Democracy Does Cause Growth*

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Abstract

We provide evidence that democracy has a significant and robust positive effect on GDP per capita. Our empirical strategy controls for country fixed effects and the rich dynamics of GDP, which otherwise confound the effect of democracy on economic growth. To reduce measurement error, we introduce a new dichotomous measure of democracy that consolidates the information from several sources. Our baseline results use a dynamic panel model for GDP, and show that democratizations increase GDP per capita by about 20% in the long run. We find similar effects of democratizations on annual GDP when we control for the estimated propensity of a country to democratize based on past GDP dynamics. We obtain comparable estimates when we instrument democracy using regional waves of democratizations and reversals. Our results suggest that democracy increases GDP by encouraging investment, increasing schooling, inducing economic reforms, improving the provision of public goods, and reducing social unrest. We find little support for the view that democracy is a constraint on economic growth for less developed economies.

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1 Introduction

With the spectacular economic growth under nondemocracy in China, the eclipse of the Arab Spring, and the recent rise of populist politics in Europe and the United States, the view that democratic institutions are at best irrelevant and at worst a hindrance for economic growth has become increasingly popular both in academia and policy discourse. For example, the prominent *The New York Times* columnist Tom Friedman argues that:

> "One-party nondemocracy certainly has its drawbacks. But when it is led by a reasonably enlightened group of people, as China is today, it can also have great advantages. That one party can just impose the politically difficult but critically important policies needed to move a society forward in the 21st century" (Friedman, 2009).

while Robert Barro states this view even more boldly:

> "More political rights do not have an effect on growth" (Barro 1997, p. 1).

Although some recent contributions estimate a positive effect of democracy on growth, the pessimistic view of the economic implications of democracy is still widely shared. From their review of the academic literature until the mid-2000s, Gerring et al. (2005, p. 323) conclude that "the *net* effect of democracy on growth performance cross-nationally over the last five decades is negative or null."

In this paper we challenge this view. Using a panel of countries from 1960 to 2010, we estimate the impact on economic growth of the unprecedented spread of democracy around the world that took place in the last 50 years. The evidence suggests that democracy does cause growth, and its effect is significant and sizable.[1] Our estimates imply that a country that transitions from nondemocracy to democracy achieves about 20 percent higher GDP per capita in the next 25 years than a country that remains a nondemocracy. The effect of democracy does not depend on the initial level of economic development, though we find some evidence that democracy is more conducive to growth in countries with greater levels of secondary education.

The estimation of the causal effect of democracy (or a democratization) on GDP faces several challenges. First, existing democracy indices are subject to considerable measurement error, leading to spurious changes in democracy scores that do not correspond to real changes in democratic institutions.

Second, democracies differ from nondemocracies in unobserved characteristics, such as institutional, historical, and cultural aspects, that also impact their GDP. As a result, cross-country regressions, as

[1] Our specifications focus on the effect of democracy on the *level* of *log* GDP *per capita*, so that democratization affects growth in log GDP per capita. With some abuse of terminology, we will sometimes describe this as "the impact of democracy on economic growth" (rather than the impact of democratization on economic growth) or "the impact of democracy on GDP" (rather than on log GDP per capita). For brevity, we also often refer to GDP instead of GDP per capita.

those in Barro (1996, 1999), could be biased and are unlikely to reveal the causal effect of democracy on growth. Recent studies tackle this problem by using differences-in-differences or panel data estimates with country fixed effects.

Third, as shown in Figure 1, as well as in Acemoglu, et al. (2005) and Brückner and Ciccone (2011), democratizations are on average preceded by a temporary dip in GDP. This figure depicts GDP dynamics in countries that democratized at year zero relative to other countries that remained nondemocratic at the time. The pattern in this figure implies that the failure to properly model GDP dynamics, or the propensity to democratize based on past GDP, will lead to biased estimates of democracy on GDP. Though largely overlooked in previous work, the dip in GDP that precedes a democratization constitutes a clear violation of the parallel trends assumption that underlies the difference-in-differences or panel data estimates used in the literature. Modeling GDP dynamics would also enable an investigation of whether the impact of democratization on GDP is short-lived or gradual.

Last but not least, even if we control for country fixed effects and GDP dynamics, changes in democracy could be driven by time-varying unobservables related to future economic conditions, potentially leading to biased estimates.

In this paper, we address these challenges. We build on the important work by Papaioannou and Siourounis (2008) to develop a dichotomous measure of democracy, which combines several indices to purge spurious changes in each. We rely on this measure for most of our analysis, but also document the robustness of our results to other measures of democracy in the Online Appendix.

There is no perfect strategy for tackling the remaining challenges and estimating the causal effect of democracy on GDP. Our approach is to use a number of different strategies, which reassuringly all give very similar results. Our first approach uses a dynamic (linear) panel model for GDP, which includes both country fixed effects and autoregressive dynamics. The underlying economic assumption here is that conditional on the lags of GDP and country fixed effects, countries that change their democratic status are not on a differential GDP trend (and thus these lags successfully model the dip in GDP that precedes democratizations shown in Figure 1). This strategy leads to robust and precise estimates which indicate that in the 25 years following a permanent democratization GDP per capita is about 20% higher than it would be otherwise.

Our second strategy adopts a semi-parametric treatment effects framework in which democratization—the treatment—influences the distribution of potential GDP in all subsequent years. This strategy requires us to model the process of selection into democracy as a function of observables, in particular, lags of GDP (e.g., Jordà, 2005, Kline, 2011, or Angrist and Kuersteiner, 2011), but it does not rely on a parametric model for the dynamics of GDP, which affords us greater flexibility in estimating the time path of the impact of democracy on GDP. Related to our first approach, the economic assumption in this case is that conditional on the lags of GDP, countries that democratize are not on a differential

GDP trend relative to other nondemocracies. We show that this approach successfully controls for the influence of the dip in GDP preceding democratizations shown in Figure 1, and estimates that, following a democratization, GDP increases gradually until it reaches a level 20-25% higher than what it would reach otherwise.

These two strategies model the selection of countries into different regimes and control for the dip in GDP in Figure 1 as a function of their recent GDP per capita and time-invariant unobserved heterogeneity. However, they do not tackle the possibility that both democracy and GDP might be affected by time-varying omitted variables. Our third strategy confronts this challenge by using an instrumental-variables approach. The political science literature emphasizes that transitions to democracy often take place in regional waves (e.g., Huntington, 1991, Markoff, 1996). Based on this observation, we use regional waves in transitions to and away from democracy as an instrument for country-level democracy. Our instrumental-variables strategy exploits the diffusion of political regimes across countries in the same region and with common political histories. We pay special attention to distinguishing the diffusion of democracy from the role of regional economic shocks or the spread of economic conditions to nearby countries through trade and other mechanisms. By focusing on the variation created by regional waves of democratizations, our instrumental-variables strategy ensures that idiosyncratic changes in a country's political regime that may be endogenous to its growth do not bias our estimates. The resulting estimates of the impact of democracy on GDP are remarkably similar to those from our other two strategies: in our preferred specification, a democratization increases GDP per capita by about 25% in the first 25 years—though in some specifications the estimated effects are larger. This similarity bolsters our confidence that all three of our strategies are estimating the causal effect of democracy on GDP.

We further investigate the channels through which democracy increases GDP. Though our findings here are less clear-cut than our baseline results, they suggest that democracy contributes to future GDP by increasing investment, encouraging economic reforms, improving the provision of schooling and health care, and reducing social unrest. These results are consistent with, though of course do not prove, the hypothesis that democracies invest more in broad-based public goods and are more likely to enact economic reforms that would otherwise be resisted by politically powerful actors (e.g., Acemoglu, 2008). Although nondemocracies could also invest in public goods or enact far-ranging economic reforms, our results indicate that, at least in our sample, these countries are less likely to do so than democracies.

At the end of the paper, we turn to the common claim that democracy constraints economic growth for countries with low levels of development (e.g., Aghion, Alesina, and Trebbi, 2008, Posner, 2010, and Brooks, 2013). Our results do not support this view, but we do find that democracy has a larger impact on growth in countries where a greater fraction of the population has secondary schooling.

There is a substantial literature in political science that investigates, but does not reach a firm conclusion on, the empirical linkages between democracy and economic outcomes, summarized in part in Przeworski and Limongi (1993) and in Doucouliagos and Ulubasoglu's (2006) meta-analysis.

Cross-country regression analyses, such as Helliwell (1994), Barro (1996, 1999), and Tavares and Wacziarg (2001) have produced negative, though generally inconsistent, results.[2] More recent work, including Rodrik and Wacziarg (2005), Persson and Tabellini (2006), Papaioannou and Siourounis (2008), and Bates, Fayad and Hoeffler (2012), estimate positive effects using panel data techniques, though Murtin and Wacziarg (2014), Burkhart and Lewis-Beck (1994), and Giavazzi and Tabellini (2005) estimate insignificant effects on growth using similar strategies.[3] These and other papers in this literature differ in their measure of democracy and choice of specifications, and neither systematically control for the dynamics of GDP nor address the endogeneity of democratizations. Although some of the papers in this literature control for lags of GDP in some of their specifications (e.g., Persson and Tabellini, 2006, Papaioannou and Siourounis, 2008, and Murtin and Wacziarg, 2014), they do not emphasize the importance of GDP dynamics and the bias that results from not appropriately controlling for the dip in GDP shown in Figure 1. The failure to recognize this point may in fact explain the divergent results in the literature: because growth rates are less serially correlated than GDP, contributions that focus on growth as the dependent variable tend to find positive effects, while studies that estimate models in levels generally find no effects—unless they model the dynamics of GDP like we do.

Persson and Tabellini (2008), too, use propensity score techniques to estimate the impact of democracy. However, they only focus on changes in the average growth rate of countries after a democratization, and do not develop the semi-parametric approach we use here nor model the selection into democracy as a function of lags of GDP. Also related is recent independent work by Meyersson (2015), who estimates the effect of successful coups on economic growth by comparing them to unsuccessful coups.

[2]Another related literature investigates the effect of economic growth on democracy (e.g., Lipsett, 1959). We do not focus on this relationship here, though Figure 1 clearly implies a very different pattern: temporary drops in GDP make transitions to democracy more likely. In addition, confirming that this is a robust property of the data, we also confirm that, consistent with Acemoglu et al. (2008, 2009), the level of GDP has no effect on democratizations, but it does have some impact on transitions to nondemocracy.

[3]A smaller literature focuses on the effects of democracy on other economic outcomes. For example, Grosjean and Senik (2011), Rode and Gwartney (2012), and Giuliano, Mishra, and Spilimbergo (2013) look at the effect of democracy on economic reforms. Ansell (2010) looks at its impact on educational spending. Gerring, Thacker and Alfaro (2012), Blaydes and Kayser (2011), Besley and Kudamatsu (2006), and Kudamatsu (2012) investigate its impact on health, infant mortality and nutrition outcomes. Reynal-Querol (2005) and Cervellati and Sunde (2013) look at its impact on civil war. A more sizable literature looks at the effects of democracy on redistribution and inequality, and is reviewed and extended in Acemoglu et al. (2013). There is also a growing and promising literature that investigates the impact of democracy using within-country differences in the extent of democratic and electoral institutions (see, among others, Martinez-Bravo et al., 2012, Naidu, 2012, and Fujiwara, 2015).

We also build on and complement Persson and Tabellini (2009), who also exploit variation in geographically proximate neighbors' democracy (or more precisely, an inverse distance-weighted average of democracy among "neighbors"; see also Ansell, 2010, Aidt and Jensen, 2012, and Madsen et al., 2015). Using this approach, Persson and Tabellini estimate the impact of a country's "democratic capital" on growth. Unlike us, they do not instrument for democracy using regional waves, but use the distance-weighted average of democracy among "neighbors"to control for the transitions in and out of democracy in a regression that focuses on the impact on growth of a country's historical experience with democracy. Besides differences in question and specification, our instrumental variables strategy differs from theirs in that we focus on regional waves of democratization for countries with common political histories. We document below that regional waves have much greater and more robust explanatory power on the likelihood of democracy for a given country than variation coming from proximate neighbors' democracy.

The rest of the paper is organized as follows. The next section discusses the theoretical and empirical literature on the relationship between democracy and growth. Section 2 describes the construction of our democracy index, and provides data sources and descriptive statistics for our sample. Section 3 presents our dynamic panel model results. This model is estimated using the standard within estimator and various Generalized Method of Moments (GMM) estimators. This section also presents a variety of robustness checks. Section 4 introduces the treatment effects framework and presents results from our semi-parametric strategy. Section 5 presents our results obtained by instrumenting democracy with regional democratization waves. Section 6 presents evidence on potential channels through which democracy affects growth. Section 7 investigates the heterogeneous effects of democracy depending on the level of economic development and education. Section 8 concludes. We present several additional exercises in our Online Appendix.

2 Data and Descriptive Statistics

We construct an annual panel that comprises 175 countries from 1960 to 2010, though not all variables are available for the entire sample. In order to address the issue of measurement error in democracy indices, we create a consolidated and dichotomous measure of democracy. Following Papaioannou and Siourounis (2008), our index combines information from several datasets, including Freedom House and Polity IV, and only considers a country as democratic when several sources classify it as such. In the Online Appendix we explain in detail the construction of our measure; here we provide an overview. We code our dichotomous measure of democracy in country c at time t, D_{ct}, as follows. First, we consider a country as democratic during a given year if Freedom House codes it as "Free"or "Partially Free," and Polity IV assigns it a positive score. When one of these two sources is unavailable, we verify if the country is also coded as democratic by Cheibub, Gandhi, and Vreeland (2010) or Boix, Miller, and Rosato (2012). (These two datasets extend the popular Przeworski et al., 2000, dichotomous measure of democracy). Many of the democratic transitions detected in this manner are studied in detail by Papaioannou and Siourounis (2008), who use historical sources to date the exact year of the transition. When possible, we also draw on their data to verify the date of a democratization event.

Our measure of democracy covers 184 countries from 1960 to 2010, and is available for all the years during which a country was independent.[4] By 1960, 31.5% of the countries that exist in the world today were democracies. By 2010, this percentage had increased to 64.1%, which shows the unprecedented spread of democracy we study in this paper. Our measure identifies 122 democratizations and 71 reversals from democracy to nondemocracy. The countries and years in which these events took place are listed in the Online Appendix Tables A1 and A2. Not surprisingly, our democracy measure is highly correlated with the Freedom House and Polity indices, as well as the Cheibub, Gandhi, and Vreeland (2010) and Boix, Miller, and Rosato (2012) measures.

