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Democracy and Development: The Devil in the Details*

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Does democracy promote economic development? Despite many attempts to address this question, the answer remains elusive. Richer countries are generally democratic. But this cross-country correlation could reflect reverse causation or omitted variables. Evidence that political regime changes produce subsequent economic growth is considerably weaker. Does this mean that political regimes do not influence economic development? Not necessarily, but such causal effects are difficult to identify from the within-country variation.

A plausible reason for this difficulty is that “democracy” is too blunt a concept. Political regimes come in various forms and are reformed in different circumstances. Such heterogeneity is interesting in its own right. Moreover, if heterogeneity is not random, correlation between specific reform features and their occurrence makes it hazardous to estimate an average causal effect on economic growth.

This paper illustrates three specific instances where the details of democratic reform influence their economic effects. Section I clarifies our empirical strategy. Section II zooms in on political and economic reforms, drawing on Francesco Giavazzi and Guido Tabellini (2005). Democratizations as well as

liberalizations induce accelerations of growth. But the sequence of reforms is crucial: countries liberalizing their economy before extending political rights do better. Section III considers different forms of democracy, drawing on Torsten Persson (2005). Specific democratic institutions influence the fiscal and trade policies implemented after democratization, which may explain why presidential democracy leads to faster growth than parliamentary democracy. Section IV distinguishes expected and actual political reforms, drawing on Persson and Tabellini (2005). Taking expectations of regime change into account helps identify a stronger growth effect of democracy.¹

I. Empirical methodology

While political institutions are generally very persistent, they sometimes change suddenly and drastically – as in many democratizations or coups. Under appropriate identifying assumptions, such regime changes can be exploited by comparing average performance before and after the event, as in the estimation of a treatment effect.

Our sample has annual observations for about 150 countries and includes about 120 regime changes over the period 1960-2000; in Section IV, we extend the panel back to the mid 1800s, with twice as many regime changes. We classify a country as democratic if the *polity2* variable in the Polity IV data set is strictly positive.² Per-capita income comes from the Penn World Tables for 1960-2000 and Angus Maddison (2001) for 1850-2000.

We estimate a panel regression:

$$y_{i,t} - y_{i,t-1} = \beta y_{i,t-1} + \phi D_{i,t} + \rho \mathbf{x}_{i,t} + \alpha_i + \theta_t + \epsilon_{i,t} , \quad (1)$$

where $y_{i,t}$ denotes (log) per capita income in country i and year t , $D_{i,t}$ is a

dummy variable equal to one under democracy, $\mathbf{x}_{i,t}$ is a vector of control variables, while α_i and θ_t are country and year fixed effects. Thus, we estimate the parameter ϕ by a difference-in-difference methodology, where countries changing regime are the “treated”, and those that do not are the “controls”.

Our identifying assumption requires that the selection of countries into democracy be uncorrelated with the *country-specific and time-varying* shock to growth: $\epsilon_{i,t}$. It allows any correlation between regime selection and the country fixed effect, α_i – e.g., that fast-growing countries more likely become democratic than slow-growing ones. But it means that, absent any regime change, average growth in treated countries should (counterfactually) have been the same as in control countries (conditional on $\mathbf{x}_{i,t}$). This would fail, e.g., if democratic transitions are enacted by far-sighted leaders, who have a lasting impact on growth irrespective of the regime change, or if lapses into dictatorship coincide with lasting deteriorations of economic performance that are independent of the regime change.

As a concrete example, we might confound economic and political transition after the fall of the Berlin wall, when many formerly communist regimes introduced democracy as well as market economy. Therefore, we include in $\mathbf{x}_{i,t}$ a binary indicator for years after 1989 in the formerly socialist countries of Central and Eastern Europe and the split-up Soviet Union. We also include indicators for years of wars (current and lagged), since wars are correlated with regime changes as well as growth. To increase the similarity between treated and controls, in most specifications we include dummy variables for continental location (Africa, Asia and Latin America) and socialist legal origin interacted with year dummy variables. To reduce serial correlation and

allow for economic convergence: lagged per-capita income is always included.

