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 polices 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

In this paper, I combine insights from two recent research programs to extend the empirical analysis of the political economy of development.

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 large 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.

A recent research program 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 polices in regulation and trade to be essential for economic performance by an imaginative use of history and geography to isolate exogenous variation in these policies. (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 vague on 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 terms of substance, my main claim in the paper 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. First, if constitutional arrangements indeed shape fiscal policy and corruption, they are also likely to be reflected in the structural polices fostering economic development, such as property-rights preserving regulations and non-protectionist trade policies.

Second, if history and culture indeed shape important societal institutions, they are also likely to be reflected in the form of democracy.

In terms of methodology, I extend the 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 that the more than 150 switches in and out of democracy in the last forty years 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 they 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. As a part of this description, I review recent research in the macro development field by replicating – in spirit – 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 the Acemoglu et al measure of settler mortality, and by birth dates of the current (democratic) constitutions.

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 for a variety of policy outcomes based on exits and entries into different forms of democracy. 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, and now also proportional (as opposed to majoritarian) democratic elections, have positive effects on the

liberalization of trade and the protection of private property rights. 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 polices, but also significantly expand government spending. Indeed my results on government spending corroborate – both qualitatively and quantitatively – the results in Persson and Tabellini (2003, 2004) based on cross-sectional data from a set of 80 democracies.

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 the results in the existing 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).

I adopt several measures from the recent development literature. 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. 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* – that is, a measure of labor productivity – 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 (which also includes the set of continent and colonial history indicators). However, this positive correlation could reflect influences in each or both directions.

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 conti-

nental 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 the argument further. Their ingenious idea was to use 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 political arrangements, if any, are we more likely to observe the adoption of more growth-promoting polices? 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 undiscriminating 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. Even though protectionist trade policies primarily might be targeted towards sectors of production rather than regions of residence, sectors and regions 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 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 their benefits, we obtain some insights from the recent theoretical work on constitutions and fiscal policy. A first insight 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 polices better serving the interests of broad majorities than do majoritarian elections, 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). A second 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 indexes 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.[1] 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 above stability argument 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 towards 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 preliminary 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. In this 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 the variables we have just defined as instruments, alongside the two Hall and Jones instruments, in the earlier two-stage specification. In other words, I

[1] Another source for the classification of democracies and non-democracies, which I exploit in the panel 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.

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 rules for democratic elections.

Estimation results for the full 1990s cross section of countries appear in the first portion of *Table* 2. Columns 1 and 2 show the first and second stage of the 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 presidential 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 correspond-

ing estimates in Table 1.

History, constitutions and policy 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 counterexample to the arguments in Glaeser et al (2004) that it is hard to find any measures of societal institutions, in the forms of tangible rules and procedures, that are associated 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.

The results support one of the main ideas 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 simultaneous with economic performance or structural policy. While the purely statistical arguments for exogeneity in the previous subsection are reassuring, they are not enough. The estimates also say nothing about the other main idea mentioned in the introduction, namely that the historical forces influencing current policy may indeed exercise their impact through constitutional arrangements. To shed light on these issues, I turn to an instrumental-variable strategy based on *a priori* arguments. Specifically, I scrutinize the first-stage estimates reported in Table 2, treating the constitutional variables as endogenous.

Suppose that the influence of Western colonization exercises its influence on current policies only (mainly) via the form of political institutions. Then, greater influence should show up in a greater probability of observing the same type of political arrangements (post independence) in previous colonies as those observed in Western Europe, i.e., stable parliamentary democracy. Under these circumstances, *settler mortality* becomes a valid instrument for parliamentary democracy and/or age of democracy. To achieve some overidentification, I also use two additional instruments for constitutional arrangements constructed and closely discussed in Persson and Tabellini (2003, 2004). These exploit different frequencies in the adoption of alternative constitutional features during different historical periods. In particular, 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.

The resulting estimation strategy is illustrated in *Figure* 3. As the figure shows, 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, the likelihood of parliamentary democracy is reduced by higher settler mortality (less Western European influence), and raised by constitutional birth in the early post-war period. The resulting variation in the form of government is estimated to have about double the effect on structural policy as that estimated in Table 2. Moreover, by the chi-2 statistic at the bottom of column 2, we cannot reject 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 the 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 specification asks quite a bit of the data, the effect on structural policy of parliamentary democracy appears quite robust.

