	1995	Elections: majoritarian to proportional
Cyprus	1970	Government: parliamentary to presidential
Cyprus	1970	Elections: proportional to majoritarian
Cyprus	1981	Elections: majoritarian to proportional
Fiji	1994	Elections: proportional to majoritarian
France	1986	Elections: majoritarian to proportional
France	1988	Elections: proportional to majoritarian
Japan	1994	Elections: majoritarian to proportional
Macedonia	1998	Elections: majoritarian to proportional
Moldova	1993	Elections: majoritarian to proportional
New Zealand	1996	Elections: majoritarian to proportional
Philippines	1998	Elections: majoritarian to proportional
South Africa	1994	Elections: majoritarian to proportional
Sri Lanka	1978	Elections: majoritarian to proportional
Sri Lanka	1978	Government: parliamentary to presidential
Ukraine	1998	Elections: majoritarian to proportional
Zimbabwe	1985	Elections: proportional to majoritarian

## (b) Exits and entries in different forms of democracy

<i>Country</i>	<i>Year</i>	<i>Entry or exit</i>	<i>Form of government</i>	<i>Form of elections</i>
Albania	1990	entry	Parliamentary	Majoritarian
Albania	1996	exit	Parliamentary	Proportional
Albania	1997	entry	Parliamentary	Proportional
Argentina	1973	entry	Presidential	Proportional
Argentina	1976	exit	Presidential	Proportional
Argentina	1983	entry	Presidential	Proportional
Armenia	1996	exit	Presidential	Proportional
Armenia	1998	entry	Presidential	Proportional
Azerbaijan	1993	exit	Presidential	Majoritarian
Bangladesh	1974	exit	Parliamentary	Majoritarian
Bangladesh	1991	entry	Parliamentary	Majoritarian
Belarus	1995	exit	Presidential	Majoritarian
Benin	1963	exit		
Benin	1991	entry	Presidential	Proportional
Bolivia	1982	entry	Presidential	Proportional
Brazil	1964	exit	Presidential	Proportional
Brazil	1985	entry	Presidential	Proportional
Bulgaria	1990	entry	Parliamentary	Proportional
Burkina Faso	1977	entry	Presidential	Proportional
Burkina Faso	1980	exit	Presidential	Proportional
Cambodia	1990	entry	Parliamentary	Proportional
Cambodia	1997	exit	Parliamentary	Proportional
Cambodia	1998	entry	Parliamentary	Proportional
Central African Rep.	1993	entry	Presidential	Majoritarian
Chile	1973	exit	Presidential	Majoritarian
Chile	1989	entry	Presidential	Majoritarian
Comoros	1990	entry	Parliamentary	Majoritarian
Comoros	1995	exit	Parliamentary	Majoritarian
Comoros	1996	entry	Parliamentary	Majoritarian
Cyprus	1963	exit	Parliamentary	Proportional
Cyprus	1968	entry	Parliamentary	Proportional
Dominican Republic	1962	entry	Presidential	Proportional
Dominican Republic	1964	exit	Presidential	Proportional
Dominican Republic	1978	entry	Presidential	Proportional
Ecuador	1968	entry	Presidential	Proportional
Ecuador	1970	exit	Presidential	Proportional
Ecuador	1979	entry	Presidential	Proportional
El Salvador	1982	entry	Presidential	Proportional
Ethiopia	1993	entry	Parliamentary	Majoritarian
Fiji	1987	exit	Parliamentary	Proportional
Fiji	1990	entry	Parliamentary	Proportional
Gambia	1994	exit	Presidential	Majoritarian
Ghana	1970	entry	Parliamentary	Majoritarian
Ghana	1972	exit	Parliamentary	Majoritarian
Ghana	1979	entry	Presidential	Majoritarian