Unobserved heterogeneity may also violate our identifying assumption. Circumstances surrounding regime changes differ widely across time and space, as do the types of political institutions adopted or abandoned. Thus, the effects of a crude democracy indicator are likely to differ across observations. If we neglect this heterogeneity and estimate the average effect of democracy as in (1), $\epsilon_{i,t}$ also includes the term $(\phi_{i,t} - \phi)D_{i,t}$, where $\phi_{i,t}$ is the effect of democracy in country i and year t . Identification of ϕ now requires heterogeneity in the effect of reforms to be uncorrelated with their occurrence. This assumption fails if countries self-select into democracy based on the growth effect of regime changes (e.g., $D_{i,t} = 1$ more likely when $\phi_{i,t} > \phi$). A specification including reform heterogeneity may thus render identification of ϕ more credible by reducing the extent of unobserved heterogeneity.

Such specifications need to be parsimonious, however, since the number of reforms is limited relative to the variety in democratic experience. Below, we decompose the effects of political reforms according to a few observable features, one at a time. Studying the economic outcomes of specific types of reforms is relevant both from a practical point of view, and as a test of specific hypothesis. Econometrically, the *relative* effect of specific reforms can be identified under weaker assumptions than those needed to identify their average effect ϕ .

II. Economic liberalization and democracy

To start off, we estimate the average effect of democracy on growth, i.e., ϕ in (1). Column 1 of Table 1 suggests that becoming a democracy accelerates growth by 0.75 percentage points, an economically relevant and statistically

significant effect (we report robust standard errors, but the results are similar with standard errors clustered by country). With an estimated convergence rate of 6 percent per year (parameter β in (1)), the long-run effect on income per capita is 12.5 percent. Democracy has very similar effects on the investment rate (results not reported). While both growth and investment tend to slow down around the democratic transition, controlling for years preceding and following the regime change does not affect the inference about the average effect of democracy (see Giavazzi and Tabellini (2005) for more extensive discussion). Elias Papaioannou and Gregorios Siourounis (2004) and Richard Roll and John Talbott (2004) also obtain results that democracy promotes growth.

Democratization is often associated with economic reforms, such as opening the economy to international trade and extending the role of markets. Sometimes economic liberalization leads democratization, more often it lags by a few years – perhaps because similar forces push for both kinds of reforms. Joint economic and political reforms could violate our identifying assumptions, however. Not controlling for economic reforms could bias upwards the estimated effect of democracy, via positive correlation between $D_{i,t}$ and $\epsilon_{i,t}$ in (1).

Like Giavazzi and Tabellini (2005), we use an indicator of economic liberalizations originally coded by Jeffrey Sachs and Andrew Werner (1995) and updated by Wacziarg and Karen Welch (2004). The difference-in-difference estimates in column 2 confirm that economic reforms promote economic performance. Without controlling for the political regimes, liberalizations accelerate growth by about 1.3 percentage points (a long-run effect on income of

26 percent).

If the column-1 estimate could be biased, however, then so could the column-2 estimate. If economic and political reforms are correlated and independently affect growth, we ought to estimate a multiple treatment equation. In column 3, we thus include both indicators. Both reforms retain a significant and positive effect on growth, with economic reform having the stronger effect (though its coefficient falls compared to column 2).

Considering the joint effect of reforms lends additional credibility to the identifying assumption, but does not fully address the issue of heterogeneity in terms of the sequence of reforms. Column 4 of Table 1 adds two dummy variables to the regression: the first equals unity if democracy is enacted first of two reforms, the second equals unity for the opposite order, while both equal zero if only one type of reform is enacted.³Countries where economic liberalization preceded democracy include South Korea, Taiwan, Chile and Mexico. The opposite sequence took place in countries such as Argentina, Brazil, the Philippines and Bangladesh.

Enacting only one reform still has a positive and significant effect on growth, similar to those in columns 1-3. Moreover, the estimated coefficient of “democracy after liberalization” is positive and significant: the boost to growth from the two reforms is about 3.5 percent. But “liberalization after democracy” is negative and significant, implying an overall effect which is barely positive and statistically insignificant. Giavazzi and Tabellini (2005) show that this finding is very robust. A plausible interpretation is that young democracies born in closed economic environments are more likely bogged down in redistributive conflict leading to populist economic policies, while

young democracies in open economies are forced to pay more attention to economic efficiency. Moreover, opening the economy often goes hand in hand with securing the protection of property rights and enforcing the rule of law, which may be a prerequisite for a well-functioning democracy. Naturally, the usual caveats about identification apply. But if the estimates do uncover a causal effect, they suggest that reformers of closed autocracies ought to give priority to economic over political liberalization.