The cross-sectional results in this section are certainly suggestive, but skeptics may still suspect them to be driven by omitted unobservable country-characteristics, or some other statistical artefact. Next, I will address some of these concerns by estimating how the form of democracy affects policy from the within-country variation associated with democratic reforms. The result that older democracies choose 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

In this section, I rely on a panel data set with political and economic data from 1960 to 2000 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. The main idea is to exploit switches into, or out of, democratic status to construct indicators for a variety of different reforms. As in the previous section, this requires a 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.[2] 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 when a nation's *democracy* indicator flips between 0 and 1 (in either direction). This definition of reform may be considered quite arbitrary, and based on marginal changes in democratic status. In fact, most reforms accord with conventional views on political history, and appear to represent major institutional change. Specifically, 156 reforms are registered in the data – 90 entries into and 66 exits out of democracy – with an average change on the 20-step scale of 8.5 (and

[2]The 1/2 million limit of the Polity data excludes some small nations included in the Freedom Hourse 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).

a standard deviation of 4.1). Availability of other variables, and a specific requirement that the outcome of interest be observed for at least two years before and after the reform, means that the typical estimates below still rely on more than 100 reform episodes – a reasonably large number.

In the estimates reported below, countries that did not undergo a democratic reform in the sample period will be used as a control group throughout the panel, whether they stayed democratic or non-democratic throughout. *Table* 4 compares the characteristics of the countries in the reform and non-reform groups for which data on both income and democracy are available in two specific years: 1963 and 1997. The higher number of countries at the end of the sample period (132 vs. 90) reflects both data availability and the formation of new countries. Non-reforming countries are, on average, richer than those in the reform group, at both points in time (but also display more income dispersion, 15-30% higher standard errors). The world trend towards greater incidence of democracy implies a higher value of the democracy indicator in the full sample in 1997 than in 1963 (0.70 vs.0.49). Naturally, this trend – meaning that more countries introduce than get rid of democracy – shows up mostly in the reform group, where the average value of the *democracy* indicator rises from 0.36 in 1963 to 0.77 in 1997. Finally, the table shows that 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.

However, my main purpose is not to compare policy outcomes under democracy and non-democracy, but to compare outcomes under different forms of democracy. To that end, I define the same indicators as in the previous section, *parliamentary, presidential, proportional*, and *majoritarian democracy*, for the country-years where the necessary data is available. *Table* 5 compares the distribution of covariates in reform episodes across each of these classifications (once more, conditional on income data being available). 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 0.45) 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 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[3], reforms into presidential and majoritarian democracy are over-represented in Africa, while reforms into parliamentary and majoritarian democracy are over-represented in Asia. The estimation strategy should thus avoid confounding this continent-specific incidence of reforms and prospective continent-specific trends in the outcome variables of interest.

Because I am also interested in the effects of reversible vs. irreversible reforms, I split the country-years when *democracy* = 1 into two groups. I set *permanent democracy* = 1 after reforms in the sample period, which are not reversed before the end of the panel (as explained earlier, countries starting out and remaining democratic throughout belong to the control group). There are 45 periods of permanent democracy in the panel, with an average length of 12.6 years, and a standard deviation of 7.8. The indicator *temporary democracy* refers to democratic periods ending within sample. We observe 61 such spells, with an average length of 6.0 years and a standard deviation of 5.0. Table 5 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 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 does not vary a great deal across continents, except that Africa is somewhat over-represented in temporary democracy.

Econometric specification and concerns Consider an outcome variable y in country i and year t, y_{it}. The basic econometric specification to be used below can then be written as:

$$y_{it} = \alpha_i + \upsilon_t + \sum_{f=1}^{F} \beta^f D_{it}^f + u_{it} \ , \tag{1}$$

where the D_{it}^f are indicator variables for the different forms f = 1,..., F of democracy discussed above. Thus, the parameters of interest are the coefficients β^f. Because the specification in (1) includes fixed country effects (α_i),

[3]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 control group because they became solid democracies directly upon independence in the mid 1960s.