Ghana	1981	exit	Presidential	Majoritarian
Ghana	1996	entry	Presidential	Majoritarian
Greece	1967	exit	Parliamentary	Proportional
Greece	1974	entry	Parliamentary	Proportional
Guatemala	1966	entry	Presidential	Proportional
Guatemala	1974	exit	Presidential	Proportional
Guatemala	1986	entry	Presidential	Proportional
Guinea-Bissau	1994	entry	Presidential	Proportional
Guinea-Bissau	1998	exit	Presidential	Proportional
Guyana	1978	exit	Presidential	Proportional
Guyana	1992	entry	Presidential	Proportional
Haiti	1990	entry	Presidential	Majoritarian
Haiti	1991	exit	Presidential	Majoritarian
Haiti	1994	entry	Presidential	Majoritarian
Honduras	1980	entry	Presidential	Proportional
Hungary	1989	entry	Parliamentary	Majoritarian
Iran	1997	entry	Presidential	Majoritarian
Kenya	1966	exit	Parliamentary	Majoritarian
Lebanon	1975	exit	Parliamentary	Proportional
Lesotho	1970	exit	Parliamentary	Majoritarian
Lesotho	1993	entry	Parliamentary	Majoritarian
Lesotho	1998	exit	Parliamentary	Majoritarian
Madagascar	1991	entry	Presidential	Proportional
Malawi	1994	entry	Presidential	Majoritarian
Mali	1992	entry	Presidential	Majoritarian
Mexico	1994	entry	Presidential	Proportional
Mongolia	1990	entry	Presidential	Majoritarian
Mozambique	1994	entry	Presidential	Proportional
Myanmar (Burma)	1962	exit	Parliamentary	Majoritarian
Nepal	1990	entry	Parliamentary	Majoritarian
Nicaragua	1990	entry	Presidential	Proportional
Niger	1991	entry	Presidential	Proportional
Niger	1996	exit	Presidential	Proportional
Nigeria	1966	exit	Parliamentary	Majoritarian
Nigeria	1979	entry	Presidential	Majoritarian
Nigeria	1984	exit	Presidential	Majoritarian
Pakistan	1962	entry	Presidential	Majoritarian
Pakistan	1970	exit	Presidential	Majoritarian
Pakistan	1972	entry	Presidential	Majoritarian
Pakistan	1977	exit	Presidential	Majoritarian
Pakistan	1988	entry	Presidential	Majoritarian
Panama	1968	exit	Presidential	Majoritarian
Panama	1989	entry	Presidential	Proportional
Paraguay	1989	entry	Presidential	Proportional
Peru	1968	exit	Presidential	Proportional
Peru	1979	entry	Presidential	Proportional
Peru	1992	exit	Presidential	Proportional
Peru	1993	entry	Presidential	Proportional
Philippines	1972	exit	Presidential	Majoritarian
Philippines	1986	entry	Presidential	Majoritarian
Poland	1989	entry	Parliamentary	Majoritarian

Portugal	1975	entry	Parliamentary	Proportional
Romania	1990	entry	Parliamentary	Proportional
Sierra Leone	1967	exit	Parliamentary	Majoritarian
Sierra Leone	1968	entry	Parliamentary	Majoritarian
Sierra Leone	1971	exit	Parliamentary	Majoritarian
Sierra Leone	1996	entry	Presidential	Proportional
Sierra Leone	1997	exit	Presidential	Proportional
Somalia	1969	exit	Parliamentary	Proportional
South Korea	1963	entry	Presidential	Majoritarian
South Korea	1972	exit	Presidential	Majoritarian
South Korea	1987	entry	Presidential	Proportional
Spain	1976	entry	Parliamentary	Proportional
Sudan	1965	entry	Parliamentary	Majoritarian
Sudan	1970	exit	Parliamentary	Majoritarian
Sudan	1986	entry	Presidential	Majoritarian
Sudan	1989	exit	Presidential	Majoritarian
Taiwan	1992	entry	Presidential	Proportional
Thailand	1969	entry	Parliamentary	Majoritarian
Thailand	1971	exit	Parliamentary	Majoritarian
Thailand	1974	entry	Parliamentary	Majoritarian
Thailand	1976	exit	Parliamentary	Majoritarian
Thailand	1978	entry	Parliamentary	Majoritarian
Thailand	1991	exit	Parliamentary	Majoritarian
Thailand	1992	entry	Parliamentary	Majoritarian
Turkey	1971	exit	Parliamentary	Proportional
Turkey	1973	entry	Parliamentary	Proportional
Turkey	1980	exit	Parliamentary	Proportional
Turkey	1983	entry	Parliamentary	Proportional
Uganda	1966	exit	Parliamentary	Majoritarian
Uganda	1980	entry	Presidential	Majoritarian
Uganda	1985	exit	Presidential	Majoritarian
Uruguay	1972	exit	Presidential	Proportional
Uruguay	1985	entry	Presidential	Proportional
Zambia	1968	exit	Presidential	Majoritarian
Zambia	1991	entry	Presidential	Majoritarian
Zimbabwe	1987	exit	Presidential	Majoritarian