The major difference between our measure of democracy and that of Papaioannou and Siourounis is that theirs only considers *permanent* transitions to democracy. By only considering democratizations that are not reversed, their index encodes information on the future state of democratic institutions, which exacerbates the endogeneity concerns when it is included as a right-hand side variable in GDP regressions. Instead, we code both permanent and transitory transitions to democracy and nondemocracy. For example, our measure of democracy indicates that Argentina had a short spell of democracy from 1973 to 1976, when it held general elections for the first time in ten years. This spell was interrupted by a military coup in 1976, which put a series of military dictators in power until 1983—a period we code as nondemocratic. Argentina returned to democracy again in 1983 when the collapse of the military junta gave way to general elections. While we code all such transitions, Papaioannou and Siourounis only code the permanent transition to democracy in 1983.

As our main outcome variable, we use the log of GDP per capita measured in year 2000 dollars, which we obtained from the World Bank Development Indicators. This measure is available for an

[4]Our measure of democracy captures a bundle of institutions that characterize electoral democracies. These institutions include free and competitive elections, checks on executive power, and an inclusive political process that permits various groups of society to be represented politically. To a lesser extent, our measure of democracy also incorporates the expansion of civil rights, which are taken into account in Freedom House's assessment of whether a country is free or not. Figure A2 in the Online Appendix shows that these institutional components covary strongly. Following a transition to democracy, we observe sharp improvements in the likelihood that the country holds free and competitive elections, enacts institutional constraints on the executive, and opens participation into the political system. The pattern in Figure A2 suggests that the effects we estimate correspond to the joint effects of this bundle of democratic institutions, which improve in tandem following a democratization.

Although our measure of democracy comprises the main characteristics of an electoral democracy, it leaves out other important *de facto* and *de jure* elements that are part of the broader set of inclusive institutions emphasized by Acemoglu and Robinson (2012). Consider for instance the case of North Korea. A democratization, according to our measure of democracy, would not transform it into South Korea. But in terms of political institutions, a democratization would get North Korea closer to the average electoral democracy in our sample, which includes countries such as Bangladesh, Indonesia, Kyrgyzstan, or Nepal. Though coded as democratic in 2010, these countries still struggle with clientelism, corruption, and lack of state capacity.

unbalanced panel of 175 countries from 1960 to 2010 that comprise our main sample. Additional covariates used include: investment, trade (exports plus imports), secondary and primary enrollment, and infant mortality from the World Bank Development Indicators; financial flows (net foreign assets over GDP) from Lane and Milesi-Ferretti (2007); TFP from the Penn World Tables constructed by Feenstra et al. (2015) ; tax revenues from Hendrix (2010); and an index of economic reforms coded by Giuliano, Mishra and Spilimbergo (2013). Finally, using Banks and Wilson's (2013) Cross-National Time-Series Data Archive, we construct a dichotomous measure of social unrest that indicates the occurrence of riots and revolts. In some of our exercises we group countries in seven geographic regions following the World Bank classification. These regions are Africa, East Asia and the Pacific, Eastern Europe and Central Asia, Western Europe and other developed countries, Latin America and the Caribbean, the Middle East and the North of Africa, and South Asia.

Table 1 presents descriptive statistics for our variables separately for democracies and nondemocracies. The raw data show several well-known patterns, including, for example, that democracies are richer and have more educated populations.

3 Dynamic Panel Estimates

In this section, we provide our baseline results using a dynamic (linear) panel model for GDP.

3.1 Baseline Results

Our first approach to estimating the effects of democracy on GDP is to posit a full dynamic model for GDP:

$$y_{ct} = \beta D_{ct} + \sum_{j=1}^{p} \gamma_j y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{1}$$

where y_{ct} is the log of GDP per capita in country c at time t, and D_{ct} is our dichotomous measure of democracy in country c at time t. The α_c's denote a full set of country fixed effects, which will absorb the impact of any time-invariant country characteristics, and the δ_t's denote a full set of year fixed effects. The error term ε_{ct} includes all other time-varying unobservable shocks to GDP per capita. The specification includes p lags of log GDP per capita on the right-hand side to control for the dynamics of GDP as discussed in the Introduction.

Letting t_0 denote the first year in the sample (1960), we impose the following assumption:

Assumption 1 (sequential exogeneity): $\mathbb{E}(\varepsilon_{ct}|y_{ct-1},\ldots,y_{ct_0},D_{ct},\ldots,D_{ct_0},\alpha_c,\delta_t) = 0$ for all y_{ct-1}, $\ldots$, y_{ct_0}, D_{ct}, $\ldots$,D_{ct_0}, α_c, and δ_t, and for all c and $t \geq t_0$.

This is the standard assumption when dealing with linear dynamic panel models. It implies that democracy and past GDP are orthogonal to contemporaneous and future shocks to GDP, and that the error term ε_{ct} is serially uncorrelated. It requires sufficiently many lags of GDP to be included in equation (1) both to eliminate the residual serial correlation in the error term of this equation and to remove the influence of the dip in GDP that precedes a democratization.[5]

Economically, this assumption imposes that countries that transition to or away from democracy are not on a different GDP trend relative to others with similar levels of GDP in the past few years (captured by the lags of GDP) and level of long-run development (captured by country fixed effects). This is a strong assumption but it is not implausible. Besides controlling for the fact that democratizations are more frequent after economic crises, the lags of GDP per capita summarize the impact of a range of economic factors that affect both growth and democracy, such as commodity prices, agricultural productivity, and technology. Indeed, many of these economic factors should impact future GDP primarily through their influence on current GDP. As our results in Section 6 show, various policy and other institutional outcomes, such as taxes and a range of economic reforms, also change following democratization. But we do not view these changes as confounding our estimates of the effects of democracy, since they constitute some of the channels via which democracy impacts economic outcomes. Finally, our confidence in the plausibility of Assumption 1 is bolstered by the fact that controlling for a variety of economic factors and potential sources of differential trends in Table 4 has very little impact on our estimates, and our instrumental-variables strategy in Section 5, which filters out country-specific changes in democracy, yields broadly similar estimates as well. This triangulation of evidence suggests that controlling for lags of GDP and country fixed effects is successfully accounting for the selection of countries into democracy.

In addition, we assume throughout this section that GDP and democracy follow stationary processes (conditional on country and year fixed effects). This assumption guarantees that the dynamic panel estimators that we use are consistent and have well-behaved limit distributions. We discuss and statistically test this assumption below.

Under Assumption 1 and stationarity, equation (1) can be estimated using the standard within estimator.[6] Columns 1-4 of Table 2 report the within estimates controlling for different numbers of

[5]It is also useful for comparison with our second strategy to note that equation (1) can be interpreted as specifying the treatment effects of a transition to democracy (or a reversal). Anticipating notation we introduce in the next section, let $\Delta y^s_{ct}(d) = y^s_{ct}(d) - y_{ct-1}$ denote the potential change in (log) GDP per capita from time $t-1$ to time $t+s$ for a country with a change in political regime to $d \in \{0,1\}$ at time t. Then the "treatment effect" implied by equation (1) is:

$$\beta^0 = \mathbb{E}\left(\Delta y^0_{ct}(1) - \Delta y^0_{ct}(0) | D_{ct} = 1, D_{ct-1} = 0\right) = \beta.$$

Moreover, for a permanent transition to democracy, as we define below, and for all $s \geq 1$, β^s is determined recursively as $\beta^s = \beta + \sum_{j=1}^{p} \gamma_j \beta^{s-j}$ (with the convention that $\beta^s = 0$ for all $s < 0$).

[6]For future reference, we note that this involves the following "within transformation,"

$$y_{ct} - \frac{1}{T_c}\sum_s y_{cs} = \beta\left(D_{ct} - \frac{1}{T_c}\sum_s D_{cs}\right) + \sum_{j=1}^{p}\gamma_j\left(y_{ct-j} - \frac{1}{T_c}\sum_s y_{cs-j}\right) + \delta_t + \left(\varepsilon_{ct} - \frac{1}{T_c}\sum_s \varepsilon_{cs}\right),$$

with T_c being the number of times a country appears in the estimation sample. The within estimator has an asymptotic bias of order $1/T$ when D_{ct} and y_{ct-j} are sequentially exogenous and GDP is stationary. Thus, for long panels, as the lags. Throughout, the reported coefficient on democracy is multiplied by 100 to ease its interpretation, and we report standard errors robust against heteroskedasticity.

The first column of the table controls for a single lag of GDP per capita. In a pattern common with all of the results that we present, we find a sizable amount of persistence in GDP, with a coefficient on lagged (log) GDP of 0.973 (standard error = 0.006). Consistent with the stationarity assumption, this coefficient is significantly less than 1.

The democracy variable is also estimated to be positive and highly significant, with a coefficient of 0.973 (standard error = 0.294). From the estimates in Table 2, we can also derive the long-run effect of a *permanent* transition to democracy, defined as the impact on $y_{c\infty}$ of a switch from $D_{ct-1} = 0$ to $D_{ct+s} = 1$ for all $s \geq 0$. Given the estimate in Table 2 of about a 1% per year increase in GDP per capita following such a permanent transition to democracy, the dynamic process for GDP in equation (1) fully determines how the effects on GDP unfold over time. These estimates imply that such a permanent transition increases GDP per capita by about 1.97% one year after democratization, by about 2.9% the year after, and so on. Iterating this calculation, the cumulative long-run effect of a permanent transition to democracy on GDP is

$$\frac{\widehat{\beta}}{1 - \sum_{j=1}^{p} \widehat{\gamma}_j}, \tag{2}$$

where a hat ("ˆ") denotes the parameter estimates.[7] Applying this formula to the estimates from column 1, we find that a permanent transition to democracy increases GDP per capita by 35.59% in the long run (standard error=14%). In the table, we also report the impact of a permanent transition to democracy after 25 years, which is computed similarly and estimated to be 17.8% in this case (standard error=5.7%).[8]

Column 2 adds a second lag of GDP per capita. Though the implied dynamics are now richer (with the first lag being positive and greater than 1, while the second one is negative), the overall amount of persistence of GDP, reported in the row at the bottom of the table, is close to that found in column 1. The long-run effect of a permanent democratization is now smaller and equal to 19.6%.

Column 3, which is our preferred specification, includes four lags of GDP per capita. The overall pattern is very similar to that of column 2. The coefficient on our democracy variable is now 0.787 (standard error=0.226), and the implied long-run impact is a 21.24% (standard error=7.21%) increase in GDP per capita.

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one we use, the within estimator provides a natural starting point.

[7] For future reference, this formula is written for the general case with multiple lags on the right-hand side. Note also that because it is a ratio of estimates, equation (2) will have a small sample bias. Our Monte Carlo exercise in the Online Appendix shows that this bias tends to attenuate the positive long-run effect of democracy on growth.

[8] Here, we computed the long-run impact of a permanent transition to democracy compared to a counter-factual path in which a country never democratizes. Table A3 in the Online Appendix provides an alternative calculation in which we take into account the possibility that the country may still democratize at other time in the future.

Figure 2 plots the time path of the effects on GDP from a permanent transition to democracy at time 0 (defined as above), together with the 95% confidence interval for these estimates. As argued above, this time path is fully determined by the estimated dynamic process for GDP. We find that 25 to 30 years after a transition to democracy, most of the long-run gains from democracy in terms of GDP are realized and GDP is about 20% higher.

Column 4 includes four more lags of GDP (for a total of eight lags). We do not present their coefficients and just report the p-value for a joint test of significance, which suggests they do not jointly affect current GDP. The overall degree of persistence and the long-run impact of democracy on GDP per capita are very similar to the estimates in column 3.

The within estimates of the dynamic panel model in columns 1-4 have an asymptotic bias of order $1/T$, which is known as the Nickell bias. This bias results from the failure of strict exogeneity in dynamic panel models (Nickell, 1981, Alvarez and Arellano, 2003). Because T is fairly large in our panel (on average, each country is observed 38.8 times), this bias should be small in our setting, which motivates our use of the within estimator in columns 1-4 as a natural starting point.

The rest of Table 2 reports various GMM estimators that deal with the Nickell bias, and produce consistent estimates of the dynamic panel model for finite T. The sequential exogeneity assumption implies the following moment conditions

$$\mathbb{E}[(\varepsilon_{ct} - \varepsilon_{ct-1})(y_{cs}, D_{cs+1})'] = 0 \text{ for all } s \leq t-2.$$

Arellano and Bond (1991) develop a GMM estimator based on these moments. In columns 5-8, we report estimates from the same four models reported in columns 1-4 using this GMM procedure. Consistent with our expectations that the within estimator has at most a small bias, the GMM estimates are very similar to our preferred specification in column 3. The only notable difference is that GMM estimates imply a slightly smaller persistence for the GDP process, which leads to smaller long-run impacts than in column 3. For example, in column 7, which presents the GMM estimates of our preferred specification with four lags, we find a long-run impact of democracy on GDP per capita of 16.45% (standard error=8.436%).

In addition, the bottom rows in columns 5 to 8 report the p-value of a test for serial correlation in the residuals of equation (1). This is a test for AR2 correlation in the first-differenced residuals, the absence of which is required for consistent estimation (and where the first-differencing is because Arellano and Bond's estimator takes first differences of the model in equation (1)). The p-values for this test indicate that we reject the assumption of no serial correlation in the residuals when we include fewer than 4 lags; this is not surprising in view of the fact that such a sparse lag structure does not adequately control for the dynamics of GDP per capita. More importantly, the assumption of no serial correlation cannot be rejected when we include four or more lags, as in our preferred specification in column 7.

One drawback of the Arellano and Bond GMM estimator is that the number of moment conditions is of the order of T^2. Thus, for large values of T, we have a version of the "too many instruments" problem, which leads to an asymptotic bias of order $1/N$ in our GMM estimates (see Alvarez and Arellano, 2003).[9] To address this issue, we use an alternative estimator proposed by Hahn, Hausman, and Kuersteiner (2002), which is unbiased when N and T are both large, Assumption 1 holds and GDP is stationary.[10] We refer to this procedure as the HHK estimator throughout the paper. The results using this estimator are reported in columns 9-12. Once we include four or more lags, they are similar to the within estimates. For example, in column 11, which corresponds to our preferred specification, the long-run effect of a permanent transition to democracy on GDP is estimated as 25.03% (standard error=10.581%).

We carried out a number of tests to check stationarity and also verified the robustness of our main findings to a unit root or to near-unit root levels of persistence in the GDP process. First, we use Levin, Lin, and Chu's (2002) test for the presence of a unit root in GDP. Below each of our within estimates, we report in the bottom rows in Table 2 adjusted t-statistics from Levin, Lin, and Chu's test for unit roots. In all cases, the presence of a unit root in GDP is comfortably rejected.[11]

As a second strategy, we explicitly allow GDP to have a unit root. We estimate a transform of equation (1) that rearranges the original equation under the assumption of a unit root to obtain

$$\Delta y_{ct} = \beta D_{ct} + \sum_{j=1}^{p} \gamma'_j \Delta y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{3}$$

[9] In our estimates, we have used Arellano and Bond's estimator with a fixed and *ad hoc* weighting matrix with 2's on the main diagonal and -1's on the two main subdiagonals above and below it. As shown in Alvarez and Arellano (2003) and Hayakawa (2009), this estimator remains consistent when T is large. The efficient GMM estimator requires the estimation of a $T \times T$ weighting matrix, and could exhibit a severe bias when T is large.