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 (υ_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 the non-reforming countries (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 β^f may 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 4, Latin America is over-represented among the reformers; moreover, 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 incidences of reform and region-specific trends in policy or performance, I check that the estimates of β^f are robust to adding a set of continent-time interaction terms, $\sum_c \gamma^c \upsilon_t$ (with γ^c denoting indicators for different continents) to the specification in (1).[4]

A second concern about identification is that the reform episodes are not 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 as well as entries with regard to democracy. Nevertheless, we may

[4]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 follwing the breakdown of the Soviet Union and the fall of the Iron curtain.

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, $f = 1, .., F$.

A final econometric issue concerns inference, rather than identification. The policies and performance measures I consider below typically display quite strong autocorrelation. In these circumstances, the conventional standard errors obtained from difference-in-difference estimation can be seriously understated (see Bertrand, Duflo, and Mullainathan, 2004). To guard against incorrect inference, I report not only report the conventional confidence levels, but those obtained by clustering the standard errors 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 criteria for an open economy are met.

Table 6 shows the difference-in-difference estimates of reforms into different forms of democracy on this aspect of structural policy. (Estimating the effect of reforms into democracy, as such, generally yields a positive coefficient, but the 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 democracy with a parliamentary and presidential form of government, respectively. Underneath the estimated coefficients are confidence levels based on two sets of standard errors: conventional ones in parentheses and clustered (by country) ones in 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, 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*, and there is no robust evidence of pre-reform changes driving the results. 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 0.3.

Columns 3 and 4 report the same kind of estimates, when reforms are distinguished by electoral rule. Here, the introduction of democracy with proportional elections raises the probability of an opening of the economy, whereas the introduction of majoritarian elections does not. Once more, there is 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. The results show that, as expected, permanent democratizations significantly raise the probability of opening up 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 coeffcicient 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. Reforms into parliamentary democracy, as well as proportional democracy, have strong significant positive effects on this broad index 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* is around 0.3. This estimate, entirely identified from the within-country variation, is quite close to the IV estimates in Table 3 where the same effect is entirely identified from the cross-country variation in the data. This coincidence of the findings strongly 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 main 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 polices 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 estimates in this section, these older democracies belong to the control group rather than the reform group.

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 should 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 percent-

age 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 3-4% of GDP and this effect is quite precisely estimated. Additional results (not shown here) 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), which only considered cross-sectional data for a set of democracies and used a very different data source (IFS data from the IMF). They estimated that a reform from presidential to parliamentary democracy raises spending by about 5% of GDP. This is the same number as that obtained here, when subtracting the coefficient on presidential democracy from that on parliamentary democracy in the more general specification of column 2. I consider this coincidence with the earlier Persson-Tabellini estimates remarkable in its own right. But from the viewpoint of the present paper, it is consistent with the second possible interpretation of 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 the regulatory and trade 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 prodcutivity. 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* = 1and *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.