III. Forms of democracy

Another source of reform heterogeneity is the kind of democratic institutions adopted or abandoned. Political scientists stress distinctions between different electoral rules and different forms of government. In recent research (Persson and Tabellini, 2003, 2004), we show that these constitutional traits imply systematic differences in economic policies. A natural question is whether the growth effects of becoming a democracy differ across these constitutional forms, and whether policy effects like those uncovered among existing democracies also appear in the reform switches between democratic and non-democratic institutions.

Column 1 of Table 2 decomposes the average growth effect of democracy in two different ways. Besides democracy, we include two additional binary variables classifying democracies by their form of government (presidential vs. parliamentary) and electoral rule (majoritarian vs. proportional). Otherwise, the regression is identical to that in column 1 of Table 1.⁴

Clearly, different constitutions are associated with different growth effects. The coefficient on democracy now picks up the default effect of becoming a presidential *and* majoritarian democracy. A new parliamentary democracy

grows 1.5 percentage points less than a new presidential democracy. By the point estimates, the growth effect of a reform from autocracy to parliamentary democracy is negative, although not significantly different from zero. The electoral system, instead, does not appear to influence the growth effect of democracy.

A possible explanation for these results is induced policy changes. Based on cross-sectional estimates within a sample of democracies, Persson and Tabellini (2003, 2004) found that parliamentary and proportional democracies have larger government spending. Here, we follow Persson (2005) and estimate the effect on government consumption with the difference-in-difference specification in (1).⁵ The results are displayed in column 2. A new majoritarian and presidential democracy cuts government consumption by almost 2 percent of GDP, while a new parliamentary democracy instead raises it considerably. The difference in spending between the two forms of government is a highly significant 5 percent of GDP. This estimate only exploits time variation in countries that enter and exit democracy, but is remarkably similar to our earlier estimates exploiting cross-sectional variation among existing democracies. Proportional rather than majoritarian elections raise spending by 1 percent of GDP. This effect is statistically significant but smaller than our previous cross-sectional estimates, probably due to the exclusion of transfers (we found the electoral rule to have a particularly strong effect on social security and welfare spending).

How about other policies? Persson (2005) argues that since parliamentary and proportional democracies seek consensus among broader coalitions of voters, they should not only have larger government spending, but also

less protectionist trade policies. In column 3, we thus estimate the effect on the liberalization indicator used in Section II. Indeed, introducing parliamentary or proportional democracy each raises the probability of a subsequent liberalization by about 10 percentage points, compared to majoritarian and presidential democracy.

These policy outcomes may explain the growth effects. A new parliamentary democracy is more prone to pursue economic liberalizations than a new presidential democracy. But as we saw in Section II, liberalizations following democratizations have weaker effects on growth.⁶ At the same time, parliamentary democracies raise government consumption much more than presidential democracies. If this spending binge distorts economic activity, growth may suffer. While the electoral system also shapes policy – with proportional democracies more prone to spend and liberalize – the spending effects are less pronounced and may not show up in the growth rate.

IV. Expected and actual democracy

If democracy has a positive effect on growth and long-run income, it is bound to raise the returns to investment. But investment reacts to expected, not actual, returns. This means that expected, and not just actual, regime change affects growth. Suppose upcoming regime changes are (partly) anticipated by investors. Then, growth will accelerate well before an imminent democratization, and decelerate well before an imminent coup. This would contradict our identifying assumption in (1), by creating a negative correlation between democracy, $D_{i,t}$, and the growth residual, $\epsilon_{i,t}$, and bias down our estimate of ϕ – the growth effect of democracy.