[10] Hahn, Hausman, and Kuersteiner (2002) note that Arellano and Bond's GMM estimator is a minimum distance combination of estimates of the model

$$y^*_{ct} = \beta D^*_{ct} + \sum_{j=1}^{p} \gamma_j y^*_{ct-j} + \varepsilon^*_{ct},$$

obtained via 2SLS separately for $t = 1, 2, \ldots, T-1$ using $\{y_{cs}, D_{cs}\}_{s=1}^{t-1}$ as instruments. Here x^*_{ct} is the forward orthogonal deviation of variable x_{ct}, defined as

$$x^*_{ct} = \sqrt{\frac{T-t}{T-t+1}} \left(x_{ct} - \frac{1}{T-1} \sum_{s>t} x_{cs} \right).$$

They instead propose estimating the above equation for each t using a Nagar estimator with $\{y_{cs}, D_{cs}\}_{s=1}^{t-1}$ as instruments, which is robust to the use of many instruments. Specifically, this estimator is given by $\widehat{\beta} = (X'(I - kM_Z)X)^{-1}X'(I - kM_Z)Y$, where $k = 1 + \frac{L}{N}$, L is the degree of overidentifying restrictions, N the number of countries ($k = 1$ yields the usual 2SLS estimator), X is the vector of the endogenous right-hand side variables, Z denotes the vector of the instruments, Y is the dependent variable, and M_Z denotes orthogonal projection on Z (Nagar, 1959). We follow this procedure and also compute standard errors using 100 bootstrap repetitions.

[11] We should note, however, that the Levin, Lin, and Chu test requires two restrictive conditions to be satisfied: that the persistence of the GDP process is the same for all countries and that all cross-sectional dependence can be fully absorbed by year fixed effects.

When computing the test statistics for our unbalanced panel, we use the adjustment factors that Levin, Lin, and Chu (2002) suggest for the average length of our panel (38.8 years).

where $\gamma'_j = \left(\sum_{i=0}^{j} \gamma_i\right) - 1$ (in terms of γ_j in equation (1)). Table 3 reports within, GMM and HHK estimates of this equation, which all show similar positive effects of democracy on GDP. Because this specification assumes that democratizations have a permanent impact on the growth rate of GDP, the long-run effect on the level of GDP is not defined, and the cumulative effects of a democratization on GDP after 25 years are somewhat larger. The bottom row of this table indicates that the growth rate of GDP exhibits little persistence, confirming that these specifications are not affected by near-unit root dynamics.

Our third strategy to deal with unit root or near-unit root dynamics in the GDP process is to impose different levels of persistence for this process ranging from 0.95 to 1. To do so, we restrict the sum of the coefficients on lags of GDP, $\sum_{j=1}^{p} \gamma_j$ (which governs the overall amount of persistence), to be equal to 0.95, 0.96, 0.97, 0.98, 0.99, or 1. These models are obtained by replacing the left-hand side variable in equation (1) by $y_{ct} - \left(\sum_{j=1}^{p} \gamma_j\right) y_{ct-1}$, which implies that the right-hand side coefficients are given by $\gamma'_j = \left(\sum_{i=0}^{j} \gamma_i\right) - \rho$. We then estimate this restricted model using the within estimator. The results, reported in Table A4 in the Online Appendix, show that our findings are robust to assuming high levels of persistence for the GDP process. Because in these models the left-hand side variable and the regressors are stationary (provided that $\sum_{j=1}^{p} \gamma_j \leq 1.95$), and the persistence term is not estimated, our estimates are robust both to the potentially poor asymptotic behavior of the estimators near a unit root and to actual nonstationarity.

Finally, Table A5 in the Online Appendix presents Monte Carlo simulations confirming that the Nickell bias in our setting, even with near-unit root persistence in GDP, is very small, typically in the range of 1 to 5%, and also that this small Nickell bias induces essentially no bias in the estimates of the effect of democracy on GDP.[12]

Overall, these results give us confidence that our results are not unduly affected by the stationarity assumption. Motivated by this, we focus on the specification in levels with four lags of GDP for the rest of the paper.

[12]Specifically, we simulate counterfactual GDP processes using the parameter estimates as well as the estimates of the dispersion of country fixed effects obtained in column 3 of Table 2. We set the level of persistence in the GDP process as either 0.963 (as estimated in column 3), 0.97, 0.98, or 0.99. We then apply our standard within and GMM estimators to these simulated datasets. (The HHK estimator is asymptotically unbiased under these scenarios). The results confirm that there is a Nickell bias in the estimation of the degree of GDP persistence ranging from 1% to 5%, but more importantly, that there is essentially no bias in the estimation of the impact of democracy on GDP. Our results further indicate that inference based on the usual limit distributions of the within estimator remains valid. For example, the standard deviation of all the estimates of the democracy coefficient is 0.223, which roughly matches the estimated standard error of 0.226 presented in column 3 of Table 2. Two reasons likely account for the very small bias of the within and GMM estimator in our context. First, as already noted, our time dimension T is large. Second, there is considerable variation in country fixed effects. As noted by Alvarez and Arellano (2003) and Hayakawa (2009), the within and the GMM estimator perform better when the variance in unobserved heterogeneity is large relative to the variance of the shock in the GDP equation.

3.2 Robustness

The critical threats to the validity of the estimates reported so far come from the presence of time-varying economic and political factors that simultaneously impact democracy and GDP (country fixed effects absorb the time-invariant factors). We next investigate these threats. The results are reported in Table 4, which is structured in three panels: the top one presents results that use the within estimator, the middle one presents results that use Arellano and Bond's GMM estimator, and the bottom one is for the HHK estimator. To save space, we only report the estimates for the democracy coefficient, the implied long-run effects of democracy, and the cumulative effects on GDP 25 years after a democratization. Column 1 reproduces our baseline estimates for comparison.

The most obvious source of bias in our estimates would come from differential GDP trends among the countries that democratize. In column 2, we control for potential trends related to differences in the level of GDP at the start of our sample. To do so, we interact dummies for the quintile of the GDP per capita rank of the country in 1960 with a full set of year effects (to maximize our sample, we use Angus Maddison's GDP estimates for 1960, which are available for 149 countries). This specification identifies the effect of democracy by comparing countries that had similar levels of economic development at the start of our sample. These controls have very little impact on our results. The within estimate for the coefficient of democracy is now 0.718 (standard error=0.249), and the long-run effect is 22.17%. Arellano and Bond's GMM and the HHK estimates remain similar once these controls are included, though the effects of democracy are slightly smaller.[13]

In column 3, we verify that our results are not driven by the transition to democracy of Soviet and Soviet satellite countries. In particular, we add interactions between a dummy for Soviet and Soviet satellite countries and dummies for the years 1989, 1990, 1991, and post-1992. These controls have little impact on our results, and the long-run effect of democracy increases slightly to 24.86%.

The dip in GDP preceding democratization shown in Figure 1 might reflect the impact of unrest preceding transitions to democracy, which may also have long-lasting effects on subsequent growth. Motivated by this concern, and anticipating further issues that will be discussed in the context of our IV strategy in Section 5, we control in column 4 for four lags of unrest, with little effect on our results.

Democracy may be driven by external economic shocks (trade or financial flows) that also affect growth directly. To deal with this possibility, in column 5 we add four lags of trade exposure (import plus exports over GDP) and in column 6 we control for lags of external financial flows. These specifications need to be interpreted with some caution since trade and financial flows are endogenous to democracy. Nevertheless, the results are very similar to our baseline findings.

[13] The effect of democracy on GDP is also robust to the inclusion of country-specific linear trends, but in this case, because the persistence of GDP is estimated to be significantly lower, the long-run effects are considerably smaller. For example, using the within estimator, the coefficient of democracy is 0.91 (se=0.37), the persistence of GDP is estimated at 0.85, and the long-run effect of democracy on GDP is an increase of 6.1%.

Demographic changes could also affect growth and simultaneously increase the likelihood of democracy. To address this possibility, in column 7 we include as controls four lags of the log of population and four lags of the share of the population below 16 and the share above 64 (all from the World Bank Development Indicators). These controls also have little effect on our estimates.

In Section 5, we will exploit regional democratization waves as an exogenous source of variation in a country's likelihood of transitioning to democracy. Here, we would like to understand whether our baseline results are driven by differential movements in GDP and democracy across region $\times$ initial regime cells (which will be the level at which our instruments vary). In column 8 we answer this question by controlling for a full set of geographic region $\times$ initial regime $\times$ year effects. This ensures that the effect of democracy on GDP is identified from differences between countries in the same region and that had the same initial political regime (democracy or nondemocracy) at the start of our sample. Reassuringly, this strategy leads to estimates that are similar to our baseline results.[14]

The Online Appendix contains additional robustness checks. First, in Table A6 we explore whether our results are robust to using other measures of democracy. We find similar qualitative results using a dichotomous version of the Freedom House democracy index, Papaioannou and Siourounis's and Boix, Miller, and Rosato's measures of democracy. We find positive, though imprecise, estimates using a dichotomous measure based on the Polity index and Cheibub, Gandhi, and Vreeland's democracy-dictatorship measure as well. Importantly, the table further shows that, with any measure of democracy, not controlling for GDP lags leads to inconsistently signed and imprecise estimates of the effect of democracy on GDP. This exercise underscores the importance of correctly specifying and estimating the GDP dynamics. In Table A7, we show similar results using alternative measures of GDP per capita.

Second, in Table A8 in the Appendix, we explore the sensitivity of our baseline results to outliers. We estimate our preferred specification excluding countries with a standarized residual above 1.96 or below -1.96, and we also exclude observations with a Cook's distance above a common rule-of-thumb threshold (four divided by the number of observations). Finally, we report results using Li's (1985) and Huber's robust estimators. In all cases, the results, especially for the long-run effect of democracy, are very similar to our baseline results, establishing that our findings are not driven by outliers.

Third, in Table A9 we present alternative GMM estimators that either truncate the number of lags used to form moment conditions so as to lessen the finite-sample bias resulting from "too many instruments" in Arellano and Bond's GMM estimator, or add additional, nonlinear moment conditions

[14] The size of our estimates is also similar to that of our baseline 2SLS results contained in Table 6 below, even though they exploit an orthogonal source of variation.

Motivated by our IV specifications reported in Section 5, in additional exercises which we do not report, we also found similar estimates controlling for four lags of the average GDP per capita, average unrest and average trade (import plus exports over GDP) among countries in the same region $\times$initial regime cells. These controls take into account regional shocks among countries with similar political characteristics.

proposed by Ahn and Schmidt (1995). The estimates remain very similar to those in Table 2.[15]

Fourth, in Table A10, we explore separately the effect of democratizations and reversals (transitions from democracy to nondemocracy). Both democratizations and reversals yield similar results: democratizations increase GDP, and reversals reduce it. Though our estimates for reversals are less precise, we cannot reject the restriction that they are of equal size (in absolute value) to the effects of democratizations. These results are of interest not only because they are informative on the extent to which we expect GDP to decline following a transition to nondemocracy, but also because they refute the possible concern that our baseline findings reflect not the impact of democracy but the impact of *any* regime change on future GDP.

4 Treatment Effects and Semi-Parametric Estimates

In the previous section, we controlled for GDP dynamics using a dynamic (linear) panel model. This strategy allowed us to remove the confounding influence of the GDP dip shown in Figure 1 and compute the cumulative effects on GDP of a permanent transition to democracy. Though this approach is closely related to the most common one in the literature and enables efficient estimation under its maintained assumptions, it heavily relies on the linearity assumption. Linearity also imposes that the effects of transitions to and from democracy are the same in absolute value, and restricts the time pattern of the cumulative effects of democracy on GDP, which is derived by extrapolating the linear process for GDP into the future.

In this section we propose an alternative strategy to estimate the effects of a transition to democracy on the subsequent path of GDP by modeling the selection of countries into democracy, but without specifying a parametric process for GDP (though we still need to specify a model for either the likelihood of a transition to democracy or the conditional expectation of future GDP among nondemocracies—hence the label “semi-parametric”). We next explain this approach and then present our estimates.

4.1 Modeling Selection on Observables

Let us recap the notation for potential outcomes used already in footnote 5. Let $y^s_{ct}(d)$ denote the potential GDP level (in logs) at time $t+s$ for country c transitioning to either democracy or a nondemocracy at time t, denoted by $d \in \{0,1\}$. Specifically, for a country transitioning to democracy at t, we have $d = 1$ ($D_{ct} = 1, D_{ct-1} = 0$), and for one that remains in nondemocracy, we have $d = 0$ ($D_{ct} = D_{ct-1} = 0$). Let $\Delta y^s_{ct}(d) = y^s_{ct}(d) - y_{ct-1}$ denote the potential change in (log) GDP per capita

[15] We do not use the full set of moments exploited in Blundell and Bond (1998), however. The additional level instruments that they use is only justified when there is stationarity, which in our setting would make sense only if the cross-section of the countries at the beginning of our sample is very near the steady state. When this is not the case, as is likely in our application, these additional moments would lead to inconsistent estimates.

from time $t-1$ to time $t+s$ for a country with a change in political regime $d \in \{0,1\}$. With analogy to the treatment effects literature, we can think of $d \in \{0,1\}$ as corresponding to the "treatment," and $\Delta y^s_{ct}(d)$ for $s \geq 0$ as the potential outcomes affected by the treatment.

The causal effect of a transition to democracy at time t on GDP s periods thereafter for countries that are democratizing is

$$\beta^s = \mathbb{E}\left(\Delta y^s_{ct}(1) - \Delta y^s_{ct}(0) | D_{ct} = 1, D_{ct-1} = 0\right).$$

Unlike the estimates in the previous section, these effects are defined without making any parametric assumptions about the GDP process. Also, these estimates correspond not to the effect of a permanent democratization, but to the impact of a democratization at time t that may itself be reversed in subsequent years. Notice that because we are focusing on countries that are democratizing (as specified by conditioning on $D_{ct} = 1$ and $D_{ct-1} = 0$), these estimates correspond to the "treatment effects on the treated."

The challenge in estimating β^s is that countries that democratize may be different in terms of their potential outcomes than those that remain in nondemocracy. The key assumption that allows us to overcome this problem is that the selection into democracy can be modeled as a function of observables (lags of GDP and time effects in our case):

Assumption 2 (selection on observables): $\Delta y^s_{ct}(d) \perp D_{ct} | D_{ct-1} = 0, y_{ct-1}, y_{ct-2}, y_{ct-3}, y_{ct-4}, t$ for all $y_{ct-1}, \ldots, y_{ct-4}$, and for all c, t, and $s \geq 0$.

This assumption recognizes that transitions to democracy may be preceded by a dip in GDP, but also implies that, among nondemocracies, there are no other confounding factors that impact the propensity to democratize and that are related to potential outcomes. Note also that because we are focusing on transitions to democracy, Assumption 2 only imposes independence conditional on $D_{ct-1} = 0$.