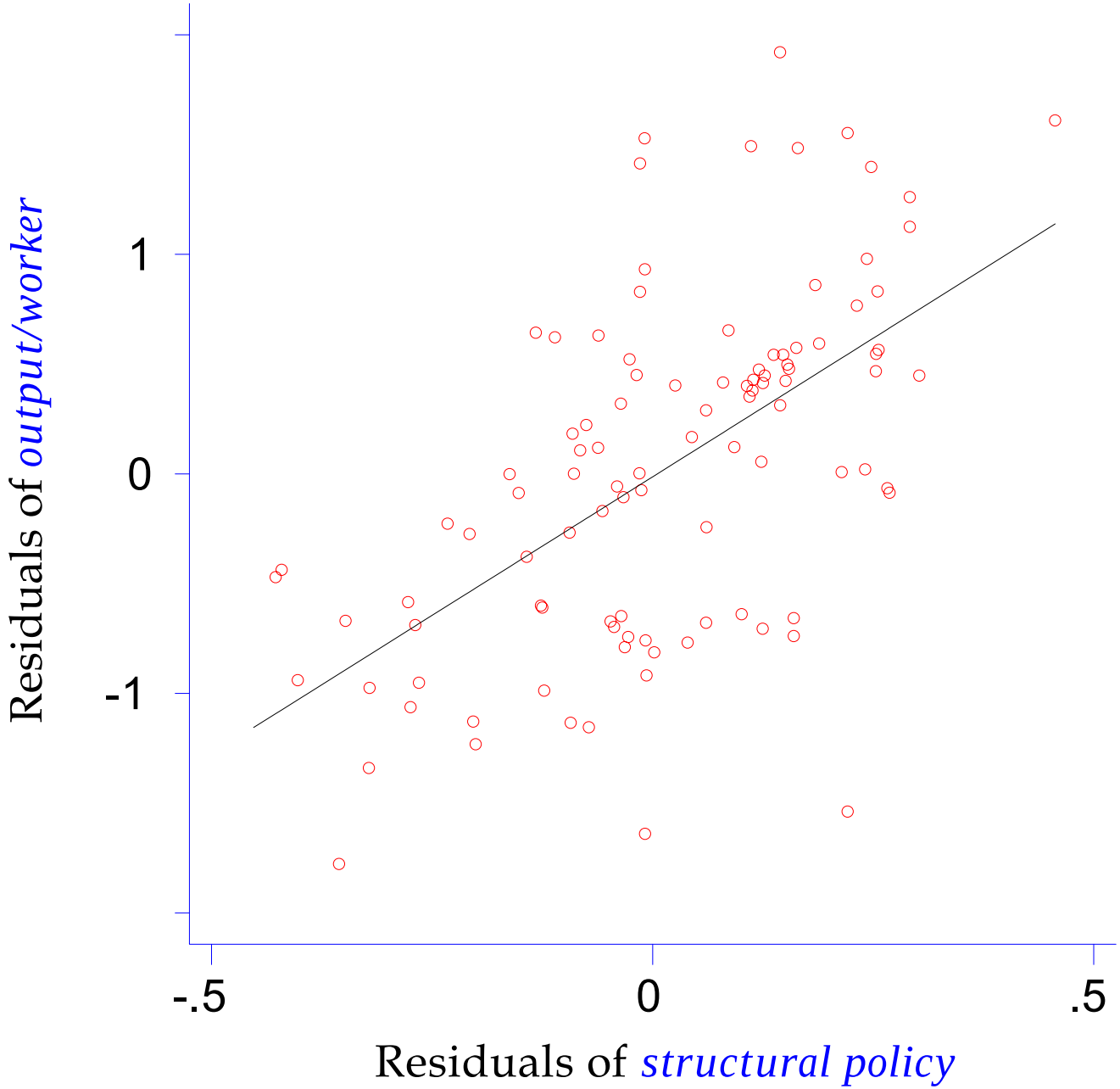
Colonial origin: Binary indicators for British, French Spanish-Portuguese, and Other colonizers. In cross-sectional specifications, these binary indicators are discounted by the factor (250 – *years since independence*)/250. In the panel specifications they are interacted with year dummies. Source: Persson 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
1
0
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.5
Residuals of structural policy