Motivated by this observation, Persson and Tabellini (2005) formulate a

theoretical model of economic and political change, where both actual and expected political regime influence economic growth. In the model, countries stochastically enter and exit from democracy with probabilities influenced by current and lagged income. The probability of regime change also depends on a country’s “democratic capital”, which influences the willingness of its citizens to stand up for democracy. Democratic capital is assumed to accumulate in years of democracy and in countries with democratic neighbors, but to depreciate under autocracy. Identification is achieved by an exclusion restriction derived in the model, namely that democratic capital has no direct effect on growth (given all the other controls).

Persson and Tabellini (2005) discuss the empirical strategy in detail. Here, we estimate an equation similar to (1) augmented by the probability of regime change – in the form of a hazard rate – as estimated by Persson and Tabellini (2005) over the full sample 1850-2000.⁷The growth equation is consistent with the estimated hazard rate: see Table 3 for details. As country and year fixed effects are included, we estimate the effects of expected democracy entirely from the time variation in the hazard rate.

The first two columns of Table 3 report the estimated results *within* regimes, confining attention to observations under democracy only, or autocracy only. In each sample, we not only consider economic growth (available for 1850-2000), in Panel A, but also investment (available for 1960-2000), in Panel B. Under democracy, the probability of regime change hurts both investment and growth, as expected and consistent with the finding that democracy raises growth. The large negative estimated coefficient reflects the dimension of the estimated hazard rate, which is typically below 10 per-

cent, with an average of 3. A fall in the hazard by 2 percentage points thus raises growth by about 0.5 percentage points.⁸ Under autocracy, the probability of regime change ought to spur growth and investment. Instead, both coefficients are negative although not significantly different from zero. One interpretation is that we have omitted a further heterogeneity, such that democratic reforms fail to boost economic performance in some autocracies. Alternatively, political uncertainty exerts an offsetting negative effect.

In column 3, we study actual as well as expected political regimes in the full sample, including the democracy dummy plus the probability of autocracy in the current period (alone and interacted with lagged democracy to allow the effect of expectations to differ by regime).⁹ In addition to the exclusion restriction for democratic capital, we rely on the usual identifying assumption – $\epsilon_{i,t}$ in (1) uncorrelated with $D_{i,t}$ – now made more credible by including the probability of autocracy as a regressor. This specification is demanding, because actual democracy and the probability of autocracy are highly collinear due to the inertia of political regimes. Nevertheless, the results support the idea that democracy has a positive effect on growth and that expectations also play a role. Actual democracy now induces a growth acceleration of over 1 percent. Given an estimated convergence rate of 2.8 percent, the implied long-run rise in per-capita income is 35 percent. The estimated growth effect is larger than in Table 1, where expectations are neglected. More importantly, it is also much larger than in the same specification over 150 years of data, without controlling for the probability of autocracy (see Persson and Tabellini, 2005). Thus, including expected regime changes brings out a more forceful effect of actual transitions on

growth, in line with the idea that omitting expectations would violate the identifying assumptions. The estimated coefficients on investment are more disappointing. Overall, the results in this section imply that stable and persistent democracy has a stronger effect on development than democracy per se.

Taken together, the results in our paper suggest that democracy is indeed too blunt a concept: the devil is in the details. Future theoretical and empirical work should pay close attention to the heterogeneity of political reforms.

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Notes

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¹ The three papers cited in this paragraph provide references on democracy and growth. Dani Rodrik and Romain Wacziarg (2005) obtain related results with different disaggregations of the effect of democracy.

² Large changes in *polity2* are generally clustered around 0. Although lower than that often chosen by political scientists, this threshold more easily captures the effect of discrete political reforms. We discard reforms in the last three years of the sample, setting to missing the observations of outcomes after such reforms. At the start of the sample, we only require one available observation before the reform. See Giavazzi and Tabellini (2005) and Persson (2005) for more details.

³ To unambiguously identify the sequence, Giavazzi and Tabellini (2005) only

classify episodes that last at least four years as reforms, omitting temporary changes in political or economic institutions. The variables in column 4 of Table 1 use this classification, which differs slightly from that in the preceding columns for a few countries.

⁴ Table 2 assumes the effect of the form of government and the electoral system to be additive. The results are robust to relaxing this assumption.