The economic content of this assumption is similar to Assumption 1, which was the basis of our analysis in the previous section. Both assumptions condition on lags of GDP to model selection into democracy and to remove the GDP dip shown in Figure 1. Moreover, as already pointed out for Assumption 1, they both rule out time-varying omitted factors that affect both GDP and democracy. Yet they differ in how they incorporate the dynamics of GDP and unobserved fixed country characteristics. Assumption 1 restricts GDP dynamics to be linear, but allows for time-invariant unobserved country characteristics to shift GDP additively. On the other hand, Assumption 2 does not require us to specify the functional form of the dynamics of GDP or how fixed and unobserved country characteristics affect it. But it does so at the cost of restricting unobserved country heterogeneity to be common to all countries that are nondemocracies at time $t-1$ and that have experienced the same recent path for

GDP per capita. Put differently, Assumption 2 imposes one of the following two requirements: either omitted characteristics that affect both the likelihood of democratization and GDP growth (such as the GDP dip in Figure 1) are fully captured by the lags of GDP, or any such omitted characteristics are common to all nondemocracies at time $t-1$ (as would be the case for institutional features common to nondemocratic regimes), so that countries that democratize are not on a different trend relative to other nondemocracies with similar levels of GDP in the recent past. We will show that controlling for the lags of GDP indeed removes the GDP dip, providing partial support for the first requirement. The second requirement receives support from the evidence in Acemoglu et al. (2005) and Table A11 in the Appendix, which suggests that the economic factors that permanently increase a country's GDP do not increase the likelihood of democratization.[16]

4.2 Estimation under Selection on Observables

We next outline three alternative methods that rely on Assumption 2 to estimate the treatment effects of democracy. The first builds on Jordà (2005) and Kline (2011) and uses a linear regression of changes in GDP s years after democratization on year fixed effects and four lags of GDP (at years $t-1, t-2, t-3$ and $t-4$) for nondemocracies to form a counterfactual for countries that transition to democracy.[17] Although we are using a linear regression to estimate the counterfactual, we are not imposing linear dynamics for GDP (as we estimate the counterfactual separately for each s).

Our second approach follows Angrist and Kuersteiner (2011) and Angrist, Jordà and Kuersteiner (2013), and estimates the effect of democratizations on growth conditioning on the propensity score for transitions to democracy. We model and estimate this propensity score via a probit regression of the probability of transitioning to democracy (conditional on $D_{ct-1}=0$) on year fixed effects and four lags of GDP. We then estimate the causal effect of democracy on GDP using the efficient weighting scheme of Hirano, Imbens, and Ridder (2003), with the propensity score determining the weights given to different observations. This scheme gives greater weight to observations in the control group

[16] Neither assumption implies the other. Assumption 1 implies Assumption 2 in two important cases, however. First, when country fixed effects in the GDP equation are unrelated to democracy, as the evidence in Acemoglu et al. (2005) suggests. Second, when GDP levels in our sample are close to their respective steady-state values, in which case the lags of GDP can fully summarize the long-run differences across countries that might otherwise invalidate Assumption 2. Beyond these two cases, Assumption 1 could hold while Assumption 2 fails if, even conditional on the lags of GDP, there are unobserved fixed country characteristics impacting both GDP and democratization. Assumption 2 could hold while Assumption 1 fails if GDP dynamics are nonlinear or if there are factors that are common to nondemocracies, affect GDP, and vary over time.

[17] Specifically, in this approach the conditional expectation of $\Delta y^s_{ct}(0)$ is modeled as $\mathbb{E}\left[\Delta y^s_{ct}(0)|X_{ct}, D_{ct}=0, D_{ct-1}=0\right] = X'_{ct}\pi^s$. Thus the estimate of the effect of democracy on GDP s years after a transition to democracy is computed as

$$\widehat{\beta}^s = \widehat{\mathbb{E}}\left[\Delta y^s_{ct}(d)|D_{ct}=1, D_{ct-1}=0\right] - \widehat{\mathbb{E}}[X'_{ct}|D_{ct}=1, D_{ct-1}=0]\widehat{\pi}^s,$$

where $\widehat{\mathbb{E}}[X|S]$ denotes the sample average of X for all observations in a set S, $\widehat{\pi}^s$ denotes the OLS estimate of π^s, and the term $\widehat{\mathbb{E}}[X'_{ct}|D_{ct}=1, D_{ct-1}=0]\widehat{\pi}^s$ stands for the counterfactual cumulative (s-year) growth for countries that democratized at time t (meaning their growth had they not democratized).

(nondemocratizers) with a high propensity score which exhibit similar dynamics in GDP to those preceding a democratization, thus generating a control group comparable to democratizers.[18]

Our third approach combines these two into a "doubly-robust estimator" that both reweighs observations in the control group by their propensity score and adjusts the counterfactual outcome using a linear regression model. Intuitively, this estimator partials out the influence of covariates linearly and reweighs the data using the propensity score to obtain a control group comparable to democratizers. The doubly-robust estimator is consistent if either the linear model for potential outcomes or the probit model for democratizations is valid (see Imbens and Wooldridge, 2009).

Figure 3 depicts the estimates $\widehat{\beta}^s$ from the first approach for $s = -15, -14, \ldots, 30$, with $s = 0$ corresponding to the year of democratization. The estimates for negative values of s are included as a specification test (they should not be affected by subsequent democratization). The solid line plots the estimated effects of a democratization on GDP (in log points) over time, and the dotted lines plot its 95% confidence interval.[19] Reassuringly, we see no differential trend in GDP before democratization. Thereafter, there is a gradual increase in GDP, plateauing between 20 and 25 years at about 25%.

Panel A of Table 5 also summarizes these estimates by reporting the average effect over different time horizons. The estimates in this table confirm the lack of significant effects before democratization, which is reassuring. They show as well that between 20 and 25 years after a democratization, GDP increases by about 24% (standard error=7.7%).

Figure 4 plots the estimates, $\widehat{\beta}^s$, obtained from our second approach based on the propensity score. The pattern is similar to that in Figure 3, with no trends preceding the democratization and an impact of democracy on subsequent GDP that plateaus at about 24% between 20 and 25 years later. These estimates are also summarized in Panel B of Table 5 and are similar to the ones presented in Panel A of the same table.

Figure 5 and Panel C of Table 5 present the doubly-robust estimates, which are also similar to those from the previous two strategies. Once again, there is no evidence of a dip in GDP preceding democracy and the effects of democracy on GDP plateau at about 24% between 20 and 25 years.

Not only are the estimates from these three approaches very close to each other, but they are also

[18]The results from the probit model and the implied propensity scores are presented in Table A11 as well as in Figure A7, which confirms that the propensity scores for democratizers and nondemocratizers have a common support. Using the estimated propensity scores, $\widehat{P}_{ct}$, we compute the effect of democracy on GDP as $\widehat{\beta}^s = \widehat{\mathbb{E}}\left[\Delta y_{ct+j} \cdot \widehat{w}_{ct} \mid D_{ct-1} = 0\right]$, with weights:

$$\widehat{w}_{ct} = \frac{1}{\widehat{\mathbb{E}}(D_{ct})}\left(1\{D_{ct}=1\} - 1\{D_{ct}=0\}\frac{\widehat{P}_{ct}}{1-\widehat{P}_{ct}}\right).$$

Our "doubly robust estimator" computes the causal effect of democracy as $\widehat{\beta}^s = \widehat{\mathbb{E}}\left((\Delta y_{ct+j} - X'_{ct}\widehat{\pi}^s) \cdot \widehat{w}_{ct} \mid D_{ct-1} = 0\right)$, where $\widehat{\pi}^s$ is our estimate for the counterfactual model $\mathbb{E}\left[\Delta y^s_{ct}(0) | X_{ct}, D_{ct} = 0, D_{ct-1} = 0\right] = X'_{ct}\pi^s$.

[19]We implemented all estimators in this section using Stata 13's newly released `teffects` command and computed standard errors using 100 bootstrap samples in which we clustered the data at the country level. This takes into account the correlation among observations for the same country, which occurs naturally since our sample is a pooled cross section.

similar to the impact of democracy on GDP obtained from the dynamic linear panel model presented in the previous section. The congruence between the results of these approaches suggests that the specific parametrization of the GDP process is not playing an unduly important role in our conclusions.

Following an analogous procedure, we also estimate the effects of a reversal from democracy to nondemocracy on GDP. Figure A5 in the Online Appendix presents our findings. Though these estimates are less precise, they show that transitions to nondemocracy produce declines in GDP that are comparable (in absolute value) to the effects on GDP from a transition to democracy.[20]

Finally, in the Online Appendix Section A8 we discuss the details of two democratizations in our sample, Portugal and South Korea. Before their democratizations, both countries had low estimated propensity scores, indicating that democratization was not ex-ante likely. Following their democratizations, both countries experienced rapid subsequent growth relative to countries with similar estimated probabilities of democratizing. These case studies also illustrate some of the mechanisms via which democracy increases growth, which we explore systematically in Section 6 below.

5 IV Estimates: Democratization Waves

So far, our estimation strategies controlled for GDP dynamics and the influence of fixed unobserved characteristics in a number of ways. In this section, we develop an instrumental variables (IV) strategy to deal with time-varying omitted variables that may simultaneously affect the likelihood of democracy and GDP growth. Our IV strategy also alleviates concerns related to measurement error in our measure of democracy, and provides a different and complementary approach to the issue of endogenous selection into democracy (which our previous strategies confronted by conditioning on past GDP lags).

5.1 IV Strategy and Exclusion Restriction

As highlighted by the Arab Spring experience, democratizations and the social unrest that leads to a change of regime often occur in regional waves. Countries in Latin America and the Caribbean reverted from democracy to nondemocracy in the 1970s, and democratized again in the 1980s and early 1990s. The fall of the Soviet Union spurred a wave of democratizations in Eastern Europe, Central Asia, and Africa in the 1990s in what Huntington (1991) dubbed the "The Third Wave" (see also Markoff, 1996).[21]

Although there is no consensus on the factors that create such waves, the existing evidence suggests

[20] Our baseline estimates correspond to the treatment effects on the treated. Figure A6 in the Online Appendix presents estimates of the average treatment effect of democracy, which can be estimated under somewhat more restrictive assumptions than treatment effects on the treated (as they require the specification of counterfactual outcomes for democratizers) and have worse finite sample properties (because of the relatively low probability of a transition to democracy).

[21] Although Przeworski et al. (2000) challenge the existence of democratization waves, the consensus in political science is that democracy waves exist (Doorenspleet, 2000, Strand et al., 2012, Brinks and Coppedge, 2006, and Treisman, 2013).

that they are not explained by regional economic trends. For instance, as elaborated further below, Bonhomme and Manresa (2015) find that, even conditioning on GDP, transitions to democracy are correlated within regions. The most reasonable hypothesis is that this regional pattern reflects the diffusion of the demand for democracy (or more generally, dissatisfaction with a given regime) across countries within a region, which tend to have similar histories, political cultures, practical problems, and close informational ties (e.g., see Kuran, 1989, Lohmann, 1994, and Ellis and Fender, 2011, for theoretical models of the informational spread of political protests, and Buera, Monge-Naranjo and Primiceri, 2011, and Aidt and Jensen, 2012, for empirical evidence).[22]

Motivated by these observations, we exploit regional waves of democratization and transitions to nondemocracy as a source of exogenous variation in democracy.

We illustrate the existence of democratization waves in the top panel of Figure 6. For each of the seven regions described in Section 2, we compute the share of democracies among initial nondemocracies, and present its evolution over time relative to the year in which the first democratization in that region took place (we remove the first democratization in the region to avoid a mechanical increase in the share of democratic countries at time zero). For comparison, we also plot for the remaining regions the share of democracies among initial nondemocracies. Following the first democratization in a region, the share of countries that democratize in that region quickly converges to that of other regions, which illustrates the existence of waves of democratization. The bottom panel presents an analogous figure for the share of countries that transitioned from being democracies to nondemocracies.

To formalize the existence of waves, we first define the set of countries that may influence the demand for democracy in a given country. For each country c, let D_{ct_0} denote whether the country was a democracy or nondemocracy at the start of our sample, and let R_c denote the geographic region in which the country lies (using the seven regions introduced in Section 2). We posit that democracy in country c is influenced by democracy in the set of countries $I_c = \{c' : c' \neq c, R_{c'} = R_c, D_{c't_0} = D_{ct_0}\}$, which includes countries in the same region and that share a similar political history, meaning that $D_{c't_0} = D_{ct_0}$. Using these sets, we define our instrument as

$$Z_{ct} = \frac{1}{|I_c|} \sum_{c' \in I_c} D_{c't}. \tag{4}$$

Here, Z_{ct} is the jack-knifed average of democracy in a region $\times$ initial regime cell, which leaves out the own-country observation.

[22] Regional waves are not only emphasized in classic accounts of the democratizations process as mentioned above, but also appear to be more important than the spatial spread of democracy mediated purely by geographic distance. In Table A12 in the Appendix, we use the same formulation of regional waves introduced below and show that they have greater and more robust explanatory power for own-country democracy than neighbors' democracy or democracy in other countries weighted by the inverse of (geographic) distance. Further supporting ideas related to the diffusion of democratic demands or discontent with nondemocratic regimes, we also find a major regional component to social unrest. In contrast, GDP does not exhibit such a marked pattern of geographic correlation.

The corresponding two-stage least squares (2SLS) model we estimate is given by

$$\begin{aligned} y_{ct} &= \beta D_{ct} + \textstyle\sum_{j=1}^{p} \gamma_j y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct} \\ D_{ct} &= \textstyle\sum_{j=1}^{q} \pi_j Z_{ct-j} + \sum_{j=1}^{p} \phi_j y_{ct-j} + \theta_c + \eta_t + \upsilon_{ct}. \end{aligned} \tag{5}$$

This is identical to our dynamic panel model above, but we treat democracy as endogenous and instrument it using the lags of Z_{ct}.

Our key assumption in this section can be written as:

Assumption 3 (exclusion restriction): $\mathbb{E}(\varepsilon_{ct}|y_{ct-1},\ldots,y_{ct_0},Z_{ct-1},\ldots,Z_{ct_0},\alpha_c,\delta_t) = 0$ for all y_{ct-1}, $\ldots$, y_{ct_0}, Z_{ct},$\ldots$, Z_{ct_0}, α_c, and δ_t, and for all c and $t \geq t_0$.

Economically, this assumption amounts to imposing that conditional on lags of GDP and year and country fixed effects, the regional democratization wave variable Z_{ct-j} has no direct effect on the GDP per capita of country c at time t. Hence, our exclusion restriction requires that regional waves are significant determinants of democracy but are not themselves caused by regional trends in future GDP. This presumption is plausible for the reasons described above. This discussion also highlights the main threat to the validity of our instrumental-variables approach, which is a potential correlation between regional GDP and regional democracy (not working through the impact of a country's own democracy on its GDP). To verify that our results are not driven by such correlated regional trends, we present our estimates both with and without controlling for a range of other economic and political factors that may also spread across countries in the same region.