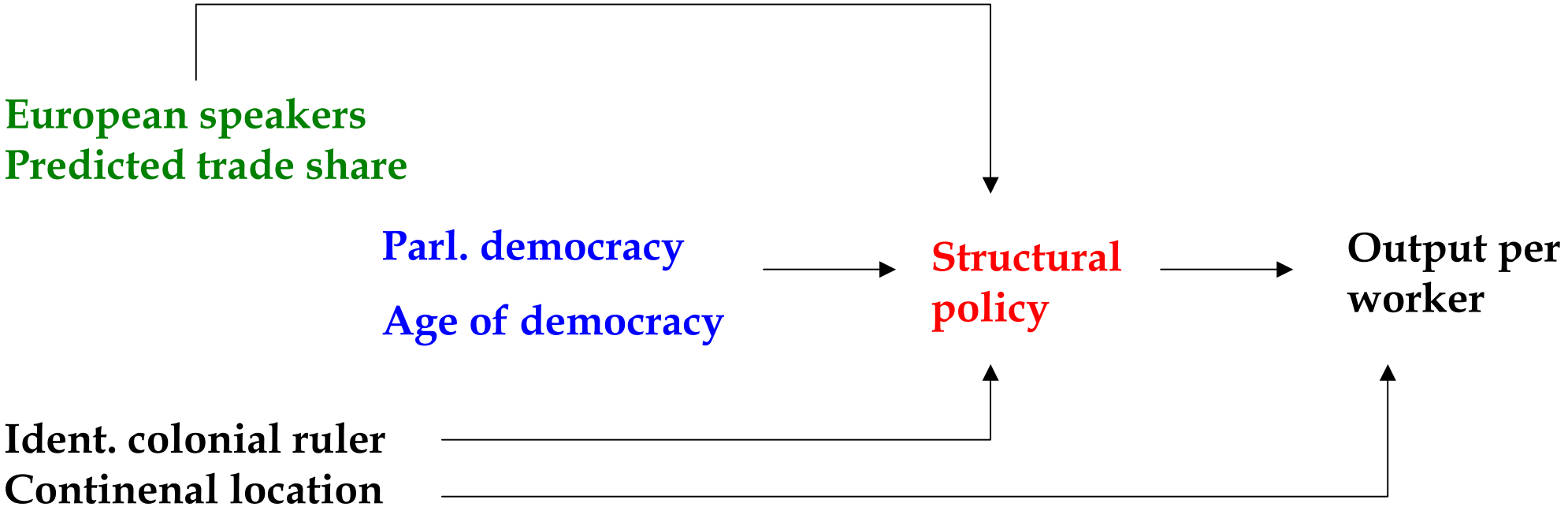
European speakers
Predicted trade share
Parl. democracy
Age of democracy
Structural policy
Output per worker
Ident. colonial ruler
Continenal location

Figure 2

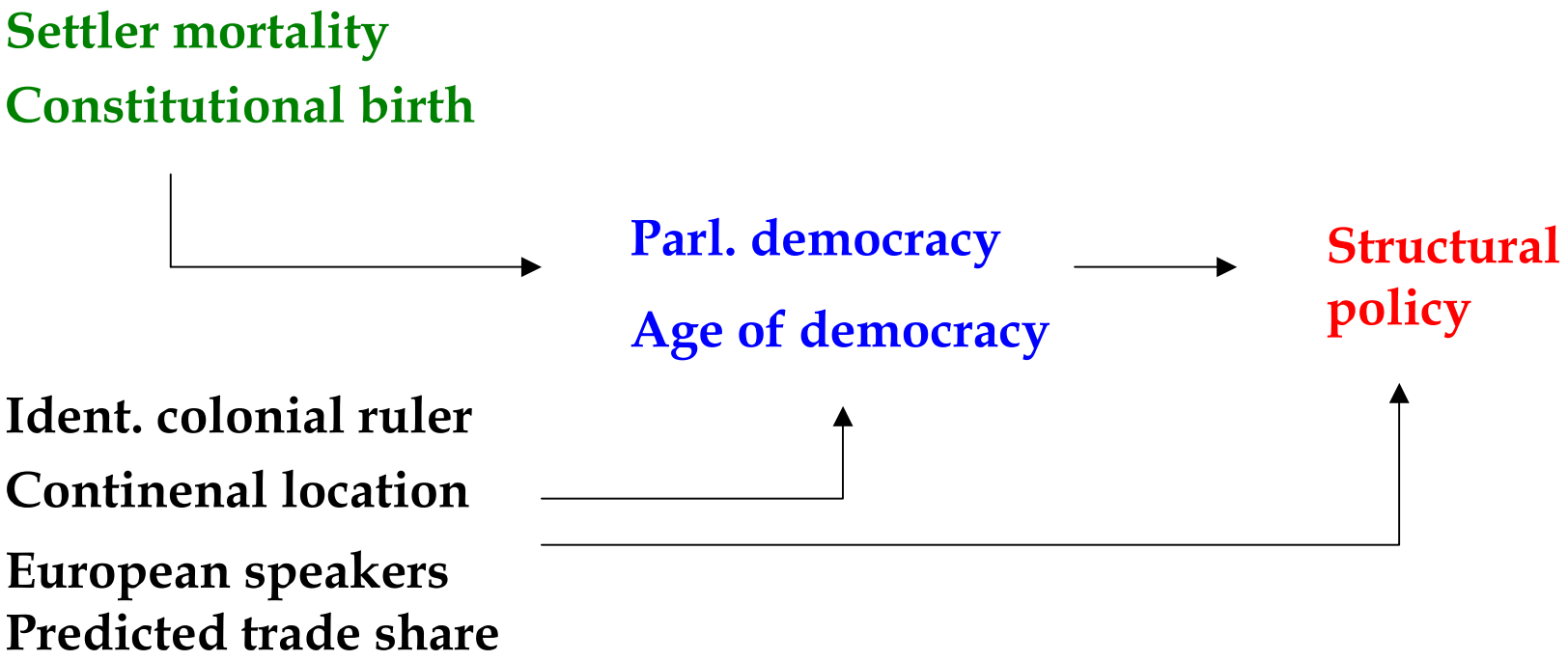
Settler mortality
Constitutional birth
Parl. democracy
Age of democracy
Structural policy
Ident. colonial ruler
Continenal location
European speakers
Predicted trade share