**Table 5 Reform episodes under different forms of democracy**

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	<i>All</i>	<i>Parliamentary</i>	<i>Presidential</i>	<i>Proportional</i>	<i>Majoritarian</i>	<i>Permanent</i>	<i>Temporary</i>
<i>Reform &lt; 1981</i>	0.46	0.56	0.39	0.43	0.48	0.16	0.65
<i>Latin America</i>	0.27	0	0.44	0.43	0.09	0.32	0.23
<i>Africa</i>	0.35	0.35	0.34	0.18	0.52	0.26	0.40
<i>Asia</i>	0.17	0.23	0.14	0.07	0.28	0.12	0.21
Number of observations	132	52	79	67	64	50	82

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Entries in the table are based on years in which countries enter or exit from state of democracy (i.e., Polity2 index turns from non-positive to strictly positive)..  
Columns show the average of country characteristics of such reform episodes, cross-tabulated by forms of democracy, defined as in the text.

**Table 6 Democratic reforms and openness 1960-2000 in 132 countries**

<i>parliamentary</i>	0.184 (0.024)*** [0.076]**	0.264 (0.024)*** [0.092]***				
<i>presidential</i>	0.080 (0.017)*** [0.056]	-0.022 (0.018) [0.060]				
<i>proportional</i>			0.192 (0.018)*** [0.060]***	0.153 (0.019)*** [0.067]**		
<i>majoritarian</i>			-0.004 (0.021) [0.065]	-0.023 (0.021) [0.078]		
<i>permanent</i>					0.225 (0.017)*** {0.064}***	0.190 (0.019)*** [0.074]**
<i>temporary</i>					-0.051 (0.020)** [0.049]	-0.056 (0.020)*** [0.054]
<i>3 years before reform type #1</i>		-0.006 (0.036) [0.048]		0.030 (0.025) [0.051]		0.055 (0.024)** [0.051]
<i>3 years before reform type #2</i>		-0.014 (0.023) [0.052]		-0.042 (0.031) [0.062]		-0.058 (0.033)* [0.058]
Continent-year dummies	No	Yes	No	Yes	No	Yes
Number of observations	4549	4549	4549	4549	4549	4549

All regressions include fixed year and country effects. Interacted indicators for years and continents (Africa, Asia, Latin America) included as indicated. Standard errors in parenthesis conventional, in square brackets clustered (by country); \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10%. *3 years before reform type #1 (#2)* refers to an indicator for the three years preceding the first (second) type of reform in the same column.

**Table 7 Democratic reforms, property rights and structural policy 1982-1997 in 113 countries**

Dependent variable	<i>GADP</i>				<i>Structural policy</i>			
<i>parliamentary</i>	0.058 (0.015) <sup>***</sup> [0.040]	0.047 (0.017) <sup>**</sup> [0.042]			0.220 (0.030) <sup>***</sup> [0.061] <sup>***</sup>	0.270 (0.034) <sup>***</sup> [0.079] <sup>***</sup>		
<i>presidential</i>	0.004 (0.008) [0.017]	0.004 (0.010) [0.019]			0.066 (0.017) <sup>***</sup> [0.041]	-0.008 (0.020) [0.043]		
<i>proportional</i>			0.017 (0.009) <sup>*</sup> [0.026]	0.010 (0.011) [0.027]			0.163 (0.019) <sup>***</sup> [0.049] <sup>***</sup>	0.130 (0.022) <sup>***</sup> [0.058] <sup>**</sup>
<i>majoritarian</i>			0.013 (0.011) [0.014]	0.019 (0.012) [0.018]			0.010 (0.022) [0.046]	-0.014 (0.024) [0.058]
<i>3 years before reform type #1</i>		-0.030 (0.019) [0.034]		-0.013 (0.012) [0.011]		-0.022 (0.036) [0.057]		0.005 (0.023) [0.036]
<i>3 years before reform type #2</i>		0.006 (0.010) [0.018]		0.011 (0.013) [0.031]		-0.038 (0.020) <sup>*</sup> [0.033]		-0.059 (0.026) <sup>**</sup> [0.047]
Continent-year dummies	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	1505	1505	1505	1505	1255	1255	1255	1255