The key advantage of Assumption 3 over Assumptions 1 and 2 is that it allows for time-varying unobserved country heterogeneity, but requires that such heterogeneity not be related to past regional waves of democratization. Thus, idiosyncratic factors that simultaneously influence GDP and the likelihood of a democratic transition in a single country would violate Assumptions 1 or 2, but not Assumption 3. Particularly salient threats to Assumptions 1 and 2 include changes in social unrest or political discontent in a nondemocratic country that makes a democratic transition more likely while at the same time directly impacting future GDP, or a change in the structure of the economy leading to the rise of a middle class that simultaneously contributes to future growth prospects and articulates demands for a democratic transition. Because these types of idiosyncratic factors are not correlated with regional trends, they do not violate our Assumption 3 or bias our IV estimates.

5.2 First-Stage and 2SLS Estimates

The first-stage relations that underly our 2SLS estimates are shown in Panel B of Table 6. The sizable F-statistics for the excluded instruments, indicate that regional waves of democracy have a strong influence on the likelihood of democracy for countries in that region. In terms of time patterns, the largest impact is from the one-year lag Z_{ct-1}, though further lags of our instrument continue to have an effect.

Panel A of Table 6 presents our 2SLS estimates of equation (5). These estimates are consistent for large T and if the GDP process is stationary as in the dynamic panel model presented in Section 3. Column 1 presents the simplest 2SLS estimate using one lag of the instrument. The democracy coefficient is estimated at 0.966 (standard error=0.558), which is slightly larger than our baseline within estimates of the dynamic panel model. The implied long-run effect of a permanent democratization on GDP per capita is now 26.32% (standard error=17.07%), which is similar to the one obtained in the previous sections.

Consistent with our treatment of a country's own GDP dynamics, column 2 uses four lags of Z_{ct} as instruments. This specification leads to a slightly larger 2SLS coefficient of 1.149 (standard error=0.554) and a long-run effect of 31.52% (standard error=17.42%). The fact that our IV strategy produces somewhat larger effects of democracy on GDP may reflect a downward bias introduced by time-varying unobservables or the possibility of attenuation in our previous estimates due to measurement error in the index of democracy. The inclusion of several lags of Z_{ct} as instruments further allows us to perform a Hansen-type overidentification test, which provides no evidence of misspecification.

In columns 3 through 7 we probe the robustness of our results to the inclusion of time-varying covariates that could invalidate the exclusion restriction. The main concern throughout are other economic or political shocks that are correlated within regions and might simultaneously impact transitions to democracy and GDP.

In column 3, as in the OLS results, we control for a full set of interactions between GDP quintiles in 1960 and year dummies, which take out common shocks related to the initial level of development of different countries and which could be correlated within regions. In column 4, we include, as we did in Table 2, interactions between a dummy for Soviet and Soviet satellite countries and dummies for the years 1989, 1990, 1991, and post-1992, which verify that our results are not driven by the geographically concentrated transitions away from socialism. Both specifications lead to only modest changes in our first-stage and 2SLS estimates.

In columns 5 through 7, we directly control for regional economic variables that may influence the onset of a regional wave in democracy and economic conditions simultaneously. In column 5 we deal with unobserved regional heterogeneity by including region-specific trends. Panel B shows that these controls have little impact on our first-stage relationships, bolstering our confidence that regional democratization waves are not correlated with other regional trends. The resulting 2SLS estimates are somewhat larger in this case than before, but the implied long-run effects remain similar.

In columns 6 we control for observable shocks at the level of the region $\times$ initial regime cell. Intuitively, GDP in a country may be influenced by contemporary GDP or other economic variables such as trade patterns among countries in the same cell. We address these concerns by including average GDP and trade in each cell on the right-hand side. Because contemporaneous values of these variables are endogenous, we instrument them using four of their lags. Panel B once again shows a robust and similar first stage. The 2SLS estimate for democracy in Panel A is larger than the baseline, but with only modestly greater long-run effects. These results are particularly reassuring in conjunction with those reported in column 7 of Table 4, which showed very similar estimates when we directly controlled for a full set of region $\times$ initial regime cell $\times$ year effects, thus exploiting the complement of the variation being exploited here.

Regional correlation in political variables, such as unrest or political instability, can lead to a violation of our exclusion restriction if they spread across countries. To deal with this concern, column 7 extends the model in column 6 by also controlling for average unrest in each region $\times$ initial regime cell, instrumented using its lags. Because our results in Section 6 suggest that social unrest is endogenous to democracy, this is a demanding specification that may attenuate the impact of democracy on GDP. Nevertheless, the results remain similar to the baseline specification in column 2.

Columns 8 and 9 develop a complementary strategy against the threat posed by regionally-correlated omitted factors, and explicitly model the spatial correlation of GDP, y_{ct}, and GDP shocks, ε_{ct}. First, we allow GDP to be spatially correlated as a function of the inverse of the distance between countries. Specifically, in column 8 we include a weighted average $\mathbb{W}^d y_t$ of GDP in other countries as a covariate and instrument it using four of its lags (see Kelejian and Prucha, 1998, Anselin, 2001, and Lee, 2007 on the estimation of spatial panel models). Here, $\mathbb{W}^d$ is the $N \times N$ matrix of inverse distances between countries with zeros on the diagonal (where N is the number of countries), and y_t is a $N \times 1$ vector of GDP at time t in all countries. The results in this case continue to be precisely estimated and are similar to our baseline findings.

In column 9 we estimate a more demanding model in which we also allow the GDP shocks, ε_{ct}, to be spatially correlated. Specifically, let ε_t denote the $N \times 1$ column vector of time t error terms ε_{ct} in the GDP equation (5). We assume that ε_t satisfies the spatial auto-regressive process

$$\varepsilon_t = \lambda \mathbb{W}^d \varepsilon_t + \zeta_t, \tag{6}$$

where ζ_t is an error term that is independent across countries. This specification for the error term allows a fairly flexible pattern of correlation in GDP across countries.

To estimate this model, we must include the "spatial lags" of all of our right-hand side variables, $\mathbb{W}^d D_t$, $\mathbb{W}^d y_t$, $\mathbb{W}^d y_{t-1}$, $\mathbb{W}^d y_{t-2}$, $\mathbb{W}^d y_{t-3}$, and $\mathbb{W}^d y_{t-4}$ on the right hand side, and instrument them using their first four time lags. Hence, $\mathbb{W}^d D_{t-1}$, $\mathbb{W}^d D_{t-2}$, $\mathbb{W}^d D_{t-3}$, and $\mathbb{W}^d D_{t-4}$ are part of the instrument list. In this case, our model continues to be identified because the matrix of inverse distances, $\mathbb{W}^d$, that governs the spatial correlation of GDP does not coincide with the regional pattern that mediates democratization waves (which was specified in equation (4)).

Indeed, we find it plausible that the correlation of GDP shocks across countries depends on geographic distance, while democratization waves take place within regions since, as discussed in footnote 22, protests and discontent with nondemocracies appear to have a marked regional element. Consistent with this reasoning, the first stages shown in Panel B indicate that the relationship between regional democratization waves and country-level transitions to democracy is essentially unaffected by the inclusion of the inverse-distance-weighted GDP and democracy in other countries. Our 2SLS estimate in this case is of a similar magnitude but less precisely estimated. This is not surprising given the difficulty of separately estimating the spatial GDP correlation and the effect of regional democratization waves.

Panel C presents the corresponding HHK estimates described in Section 3, but now we use lags of Z_{ct} as external instruments for democracy.[23] This estimator is consistent for finite T as long as our exclusion restriction in Assumption 3 holds. The results are broadly similar to our IV estimates.

In the Online Appendix, we report a number of additional robustness checks for our IV estimates. Table A13 explores the sensitivity of our IV results to outliers. In addition, we investigated the sensitivity of our IV results to different constructions of the instrument in Table A14. For example, we find similar results when we construct the instrument using alternative codings of the initial regime or using finer distinctions among initial regimes (e.g., British colonies, French colonies, civil dictatorships, military dictatorships, mixed and presidential democracies, parliamentary democracies, royal dictatorships, and socialist regimes). We further constructed an alternative instrument computed as a jack-knifed average of democracy in each region interacted with a full set of region $\times$ initial regime dummies. This instrument produced similar results as well.

In summary, relying on the plausibly exogenous sources of variation in democracy resulting from regional democratization waves leads to estimates of the impact of democracy on GDP that are in the ballpark of our results in Sections 3 and 4. It is particularly reassuring that this IV strategy, which models selection into democracy and nondemocracy in an entirely different way than our two first strategies, nonetheless produces very similar estimates.

6 Mechanisms

In this section we use our design to explore the potential mechanisms via which democracy might affect growth, even though we cannot definitively distinguish across these mechanisms or rule out the possibility that there are other intermediating variables at work.

[23]In particular, using the notation from footnote 10, we estimate the model $y^*_{ct} = \beta D^*_{ct} + \sum_{j=1}^{p} \gamma_j y^*_{ct-j} + \varepsilon^*_{ct}$, with the Nagar estimator, separately for $t = 1, 2, \ldots, T-1$. We use $\{y_{cs}\}_{s=1}^{t-1}$ and $Z_{ct-1}, \ldots, Z_{ct-4}$ as instruments. These $T-1$ estimators are consistent (even with many instruments) and are again combined with efficient weights.

We estimate models of the form

$$m_{ct} = \beta D_{ct} + \sum_{j=1}^{p} \gamma_j y_{ct-j} + \sum_{j=1}^{p} \eta_j m_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{7}$$

where m_{ct} is one of several potential channels, including the share of investment in GDP (in logs), TFP (in logs), the measure of economic reforms introduced by Giuliano, Mishra and Spilimbergo, 2013, (normalized between 0 and 100), the share of trade in GDP (in logs), the share of taxes in GDP (in logs), primary school enrollment, secondary school enrollment, child mortality rates (in logs), and the social unrest dummy introduced above. Besides controlling for the dip in GDP that precedes a democratization, the lags of GDP on the right-hand side of (7) help remove the mechanical effect of greater GDP on some of these intermediating variables.

Table 7 presents estimates of equation (7) using the within estimator (corresponding to column 3 of Table 2), our preferred specification for the 2SLS estimator (corresponding to column 2 of Table 6, Panel A), and our preferred specification for the HHK estimator using the regional waves as instruments for democracy (corresponding to column 2 of our IV table, Table 6, Panel C).

In all specifications we find that democracy increases the likelihood of economic reforms, tax revenue as a percentage of GDP, and enrollment in primary and secondary education, and reduces child mortality (though for some of these variables the 2SLS estimates become considerably larger). We also obtain evidence of positive effects of democracy on investment and openness to trade, and negative estimates on social unrest, but these estimates are not precise in all specifications. Finally, we find no evidence of an impact of democracy on TFP.

Overall, these results suggest that democracy might be working through a number of channels. In particular, democracies seem to enact economic reforms that are conducive to growth. Democracies also seem to raise more taxes, but tend to invest more on public goods related to health and schooling, which may contribute to growth. In addition, democracy seems to reduce social unrest, which could also have a positive impact on economic growth. Of course, our strategy does not allow us to conclusively establish that these are the most important mechanisms, as they may be themselves outcomes of economic growth, but the fact that these variables increase following a democratization—even controlling for lags of GDP per capita—suggests that they are prime candidates for the channels through which democracy might cause growth.

7 Does Democracy Need Development?

As hinted in the Introduction, many critics of the view that democracy is good for economic performance suggest that democracy will be economically costly when certain preconditions, especially related to economic development and high human capital, are not satisfied. For example, Richard

Posner (2010) has argued "Dictatorship will often be optimal for very poor countries. Such countries tend not only to have simple economies but also to lack the cultural and institutional preconditions to democracy," while David Brooks (2013) stated in the wake of the Egyptian coup of 2013, "It's not that Egypt doesn't have a recipe for a democratic transition. It seems to lack even the basic mental ingredients."

We investigate this hypothesis by considering interactions between democracy and the level of economic development (as proxied by the log of GDP per capita) and human capital (as proxied by the share of the population with secondary schooling from the Barro-Lee dataset). If this hypothesis is valid, we would expect the interaction terms to be positive and significant in both cases, and the main effect of democracy for low economic development or for low schooling countries to be negative.

Table 8 presents the results from this exercise. We focus on the same three estimators as in Table 7 (the within estimator, the 2SLS estimator, and the HHK estimator instrumenting for democracy and its interactions). Columns 1 through 4 present interactions with the log of GDP per capita, and columns 5 through 8 present interactions with the share of the population with secondary schooling. In columns 1 and 5, we interact democracy with the baseline level of GDP per capita (column 1) and secondary education (column 5) that prevailed in 1960. In columns 2 and 6, we interact democracy with the baseline level of GDP per capita (column 2) and secondary education (column 6) that prevailed in 1970. In columns 3 and 7, we interact democracy with the baseline level of GDP per capita (column 3) and secondary education (column 7) that prevailed in 1980. Finally, in columns 4 and 8 we interact democracy with, respectively, the lagged level of GDP per capita (column 4) or secondary education in the last five years before each observation (column 8). In all models we evaluate the main effect of democracy at the bottom 25th percentile of the interaction variable, so that it indicates whether democracy has a negative effect for countries at a low level of economic development or with low levels of schooling.

The patterns in Table 8 are fairly clear. There is no significant interaction between democracy and the income level of a country that democratizes. The impact of democracy does not seem to depend on the level of development. Unlike popular claims in the literature, democracy does not have a negative effect for countries with low income levels. In fact, all of the main effects of democracy, which are computed for countries at the 25th income percentile, are positive and some are significant.

Only the interactions with the share of the population with secondary schooling play a significant role. These results, which are reported in columns 5-8, indicate that democracy is more conducive to growth in countries with more educated people than in others. Nevertheless, these interactions are quantitatively small; the effect of democracy is not negative even for countries at the 25th percentile of education in the top panel. Moreover, we do not find a similar pattern for the interactions between democracy and the share of the population with primary and tertiary education.

Our strategy does not reveal what drives the interaction with secondary schooling. It could be that, as some experts believe, democracy works better with a more literate and modernized population (though Acemoglu et al., 2005, and 2009, find no evidence that democracies are more stable or more likely to emerge when human capital is high) or, as suggested in Acemoglu and Robinson (2006) and Galor and Moav (2006), high human capital might reduce the stakes of distributional conflicts in society, making democracy more stable. Our preferred interpretation is the latter, in part because we do not find any evidence of significant interactions with other variables related to how modernized the population is, such as the income level.