Figure 3

Table 1 History, structural policy and economic performance

Cross section	Full data set		Settler mortality data set			
Dependent variable	*structural policy*	*output per worker*	*structural policy*	*output per worker*	*structural policy*	*output per worker*
structural policy		3.81*** (0.66)		5.17*** (1.06)		9.00*** (2.57)
European speakers	0.21*** (0.06)		0.33*** (0.09)			
predicted trade share	0.09*** (0.03)		0.05 (0.04)			
settler mortality					–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	*structural policy*	*output per worker*	*GDP per capita*	*TFP*	*structural policy*	*output per worker*	*GDP per capita*	*TFP*
structural policy		3.59***	3.91***	1.88***		4.78***	5.22***	2.05***
		(0.64)	(0.70)	(0.39)		(0.86)	(0.85)	(0.56)
parliamentary democracy	0.10**				0.17**			
	(0.05)				(0.08)			
age of democracy	0.32***				0.34**			
	(0.09)				(0.15)			
European speakers	0.13**				0.20*			
	(0.06)				(0.08)			
predicted trade share	0.08***				0.01			
	(0.02)				(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	*parliamentary democracy*	*structural policy*	*age of democracy*	*structural policy*	*parliamentary democracy*	*age of democracy*	*structural policy*
parliamentary democracy		0.31**		0.18**			0.49***
		(0.16)		(0.08)			(0.19)
age of democracy		0.38***		0.64**			–0.05
		(0.14)		(0.30)			(0.25)
settler mortality	–0.10**		–0.05**		–0.08**	–0.04*	
	(0.04)		(0.02)		(0.04)	(0.02)	
constitutional birth 51-80	0.25**		0.15***		0.23**	0.07	
	(0.11)		(0.06)		(0.11)	(0.05)	
constitutional birth 21-50					–0.26	0.26***	
					(0.17)	(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 Characteristics of reform and non-refeorm countries

	All in 1963	*Reformers in 1963*	*Non-reformers in 1963*	*All in 1997*	*Reformers in 1997*	*Non-reformers in 1997*
income	7.88	7.52	8.54	8.36	7.94	8.73
democracy	0.49	0.36	0.62	0.70	0.77	0.63
Latin America	0.22	0.36	0.09	0.17	0.29	0.06
Africa	0.33	0.33	0.33	0.32	0.35	0.29
Asia	0.11	0.16	0.07	0.10	0.13	0.07
Number of observations	90	45	45	132	70	62

Reform group includes countries that entered or exited the state of democracy (strictly positive values of Polity2) at least once during 1960-2000.
Non-reform group includes countries that remain democracies or non-democracies throughout this period.
First (last) three columns refer to averages of all existing countries 1963 (1997) for which data is available.