All regressions include fixed year and country effects. Interacted indicators for years and continents (Africa, Asia, Latin America) included as indicated. Standard errors in parenthesis conventional, in square brackets clustered (by country); \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10%. *3 years before reform type #1 (#2)* refers to an indicator for the three years preceding the first (second) type of reform in the same column.

**Table 8 Democratic reforms and output per worker 1960-2000 in 135 countries**

<i>parliamentary</i>	0.154 (0.021)*** [0.047]***	0.049 (0.020)** [0.045]				
<i>presidential</i>	-0.053 (0.015)*** [0.054]	-0.006 (0.014) [0.048]				
<i>proportional</i>			0.016 (0.016) [0.062]	0.042 (0.015)*** [0.051]		
<i>majoritarian</i>			0.001 (0.018) [0.068]	-0.025 (0.016) [0.053]		
<i>permanent</i>					-0.010 (0.015) [0.061]	- 0.019 (0.015) [0.056]
<i>temporary</i>					0.042 (0.017)** [0.051]	0.042 (0.015)*** [0.042]
<i>3 years before reform type #1</i>		0.053 (0.029)* [0.039]		-0.008 (0.020) [0.038]		-0.053 (0.019)*** [0.036]
<i>3 years before reform type #2</i>		-0.066 (0.018)*** [0.036]*		-0.057 (0.023)** [0.044]		-0.007 (0.025) [0.044]
Continent-year dummies	No	Yes	No	Yes	No	Yes
Number of observations	4320	4320	4320	4320	4320	4320

All regressions include fixed year and country effects. Interacted indicators for years and continents (Africa, Asia, Latin America) , as well as for years and socialist legal origin, included as indicated.

Standard errors in parenthesis conventional, in square brackets clustered (by country) ; \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10% .

*3 years before reform type #1 (#2)* refers to an indicator for the three years preceding the first (second) type of reform in the same column.

**Table 9 Democratic reforms and government consumption 1960-2000 in 137 countries**

<i>parliamentary</i>	1.320 (0.494) <sup>***</sup> [0.916]	2.332 (0.522) <sup>***</sup> [1.174] <sup>**</sup>				
<i>presidential</i>	-0.120 (0.339) [0.860]	-0.984 (0.378) <sup>***</sup> [1.041]				
<i>proportional</i>			0.646 (0.360) <sup>*</sup> [0.705]	0.417 (0.401) [0.820]		
<i>majoritarian</i>			-0.149 (0.418) [1.308]	-0.212 (0.431) [1.465]		
<i>permanent</i>					0.653 (0.355) <sup>*</sup> [0.951]	0.433 (0.402) [1.260]
<i>temporary</i>					-0.223 (0.397) [0.751]	-0.271 (0.406) [0.835]
<i>3 years before reform type #1</i>		-0.068 (0.738) [0.884]		-0.025 (0.529) [0.826]		-0.140 (0.501) [0.932]
<i>3 years before reform type #2</i>		-0.277 (0.491) [0.853]		-0.381 (0.635) [0.973]		-0.154 (0.685) [0.883]
Continent-year dummies	No	Yes	No	Yes	No	Yes
Number of observations	4460	4460	4460	4460	4460	4460

All regressions include fixed year and country effects. Interacted indicators for years and continents (Africa, Asia, Latin America) included as indicated. Standard errors in parenthesis conventional, in square brackets clustered (by country); \*\*\* significant at 1%, \*\* significant at 5%, \* significant at 10% .  
*3 years before reform type #1 (#2)* refers to an indicator for the three years preceding the first (second) type of reform in the same column.