8 Conclusion

Skepticism about the performance of democratic institutions is as old as democracy itself. Plato denigrated democracy as the second worst form of government after tyranny, arguing that "in democracy they [the class of idle spendthrifts] are almost the entire ruling power"(1908, p. 564). Aristotle similarly thought that "it is not safe to trust them [the bulk of the people] with the first offices in the state, both on account of their iniquity and their ignorance; from the one of which they will do what is wrong, from the other they will mistake" (1912, p. 86). The view that democracy is a constraint on economic growth has recently been gaining ground.

In this paper, we show that once the dynamics of GDP are controlled for in a fixed effects OLS regression, there is an economically and statistically significant positive effect of democracy on future GDP per capita. This result remains true in GMM estimates that account for any bias due to lagged dependent variables, as well as with semi-parametric estimators that model the propensity to transition to democracy (and nondemocracy) using lags of GDP. Our preferred specifications imply that long-run GDP increases by about 20-25% in the 25 years following a democratization.

We also document that democratizations take place in regional waves: a country is more likely to transition to democracy or nondemocracy when the same transition recently occurred in other countries in the same region. We exploit this source of variation to identify the effect of democracy on GDP. Using regional waves as an instrument for democracy, we corroborate our finding that democracy increases GDP.

The triangulation of evidence from dynamic linear panel data models, semi-parametric matching and instrumental-variables methods, all leading to fairly similar estimates of the impact of democracy on GDP, gives us confidence that there is a positive causal effect of democracy on economic growth. Our evidence also suggests that democracy fosters higher GDP by enacting economic reforms, improving fiscal capacity and the provision of schooling and health care, and perhaps also by inducing greater investment and lower social unrest.

In contrast to the popular claims that democracy is bad for growth at early stages of economic development, we find no heterogeneity by level of income. There is some heterogeneity depending on the level of human capital, but these effects are not large enough to lead to negative effects of democracy for countries with low human capital.

Taken together, our results suggest that democracy is more conducive to economic growth than its detractors have argued, and that there are many complementarities between democratic institutions and proximate causes of economic development. Work using cross-country and within-country variation to shed more light on how democracy alters economic incentives and organizations, and to pinpoint what aspects of democratic institutions are more conducive to economic success is an obvious fruitful area for future research. An exploration of the possibly more complex interactions between political regimes and economic outcomes, incorporating among other things nonlinear dynamics, multiple regime types and richer heterogeneous effects, is another important area of future inquiry.

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Table 1: Summary statistics for the main variables used in our analysis.

	Nondemocracies			Democracies		
	Obs.	*Mean*	*Std. dev.*	*Obs.*	*Mean*	*Std. dev*
GDP per Capita	3,376	$2,074.46	$3,838.65	3,558	$8,149.97	$9,334.83
Investment Share of GDP	3,225	0.2182	0.1023	3,340	0.2328	0.0741
TFP	1,863	1.0676	0.4056	2,744	0.9345	0.1646
Trade Share of GDP	3,175	0.7162	0.5106	3,485	0.7715	0.4104
Primary Enrollment Rate	2,861	90.29	29.51	2,823	101.60	15.86
Secondary Enrollment Rate	2,424	45.76	31.77	2,538	75.40	29.78
Tax Revenue Share of GDP	3,122	0.1587	0.0948	2,564	0.2075	0.0955
Child Mortality Per 1000 births	4,142	77.29	49.64	3,615	33.26	32.65
Unrest Dummy	3,739	0.2870	0.4524	3,610	0.2191	0.4137
Market Reforms Index (0-100)	3476	21.89	23.26	2,829	52.11	24.75

Notes: See the text for a full description of the variables and their corresponding sources. The table presents the statistics separately for nondemocracies (country/years for which our dichotomous democracy measure is 0) and democracies (country/years for which our dichotomous democracy measure is 1).

Table 2: Effect of democracy on (log) GDP per capita.

	Within estimates				Arellano and Bond estimates				HHK estimates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Democracy	0.973	0.651	0.787	0.887	0.959	0.797	0.875	0.659	0.781	0.582	1.178	1.682
	(0.294)	(0.248)	(0.226)	(0.245)	(0.477)	(0.417)	(0.374)	(0.378)	(0.455)	(0.387)	(0.370)	(0.352)
log GDP first lag	0.973	1.266	1.238	1.233	0.946	1.216	1.204	1.204	0.938	1.158	1.150	1.155
	(0.006)	(0.038)	(0.038)	(0.039)	(0.009)	(0.041)	(0.041)	(0.038)	(0.011)	(0.038)	(0.040)	(0.036)
log GDP second lag		-0.300	-0.207	-0.214		-0.270	-0.193	-0.205		-0.217	-0.127	-0.122
		(0.037)	(0.046)	(0.043)		(0.038)	(0.045)	(0.042)		(0.035)	(0.050)	(0.041)
log GDP third lag			-0.026	-0.021			-0.028	-0.020			-0.030	-0.040
			(0.028)	(0.028)			(0.028)	(0.027)			(0.026)	(0.024)
log GDP fourth lag			-0.043	-0.039			-0.036	-0.038			-0.039	-0.028
			(0.017)	(0.034)			(0.020)	(0.033)			(0.015)	(0.026)
p-value lags 5 to 8				[0.565]				[0.478]				[0.094]
Long-run effect of democracy	35.587	19.599	21.240	22.008	17.608	14.882	16.448	11.810	12.644	9.929	25.032	35.104
	(13.998)	(8.595)	(7.215)	(7.740)	(10.609)	(9.152)	(8.436)	(7.829)	(8.282)	(7.258)	(10.581)	(11.140)
Effect of democracy after 25 years	17.791	13.800	16.895	17.715	13.263	12.721	14.713	10.500	10.076	8.537	20.853	29.528
	(5.649)	(5.550)	(5.297)	(5.455)	(7.281)	(7.371)	(7.128)	(6.653)	(6.245)	(6.032)	(7.731)	(7.772)
Persistence of GDP process	0.973	0.967	0.963	0.960	0.946	0.946	0.947	0.944	0.938	0.941	0.953	0.952
	(0.006)	(0.005)	(0.005)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)	(0.011)	(0.010)	(0.009)	(0.009)
AR2 test p-value					[0.01]	[0.08]	[0.51]	[0.95]				
Unit root test $t-$statistics	-4.79	-3.89	-4.13	-7.00								
$p-$value (reject unit root)	[0.00]	[0.00]	[0.00]	[0.00]								
Observations	6,790	6,642	6,336	5,688	6,615	6,467	6,161	5,513	6,615	6,467	6,161	5,513
Countries in sample	175	175	175	175	175	175	175	175	175	175	175	175

Notes: This table presents estimates of the effect of democracy on log GDP per capita. The reported coefficient on democracy is multiplied by 100. Columns 1-4 present results using the within estimator. Columns 5-8 present results using Arellano and Bond's GMM estimator. The AR2 row reports the p-value for a test of serial correlation in the residuals of the GDP series. Columns 9-12 present results using the HHK estimator. In all specifications we control for a full set of country and year fixed effects. Columns 4, 8 and 12 include 8 lags of GDP per capita as controls, but we only report the p-value of a test for joint significance of lags 5 to 8. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

Table 3: Effect of democracy on the growth rate of GDP per capita.

	Within estimates				Arellano and Bond estimates				HHK estimates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Democracy	1.028	1.201	1.269	1.378	1.458	1.715	1.545	1.554	1.410	1.413	1.343	1.253
	(0.250)	(0.237)	(0.243)	(0.284)	(0.385)	(0.385)	(0.368)	(0.405)	(0.323)	(0.341)	(0.348)	(0.330)
GDP growth first lag	0.287	0.274	0.263	0.250	0.294	0.278	0.270	0.254	0.245	0.230	0.211	0.211
	(0.037)	(0.037)	(0.039)	(0.037)	(0.040)	(0.038)	(0.041)	(0.036)	(0.034)	(0.037)	(0.039)	(0.036)
GDP growth second lag		0.047	0.060	0.042		0.057	0.064	0.047		0.075	0.082	0.057
		(0.024)	(0.025)	(0.023)		(0.025)	(0.025)	(0.024)		(0.023)	(0.025)	(0.020)
GDP growth third lag			0.023	0.024			0.031	0.028			0.029	0.036
			(0.018)	(0.017)			(0.018)	(0.018)			(0.015)	(0.021)
GDP growth fourth lag			-0.033	-0.018			-0.022	-0.013			-0.007	-0.004
			(0.022)	(0.025)			(0.022)	(0.025)			(0.015)	(0.020)
p-value lags 5 to 8				[0.00]				[0.00]				[0.02]
Long-run effect of democracy (on growth rate)	1.443	1.768	1.845	1.960	2.065	2.579	2.349	2.273	1.867	2.032	1.961	1.790
	(0.360)	(0.372)	(0.370)	(0.441)	(0.559)	(0.624)	(0.602)	(0.644)	(0.448)	(0.511)	(0.527)	(0.488)
Level effect of democracy after 25 years	35.483	43.245	45.275	42.715	50.767	62.957	57.296	50.090	46.067	49.685	47.791	40.543
	(8.820)	(9.046)	(9.038)	(9.191)	(13.718)	(15.105)	(14.572)	(13.322)	(11.017)	(12.448)	(12.770)	(10.837)
Persistence of growth rate process	0.287	0.321	0.312	0.297	0.294	0.335	0.342	0.316	0.245	0.305	0.315	0.300
	(0.037)	(0.041)	(0.038)	(0.043)	(0.040)	(0.043)	(0.041)	(0.044)	(0.034)	(0.039)	(0.043)	(0.049)
AR2 test p-value					[0.10]	[0.81]	[0.98]	[0.93]				
Observations	6,642	6,490	6,178	5,523	6,467	6,315	6,003	5,348	6,467	6,315	6,003	5,348
Countries in sample	175	175	175	175	175	175	175	174	175	175	175	174

Notes: This table presents estimates of the effect of democracy on the growth rate of GDP per capita. The reported coefficient on democracy is multiplied by 100. Columns 1-4 present results using the within estimator. Columns 5-8 present results using Arellano and Bond's GMM estimator. The AR2 row reports the p-value for a test of serial correlation in the residuals of the growth rate series. Columns 9-12 present results using the HHK estimator. In all specifications we control for a full set of country and year fixed effects. Columns 4, 8 and 12 include 8 lags of the growth rate of GDP per capita as controls, but we only report the p-value of a test for joint significance of lags 5 to 8. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

Table 4: Effect of democracy on (log) GDP per capita controlling for covariates.

Covariates included:	(1)	GDP in 1960 quintiles × year effects (2)	Soviet dummies (3)	Lags of unrest (4)	Lags of trade (5)	Lags of financial flows (6)	Lags of demographic structure (7)	Region × regime × year effects (8)
	Panel A: Within estimates.							
Democracy	0.787	0.718	0.911	0.705	0.595	0.926	0.650	0.834
	(0.226)	(0.249)	(0.251)	(0.224)	(0.264)	(0.244)	(0.230)	(0.264)
Long-run effect of democracy	21.240	22.173	24.860	17.000	14.593	23.870	14.153	16.651
	(7.215)	(8.702)	(7.783)	(5.980)	(7.122)	(8.211)	(5.419)	(5.546)
Effect of democracy after 25 years	16.895	16.261	19.587	13.567	11.500	18.149	12.251	14.532
	(5.297)	(5.982)	(5.724)	(4.644)	(5.336)	(5.435)	(4.552)	(4.726)
Persistence of GDP process	0.963	0.968	0.963	0.959	0.959	0.961	0.954	0.950
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)	(0.005)	(0.005)
Observations	6,336	5,523	6,336	5,643	5,750	4,950	6,262	6,336
Countries in sample	175	149	175	171	172	171	172	175
	Panel B: Arellano and Bond estimates.							
Democracy	0.875	0.730	1.073	0.693	1.034	1.017	0.756	1.217
	(0.374)	(0.387)	(0.403)	(0.396)	(0.469)	(0.373)	(0.370)	(0.420)
Long-run effect of democracy	16.448	14.865	20.006	9.871	17.926	18.607	12.152	18.209
	(8.436)	(8.998)	(8.981)	(6.479)	(9.021)	(7.842)	(6.639)	(6.746)
Effect of democracy after 25 years	14.713	12.759	17.874	9.159	15.659	15.903	11.334	16.861
	(7.128)	(7.350)	(7.564)	(5.768)	(7.593)	(6.327)	(6.004)	(6.050)
Persistence of GDP process	0.947	0.951	0.946	0.930	0.942	0.945	0.938	0.933
	(0.009)	(0.008)	(0.009)	(0.012)	(0.009)	(0.007)	(0.010)	(0.010)
AR2 test p-value	[0.51]	[0.90]	[0.28]	[0.62]	[0.72]	[0.34]	[0.58]	[0.70]
Observations	6,161	5,374	6,161	5,467	5,570	4,779	6,090	6,161
Countries in sample	175	149	175	171	172	171	172	175
	Panel C: HHK estimates.							
Democracy	1.178	0.722	1.059	1.203	1.110	2.030	1.262	1.482
	(0.370)	(0.357)	(0.364)	(0.376)	(0.332)	(0.359)	(0.355)	(0.449)
Long-run effect of democracy	25.032	15.731	21.648	25.557	24.575	32.631	22.161	26.358
	(10.581)	(8.476)	(9.431)	(9.842)	(9.031)	(7.727)	(6.641)	(9.178)
Effect of democracy after 25 years	20.853	12.719	18.313	20.753	19.407	28.896	19.633	22.776
	(7.731)	(6.503)	(7.162)	(7.072)	(6.359)	(6.223)	(5.647)	(7.380)
Persistence of GDP process	0.953	0.954	0.951	0.953	0.955	0.938	0.943	0.944
	(0.009)	(0.006)	(0.009)	(0.008)	(0.008)	(0.008)	(0.006)	(0.008)
Observations	6,161	5,374	6,161	5,467	5,570	4,779	6,090	6,161
Countries in sample	175	149	175	171	172	171	172	175

Notes: This table presents estimates of the effect of democracy on log GDP per capita. The reported coefficient of democracy is multiplied by 100. Panel A presents results using the within estimator. Panel B presents results using Arellano and Bond's GMM estimator. The AR2 row reports the p-value for a test of serial correlation in the residuals of the GDP series. Panel C presents results using the HHK estimator. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Additionally, we control for the covariates specified in each column label and described in the text. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

Table 5: Semi-parametric estimates of the effect of democratizations on (log) GDP per capita.