Table 5 Reform episodes under different forms of democracy

	All	*Parliamentary*	*Presidential*	*Proportional*	*Majoritarian*	*Permanent*	*Temporary*
Reform < 1981	0.45	0.47	0.45	0.46	0.45	0.14	0.67
Latin America	0.27	0	0.37	0.48	0.09	0.33	0.25
Africa	0.36	0.16	0.41	0.16	0.52	0.24	0.41
Asia	0.18	0.25	0.16	0.04	0.31	0.14	0.22
Number of observations	132	32	97	62	67	49	76

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

parliamentary	0.220	0.331				
	(0.027)***	(0.027)***				
	[0.095]**	[0.113]***				
presidential	0.099	-0.007				
	(0.017)***	(0.018)				
	[0.058]*	[0.062]				
proportional			0.202	0.165		
			(0.018)***	(0.019)***		
			[0.061]***	[0.069]**		
majoritarian			0.012	-0.025		
			(0.024)	(0.024)		
			[0.086]	[0.100]		
permanent					0.225	0.190
					(0.017)***	(0.019)***
					{0.064}***	[0.074]**
temporary					-0.051	-0.056
					(0.020)**	(0.020)***
					[0.049]	[0.054]
3 years before reform type #1		0.070		0.058		0.055
		(0.041)*		(0.027)**		(0.024)**
		[0.062}		[0.061]		[0.051]
3 years before reform type #2		-0.007		-0.036		-0.058
		(0.025)		(0.034)		(0.033)*
		[0.055]		[0.069]		[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 shown. 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 129 countries

Dependent variable	*GADP*				*Structural policy*			
parliamentary	0.056 (0.015)*** [0.042]	0.060 (0.016)*** [0.047]			0.256 (0.029)*** [0.050]***	0.347 (0.030)*** [0.053]***		
presidential	0.006 (0.008) [0.017]	-0.006 (0.009) [0.018]			0.062 (0.016)*** [0.039]	-0.020 (0.017) [0.041]		
proportional			0.014 (0.009) [0.024]	0.004 (0.010) [0.028]			0.168 (0.018)*** [0.047]***	0.139 (0.019)*** [0.057]**
majoritarian			0.023 (0.011)** [0.017]	0.017 (0.011) [0.018]			0.024 (0.021) [0.045}	-0.007 (0.022) [0.059]
3 years before reform type #1		0.012 (0.020) [0.034}		-0.007 (0.012) [0.018]		0.037 (0.037) [0.052]		0.035 (0.022) [0.042]
3 years before reform type #2		-0.017 (0.010)* [0.016]		-0.018 (0.013) [0.024]		-0.049 (0.019)** [0.036]		-0.082 (0.025)*** [0.056]
Continent-year dummies	No	Yes	No	Yes	No	Yes	No	Yes
Number of observations	1918	1804	1918	1804	1587	1536	1587	1536

All regressions include fixed year and country effects. Interacted indicators for years and continents (Africa, Asia, Latin America) included as shown.
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

parliamentary	0.157 (0.024)*** [0.053]***	0.053 (0.024)** [0.057]				
presidential	-0.042 (0.015)** [0.067]	0.001 (0.014) [0.056]				
proportional			0.017 (0.016) [0.064]	0.040 (0.015)*** [0.052]		
majoritarian			-0.010 (0.021) [0.084]	-0.057 (0.018)*** [0.072]		
permanent					-0.010 (0.015) [0.061]	- 0.019 (0.015) [0.056]
temporary					0.042 (0.017)** [0.051]	0.042 (0.015)*** [0.042]
3 years before reform type #1		0.017 (0.032) [0.045}		-0.014 (0.021) [0.038]		-0.053 (0.019)*** [0.036]
3 years before reform type #2		-0.047 (0.019)** [0.036]		-0.076 (0.026)*** [0.042]*		-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 shown.
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

parliamentary	2.949 (0.560)*** [1.219]**	4.260 (0.599)*** [1.742]**				
presidential	0.067 (0.346) [0.844]	-0.772 (0.384)** [1.015]				
proportional			0.818 (0.362)** [0.704]	0.684 (0.401)* [0.818]		
majoritarian			0.957 (0.478)** [1.574]	1.014 (0.498)** [1.793]		
permanent					0.653 (0.355)* [0.951]	0.433 (0.402) [1.260]
temporary					-0.223 (0.397) [0.751]	-0.271 (0.406) [0.835]
3 years before reform type #1		0.603 (0.855) [1.796}		0.236 (0.574) [0.999]		-0.140 (0.501) [0.932]
3 years before reform type #2		-0.204 (0.522) [0.940]		-0.265 (0.698) [1.170]		-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 shown. 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.