⁵ While Persson and Tabellini (2003, 2004) use IMF data for central government spending (including transfers), here we use Penn World Tables for central plus local government consumption, in percent of GDP.

⁶ Indeed, all countries that first opened the economy, and then democratized, became presidential democracies, while the opposite sequence is observed for both forms of government.

⁷ The specification of the hazard rate includes democratic capital, lagged per capita income, a dummy variable for war years (current and lagged), dummy variables for democracy at independence, colonial origin, geographic location, socialist legal origin, and a linear and quadratic time trend.

⁸ Transition years (defined as the year of the change in regime and the immediately preceding year) are omitted from the sample, to ensure that the results are not just due to unrest during democratic transitions. Results are very similar for growth on the shorter sample 1960-00 (still using the Maddison data for per capita income).

⁹ Earlier empirical work by Adam Przeworski et al (2000) considers the effect of expected regime changes on economic growth in the post war period.

Table 1 Effects of political and economic reforms on economic growth (1960-2000)

	(1) Growth	(2) Growth	(3) Growth	(4) Growth
Democracy	0.75** (0.34)		0.81** (0.33)	0.70* (0.33)
Liberalization		1.31*** (0.39)	0.92** (0.39)	1.22*** (0.43)
Democracy after liberalization				1.62* (0.86)
Liberalization after democracy				-1.71*** (0.62)
N. of countries	138	134	130	130
N. of observations	4338	4492	4229	4229
Adj. R-square	0.10	0.09	0.11	0.12

Notes: Robust standard errors in parentheses: * significant at 10%; ** significant at 5%; *** significant at 1%

Control variables: country and year fixed effects, lagged income, dummy variable for wars and lagged wars, dummy variable for former socialist countries in Central and Eastern Europe plus former Soviet Union after 1990, year dummy variables interacted with dummy variables for Latin America, Africa, Asia and Socialist legal origin

Table 2 Forms of democracy, growth and economic policies (1960-2000)

	(1) Growth	(2) Government consumption	(3) Liberalization
Democracy	1.00** (0.51)	– 1.87*** (0.54)	– 0.07*** (0.02)
Parliamentary democracy	– 1.61*** (0.59)	4.89*** (0.79)	0.11*** (0.04)
Proportional democracy	0.16 (0.49)	1.15** (0.49)	0.11*** (0.03)
N. of countries	138	150	132
N. of observations	4338	4552	4578
Adj. R-square	0.14	0.20	0.47

Notes: Robust standard errors in parentheses: * significant at 10%; ** significant at 5%; *** significant at 1%

Control variables: country and year fixed effects, dummy variable for wars and lagged wars, dummy variable for former socialist countries in Central and Eastern Europe plus former Soviet Union after 1990, year dummy variables interacted with dummy variables for Latin America, Africa, Asia and Socialist legal origin

**Table 3 Expected and actual democracy,
growth, and investment**

	(1)	(2)	(3)
	Democracies	Autocracies	Full sample
Panel A Dependent variable: Growth 1850-2000			
Hazard rate out of current regime	- 20.05*** (5.51)	- 17.85 (11.93)	
Democracy			1.04* (0.62)
Probability of autocracy			0.47 (0.73)
Prob. of autocracy in lagged democracy			- 3.42 (2.52)
N. of countries	107	117	148
N. of observations	3656	4130	8135
Adj. R-square	0.19	0.12	0.10

Panel B Dependent Variable: Investment 1960-2000

Hazard rate out of current regime	- 27.03** (12.10)	- 19.43 (21.73)	
Democracy			0.38 (0.64)
Probability of autocracy			0.00 (0.75)
Prob. of autocracy in lagged democracy			- 2.30 (4.15)
N. of countries	94	84	131
N. of observations	1840	1897	4080
Adj. R-square	0.14	0.12	0.11

Notes: Robust standard errors in first parentheses: significant at 10%; ** significant at 5%; *** significant at 1%

Control variables: country and year fixed effects, lagged income, dummy variable for wars and lagged wars, dummy variable for former socialist countries in Central and Eastern Europe plus former Soviet Union after 1990

Transition years excluded from columns 1-4; an indicator for transition years is included in columns 5-6.