Average effects from:	-5 to -1 years (1)	0 to 4 years (2)	5 to 9 years (3)	10 to 14 years (4)	15 to 19 years (5)	20 to 24 years (6)	25 to 29 years (7)
	Panel A: Linear regression adjustment.						
Average effect of democracy on log GDP	0.060	2.454	3.621	7.806	14.037	24.075	21.310
	(0.156)	(1.382)	(2.792)	(4.416)	(5.384)	(8.262)	(9.643)
	Panel B: Inverse propensity score reweighting.						
Average effect of democracy on log GDP	-1.586	3.724	3.214	6.818	13.542	24.111	22.184
	(1.478)	(1.789)	(3.327)	(4.848)	(5.892)	(9.035)	(11.561)
	Panel C: Doubly-robust estimator.						
Average effect of democracy on log GDP	0.051	2.795	2.969	6.966	12.947	23.691	21.793
	(0.150)	(1.478)	(3.070)	(4.354)	(4.886)	(7.674)	(9.612)

Notes: This table presents semi-parametric estimates of the effect of a democratization on log GDP per capita over different time horizons, indicated in the column labels. We report estimates of the average effect on the treated. Panel A presents estimates using regression adjustment to compute counterfactual outcomes for treated countries. Panel B presents estimates obtained via inverse propensity score reweighting. Panel C presents estimates obtained using a doubly-robust estimator, combining the regression adjustment and the inverse propensity score reweighting. Below each estimate we report robust standard errors obtained via bootstrapping.

Table 6: Instrumental-variables estimates of the effect of democracy on (log) GDP per capita.

Covariates included:	(1)	(2)	GDP in 1960 quintiles× year effects (3)	Soviet dummies (4)	Regional trends (5)	Regional GDP & trade (6)	Regional unrest GDP & trade (7)	Spatial lag of GDP (8)	Spatial lags of GDP and democracy (9)
	Panel A: 2SLS estimates with fixed effects.								
Democracy	0.966	1.149	1.125	1.292	1.697	1.817	1.107	1.335	1.361
	(0.558)	(0.554)	(0.689)	(0.651)	(0.885)	(0.663)	(0.656)	(0.536)	(0.895)
Long-run effect of democracy	26.315	31.521	35.226	35.723	36.788	41.544	25.016	37.482	38.439
	(17.075)	(17.425)	(23.846)	(19.997)	(20.657)	(17.157)	(16.002)	(17.836)	(27.883)
Effect of democracy after 25 years	20.836	24.866	25.618	27.929	32.051	35.350	21.386	29.217	29.011
	(12.862)	(12.978)	(16.538)	(14.944)	(17.703)	(14.017)	(13.342)	(12.894)	(19.692)
Persistence of GDP process	0.963	0.964	0.968	0.964	0.954	0.956	0.956	0.964	0.965
	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)
Hansen p-value		[0.21]	[0.18]	[0.32]	[0.28]	[0.25]	[0.09]	[0.04]	[0.19]
Observations	6,312	6,309	5,496	6,309	6,309	6,309	6,309	6,181	6,009
Countries in sample	174	174	148	174	174	174	174	173	173
Exc. Instruments F-stat.	119.1	33.2	16.8	26.7	23.7	13.6	16.7	17.5	4.6
	Panel B: First-stage estimates.								
Democracy wave t-1	0.800	0.547	0.503	0.480	0.498	0.522	0.508	0.540	0.586
	(0.073)	(0.101)	(0.130)	(0.099)	(0.092)	(0.104)	(0.102)	(0.103)	(0.101)
Democracy wave t-2		0.133	0.109	0.133	0.129	0.117	0.115	0.136	0.128
		(0.081)	(0.094)	(0.080)	(0.081)	(0.079)	(0.078)	(0.078)	(0.088)
Democracy wave t-3		0.227	0.270	0.223	0.228	0.221	0.223	0.224	0.282
		(0.067)	(0.077)	(0.065)	(0.070)	(0.069)	(0.070)	(0.070)	(0.077)
Democracy wave t-4		-0.087	-0.119	-0.075	-0.123	-0.083	-0.064	-0.072	-0.107
		(0.110)	(0.126)	(0.110)	(0.106)	(0.113)	(0.113)	(0.113)	(0.116)
	Panel C: HHK estimates.								
Democracy	0.690	0.944	1.435	0.719	0.822	1.311	0.897	1.021	1.206
	(0.642)	(0.479)	(0.599)	(0.503)	(0.480)	(0.435)	(0.371)	(0.549)	(0.485)
Long-run effect of democracy	14.512	24.766	46.767	18.337	16.413	24.040	17.290	29.286	31.111
	(14.703)	(14.083)	(22.556)	(13.688)	(10.700)	(9.989)	(8.556)	(18.354)	(15.167)
Effect of democracy after 25 years	11.768	18.670	31.039	13.969	13.778	21.100	14.668	21.133	23.702
	(11.445)	(9.799)	(13.113)	(9.935)	(8.523)	(8.038)	(6.734)	(11.942)	(10.243)
Persistence of GDP process	0.952	0.962	0.969	0.961	0.950	0.945	0.948	0.965	0.961
	(0.011)	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)	(0.010)	(0.009)	(0.008)
Observations	6,161	6,161	5,374	6,161	6,161	6,161	6,161	6,132	5,960
Countries in sample	174	174	148	174	174	174	174	173	173

Notes: This table presents IV estimates of the effect of democracy on log GDP per capita. The reported coefficient of democracy is multiplied by 100. Panel A presents 2SLS estimates instrumenting democracy with up to four lags of regional democracy waves and the p-value of a Hansen overidentification test. Panel B presents the corresponding first stage estimates and the excluded instruments F statistic. Panel C presents results using the HHK estimator instrumenting democracy with up to four lags of regional democracy waves (except for column 1, where we only use one lag). In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Additionally, we control for the covariates specified in each column label and described in the text. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table 7: Effects of democracy on potential mechanisms.

Dependent variable:	Log of investment share in GDP	Log of TFP	Index of economic reforms	Log of trade share in GDP	log of tax share in GDP	Log of primary enrollment	Log of secondary enrollment	log of child mortality	Dummy for unrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Within estimates.								
Democracy	2.391	-0.205	0.687	0.689	3.311	1.042	1.345	-0.253	-7.832
	(1.114)	(0.276)	(0.348)	(0.676)	(1.409)	(0.338)	(0.610)	(0.063)	(2.185)
Long-run effect of democracy	9.112	-2.883	5.580	5.445	16.062	21.908	18.960	-34.264	-11.944
	(4.255)	(3.858)	(2.883)	(5.253)	(6.650)	(7.624)	(8.622)	(10.747)	(3.329)
Effect of democracy after 25 years	9.089	-2.738	5.359	5.303	15.864	18.892	18.057	-21.400	-11.944
	(4.245)	(3.648)	(2.753)	(5.126)	(6.574)	(6.321)	(8.146)	(5.124)	(3.329)
Persistence of outcome process	0.738	0.929	0.877	0.873	0.794	0.952	0.929	0.993	0.344
	(0.020)	(0.012)	(0.012)	(0.011)	(0.016)	(0.008)	(0.013)	(0.001)	(0.030)
Observations	5,665	3,879	4,692	5,738	4,511	3,714	2,883	6,084	5,646
Countries in sample	169	107	150	172	131	166	158	173	171
	Panel B: 2SLS estimates.								
Democracy	2.211	-0.941	3.224	5.512	8.088	1.757	4.116	-0.715	-5.569
	(2.852)	(0.667)	(0.863)	(2.005)	(3.021)	(0.721)	(1.626)	(0.164)	(5.682)
Long-run effect of democracy	8.440	-12.738	23.775	40.589	38.609	36.693	57.072	-95.728	-8.471
	(10.705)	(8.854)	(6.215)	(13.580)	(14.330)	(15.505)	(21.698)	(26.347)	(8.577)
Effect of democracy after 25 years	8.419	-12.167	23.156	39.817	38.159	31.611	54.252	-58.625	-8.471
	(10.681)	(8.380)	(6.039)	(13.375)	(14.121)	(12.863)	(20.267)	(13.123)	(8.577)
Persistence of outcome process	0.738	0.926	0.864	0.864	0.791	0.952	0.928	0.993	0.343
	(0.020)	(0.012)	(0.012)	(0.012)	(0.017)	(0.008)	(0.013)	(0.001)	(0.030)
Exc. instruments F-stat.	21.7	27.7	43.7	21.5	31.8	12.1	10.4	26.3	28.6
Hansen p-value	[0.29]	[0.06]	[0.22]	[0.09]	[0.69]	[0.09]	[0.12]	[0.02]	[0.84]
Observations	5,640	3,871	4,670	5,714	4,489	3,710	2,879	6,057	5,619
Countries in sample	168	107	149	171	130	164	156	172	170
	Panel C: HHK estimates.								
Democracy	6.603	0.388	1.121	1.255	4.277	1.384	2.144	-0.306	-3.638
	(1.336)	(0.294)	(0.371)	(0.790)	(2.044)	(0.366)	(0.644)	(0.068)	(2.931)
Long-run effect of democracy	25.495	7.518	22.655	10.182	24.622	41.349	43.070	-54.798	-5.742
	(5.313)	(6.011)	(11.199)	(6.584)	(11.858)	(14.855)	(15.445)	(15.745)	(4.630)
Effect of democracy after 25 years	25.432	6.748	15.698	9.807	23.966	29.049	36.865	-29.139	-5.742
	(5.294)	(5.366)	(5.953)	(6.307)	(11.461)	(8.614)	(11.888)	(6.131)	(4.630)
Persistence of outcome process	0.741	0.948	0.951	0.877	0.826	0.967	0.950	0.994	0.366
	(0.018)	(0.009)	(0.018)	(0.014)	(0.031)	(0.007)	(0.012)	(0.001)	(0.037)
Observations	5,125	3,557	4,236	4,866	4,045	3,579	2,683	5,454	5,233
Countries in sample	168	107	149	171	130	164	156	172	170

Notes: This table presents estimates of the effect of democracy on the different channels specified in the columns labels. The reported coefficient of democracy is multiplied by 100 (except for columns 3 and 9). Panel A presents within estimates. Panel B presents 2SLS estimates instrumenting democracy with four lags of regional democracy waves, the F statistic for the excluded instruments and the p-value of Hansen's overidentification test. Panel C presents results using the HHK estimator instrumenting democracy with four lags of regional democracy. In all specifications we control for a full set of country and year fixed effects, four lags of GDP per capita and four lags of the dependent variable. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table 8: Heterogeneous effects of democracy on (log) GDP per capita.

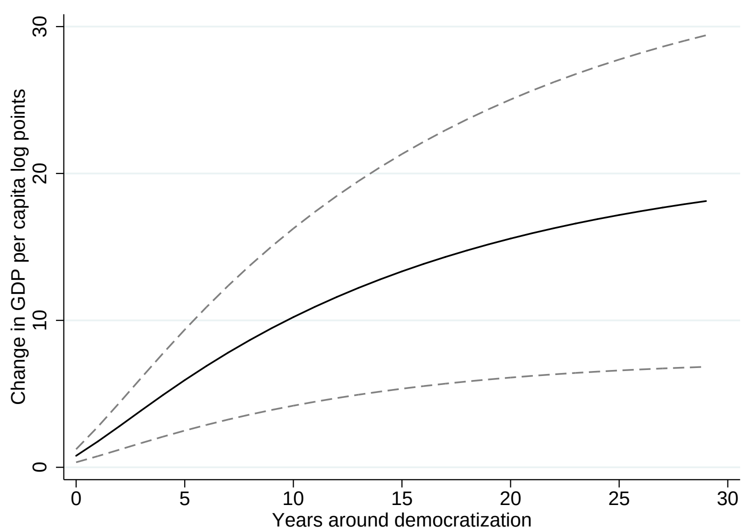
Interaction with:	Log GDP per capita:				Share with secondary:			
Measured at:	1960	1970	1980	Lagged	1960	1970	1980	Lagged
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Within estimates.							
Democracy	0.432	0.572	0.687	0.744	0.446	0.340	0.385	0.495
	(0.275)	(0.248)	(0.248)	(0.246)	(0.254)	(0.253)	(0.246)	(0.241)
Interaction	0.001	0.001	0.002	0.001	0.046	0.049	0.038	0.020
	(0.002)	(0.001)	(0.002)	(0.002)	(0.028)	(0.020)	(0.014)	(0.013)
Long-run effect of democracy	16.231	18.631	20.489	19.843	13.785	10.480	11.841	14.597
	(11.160)	(9.073)	(8.608)	(8.255)	(8.550)	(8.275)	(8.118)	(8.432)
Effect of democracy after 25 years	10.013	12.916	14.985	15.877	10.081	7.679	8.687	10.953
	(6.565)	(5.960)	(5.848)	(5.943)	(5.964)	(5.872)	(5.728)	(5.821)
Persistence of GDP process	0.973	0.969	0.966	0.963	0.968	0.968	0.967	0.966
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	4,281	4,909	5,525	6,336	5,300	5,300	5,300	5,300
Countries in sample	93	109	131	175	138	138	138	138
	Panel B: 2SLS estimates.							
Democracy	0.500	0.155	0.645	1.326	-0.119	-0.484	-0.474	0.600
	(1.088)	(0.961)	(0.929)	(0.887)	(0.662)	(0.665)	(0.639)	(0.576)
Interaction	-0.002	0.000	-0.000	-0.003	0.174	0.156	0.116	0.049
	(0.005)	(0.004)	(0.004)	(0.004)	(0.060)	(0.047)	(0.033)	(0.023)
Long-run effect of democracy	18.838	4.978	19.275	36.116	-3.649	-14.586	-14.135	17.373
	(43.554)	(31.473)	(30.208)	(29.900)	(19.968)	(19.023)	(18.114)	(18.629)
Effect of democracy after 25 years	11.592	3.486	14.078	28.377	-2.692	-10.843	-10.574	13.133
	(25.784)	(21.795)	(21.085)	(21.317)	(14.837)	(14.524)	(13.901)	(13.312)
Persistence of GDP process	0.973	0.969	0.967	0.963	0.967	0.967	0.966	0.965
	(0.006)	(0.006)	(0.006)	(0.008)	(0.006)	(0.006)	(0.006)	(0.006)
Exc. instruments F-stat.	6.6	6.1	7.0	14.0	18.5	17.6	16.0	12.4
Hansen p-value	[0.81]	[0.73]	[0.54]	[0.33]	[0.44]	[0.41]	[0.25]	[0.50]
Observations	4,273	4,901	5,517	6,153	5,292	5,292	5,292	5,218
Countries in sample	93	109	131	174	138	138	138	138
	Panel C: HHK estimates.							
Democracy	0.222	0.234	0.144	1.619	1.101	0.887	0.790	1.713
	(0.379)	(0.401)	(0.445)	(0.477)	(0.686)	(0.679)	(0.638)	(0.584)
Interaction	0.004	-0.000	0.001	0.002	0.093	0.089	0.058	0.016
	(0.003)	(0.003)	(0.004)	(0.004)	(0.046)	(0.037)	(0.028)	(0.013)
Long-run effect of democracy	7.692	7.453	4.480	48.375	31.605	25.022	22.375	49.338
	(13.442)	(13.213)	(14.002)	(21.975)	(21.502)	(20.748)	(19.522)	(23.950)
Effect of democracy after 25 years	4.869	5.084	3.054	34.304	23.787	19.159	17.091	36.069
	(8.286)	(8.850)	(9.435)	(11.965)	(15.084)	(14.981)	(14.107)	(14.116)
Persistence of GDP process	0.971	0.969	0.968	0.967	0.965	0.965	0.965	0.965
	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)	(0.008)	(0.009)
Observations	4,180	4,792	5,386	6,110	5,154	5,154	5,154	5,154
Countries in sample	93	109	131	174	138	138	138	138

Notes: This table presents estimates of the effect of democracy on log GDP per capita and its interaction with other country characteristics indicated in the columns' headers. The reported coefficients of democracy and the interaction are multiplied by 100. We report main effects and long-run effects evaluated at the 25th percentile of the interacted variable. Panel A presents within estimates. Panel B presents 2SLS estimates instrumenting democracy (and the interaction term) with four lags of regional democracy waves. It also reports the F statistic for the excluded instruments and the p-value of Hansen's overidentification test. Panel C presents results using the HHK estimator instrumenting democracy (and the interaction term) with four lags of regional democracy waves. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Figure 1: GDP per capita before and after a democratization.

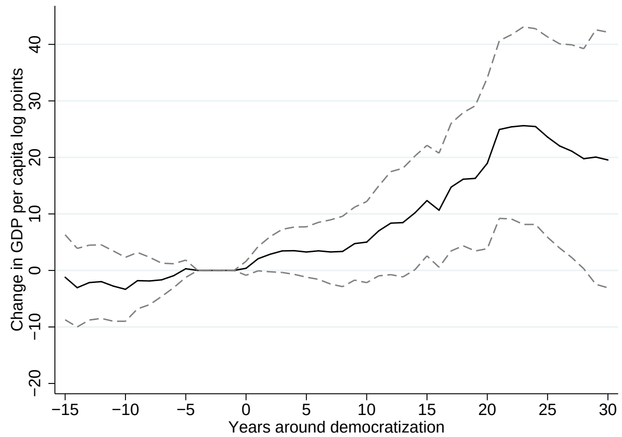
Notes: This figure plots GDP per capita in log points around a democratic transition. We normalize log GDP per capita to zero in the year preceding the democratization. Time (in years) relative to the year of democratization runs on the horizontal axis.

Figure 2: Dynamic panel model estimates of the over-time effects of democracy on the log of GDP per capita.



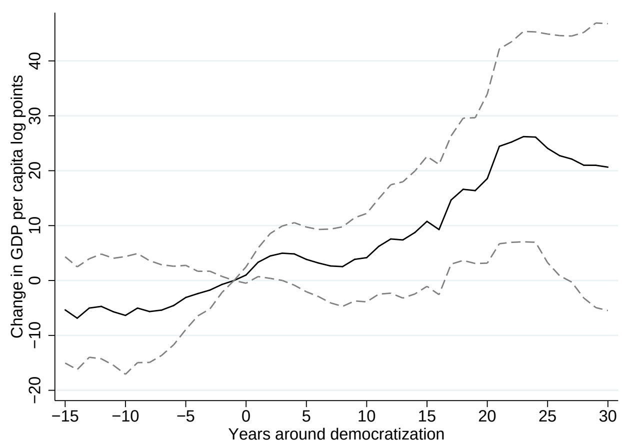
Notes: This figure plots the estimated change in the log of GDP per capita caused by a permanent transition to democracy. The effects are obtained by forward iteration of the estimated process for GDP modeled in equation (1). A 95% confidence interval obtained using the delta method is presented in dotted lines. Time (in years) relative to the year of democratization runs on the horizontal axis.

Figure 3: Semi-parametric estimates of the over-time effects of democracy on the log of GDP. Estimates obtained using a regression model to estimate counterfactuals.



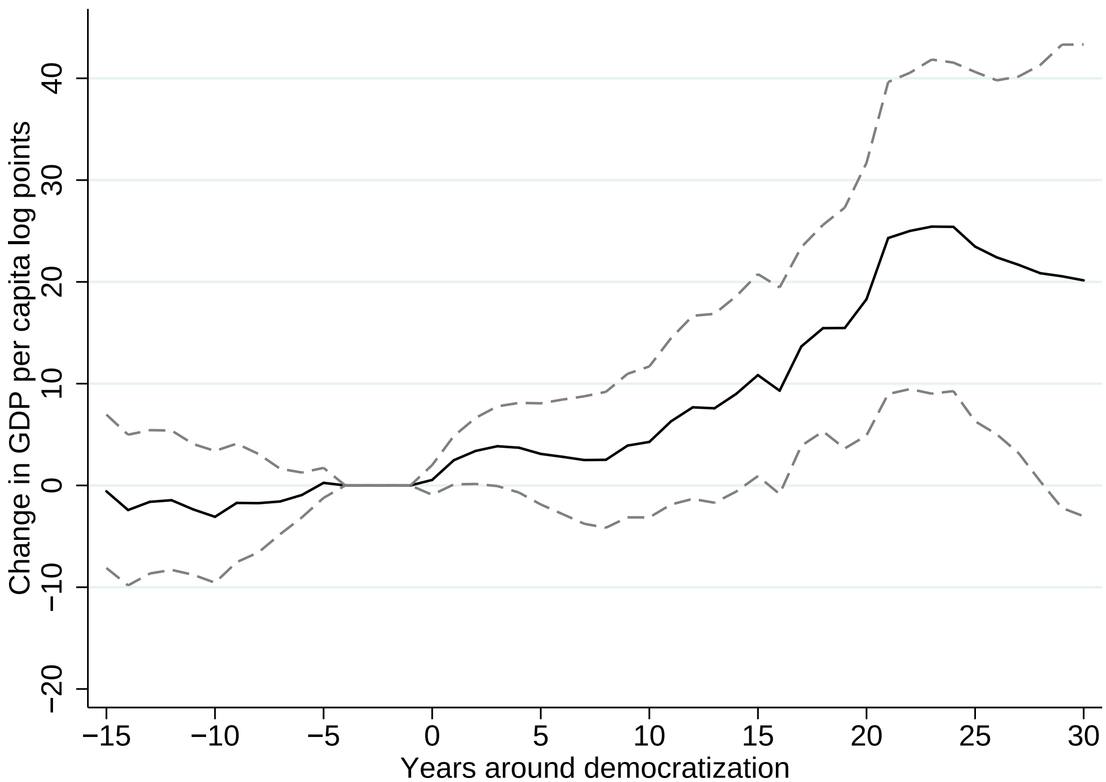
Notes: This figure plots semi-parametric estimates of the effect of democratizations on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), together with a 95% confidence interval in dashed lines. Time (in years) relative to the year of democratization runs on the horizontal axis. The estimates are obtained by assuming and estimating a linear model for counterfactual outcomes, which we use to control for the influence of GDP dynamics. Section 4 explains our approach in full detail.

Figure 4: Semi-parametric estimates of the over-time effects of democracy on the log of GDP. Estimates obtained using inverse-propensity score reweighting.



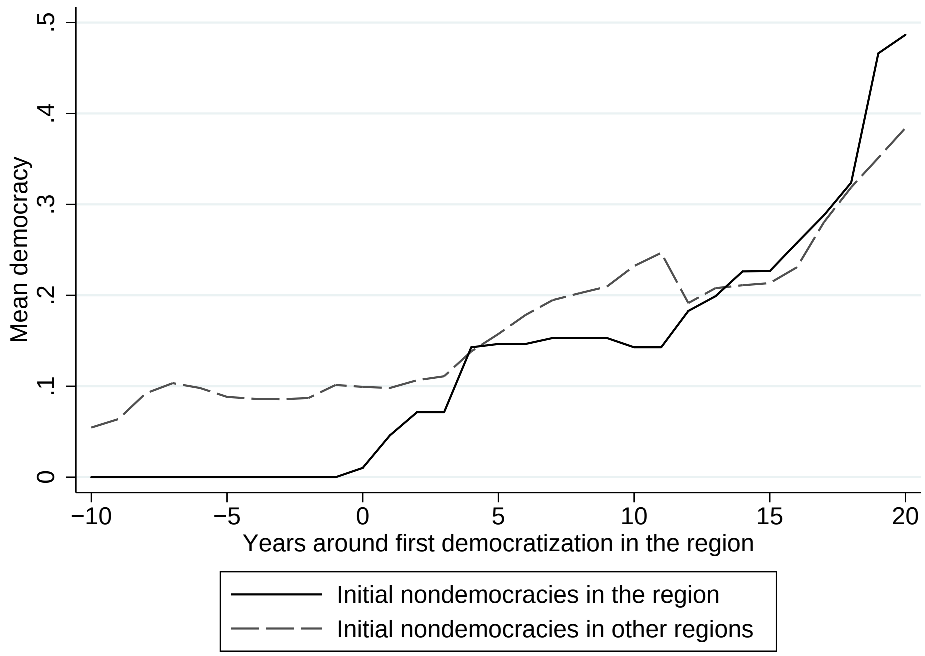
Notes: This figure plots semi-parametric estimates of the effect of democratizations on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), together with a 95% confidence interval in dashed lines. Time (in years) relative to the year of democratization runs on the horizontal axis. The estimates are obtained by assuming and estimating a probit model for democratizations based on GDP lags, which we use to estimate the propensity score and reweight the data. Section 4 explains our approach in full detail.

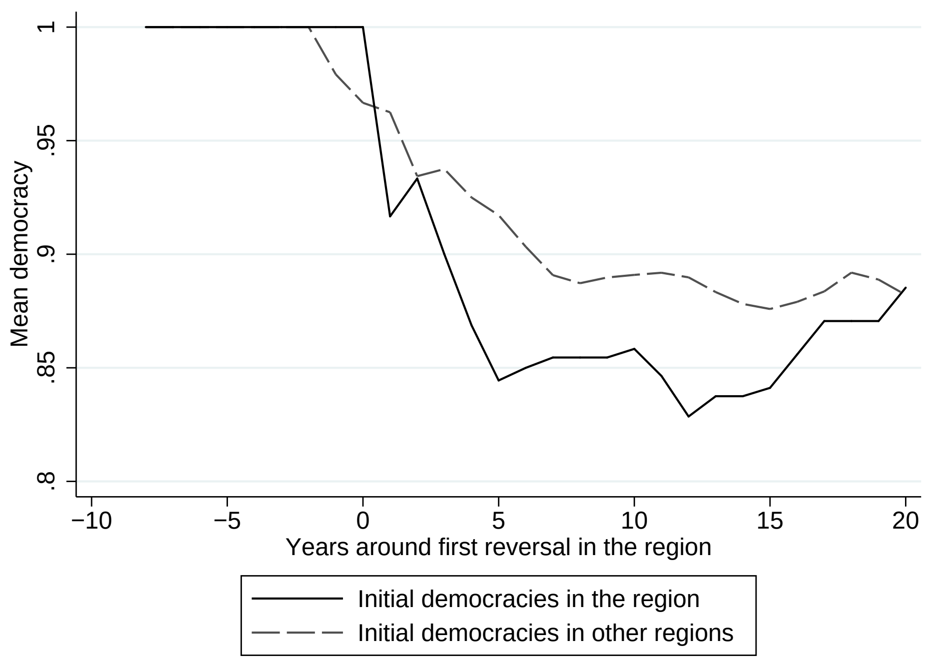
Figure 5: Semi-parametric estimates of the over-time effects of democracy on the log of GDP. Doubly-robust estimates.



Notes: This figure plots semi-parametric estimates of the effect of democratizations on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), together with a 95% confidence interval in dashed lines. Time (in years) relative to the year of democratization runs on the horizontal axis. The estimates are obtained by assuming and estimating a probit model for democratizations based on GDP lags, which we use to estimate the propensity score and reweight the data. In addition, we partial out lags of GDP linearly, making our approach doubly robust. Section 4 explains our approach in full detail.

Figure 6: Regional democratizations and reversal waves.





Notes: These figures illustrate the existence of regional democracy waves. The top figure plots average democracy among initial nondemocracies around the first democratization in the region. For comparison it also plots average democracy among other initial nondemocracies in other regions. The bottom figure plots average democracy among initial democracies around the first reversal in the region. For comparison it also plots average democracy among other initial democracies in other regions.

ONLINE APPENDIX FOR "DEMOCRACY DOES CAUSE GROWTH."

A1 Detailed Construction of our Democracy Measure

We construct our consolidated measure of democracy using Freedom House and Polity IV as our main sources. We also use secondary sources to resolve ambiguous cases (those in which Polity and Freedom house report contrary assessments) or those without data coverage in Freedom House or Polity IV. For instance, Freedom House only covers the period since 1972, so we use secondary sources and the Polity IV index to code our measure of democracy prior to this period. Likewise, Polity IV does not cover some small countries that are in the Freedom House sample and in other secondary sources. The secondary sources are the dichotomous measures introduced by Cheibub, Gandhi, and Vreeland (2010)—henceforth CGV—and Boix, Miller, and Rosato (2012)—henceforth BMR.[24] Both measures extend and refine Przeworski et al.'s (2000) measure of democracy. Finally, we use Papaioannou and Siourounis's (2008) data—henceforth PS—which contains the exact year of a permanent transition to democracy for many of the countries in our sample, but that does not include temporary transitions in and out of democracy.

Our measure of democracy, $D_{ct} \in \{0, 1\}$ for country c at time t, is coded as follows:

1. We code a country c as democratic in year t (i.e., $D_{ct} = 1$) if Freedom House regards it as "Free" or "Partially Free" and Polity IV gives it a positive democracy score (The Polity IV index is between -10 and 10). This procedure generates the bulk of the variation in our democracy measure.[25]

2. For small countries that only appear in the Freedom House sample, we code them as democratic if their Freedom House status is "Free" or "Partially Free," and either CGV or BMR consider them to be democratic. There is overwhelming agreement between Freedom House, CGV and BMR in all such cases, making the coding straightforward.[26]

[24] CGV code a period as democratic when the chief executive is chosen by popular election (directly or indirectly), the legislature is popularly elected, there are multiple parties competing in the election, and an "alternation in power under electoral rules identical to the ones that brought the incumbent to office takes place." BMR update Przeworski et al. (2000) and add the additional qualification that only instances in which more than 50% of the male population are allowed to vote are coded as democracies.

[25] Using the "Free" or "Partially Free" and the positive Polity scores to define dichotomous democracy indices is a relatively common practice in the literature. For instance, this is the approach used by Papaioannou and Siourounis (2008) to identify the transitions they then analyze in more detail using historical sources. Giavazzi and Tabellini (2005) and Persson and Tabellini (2006) use similar cutoffs for the Polity score to define dichotomous democracy indices.

[26] The only ambiguous case is Samoa, which is coded as "Free" since 1989 by Freedom House, while CGV and BMR both code it as nondemocratic. We follow the latter coding since rulers in Samoa have a long tenure and are appointed to

3. Freedom House does not provide any data before 1972. For these early years, we code a country as democractic if it has a positive Polity score and either CGV or BMR code it as democratic. There are a few cases coded as nondemocracies by CGV and BMR with a positive Polity score. In these cases, the Polity score is always near zero and we code the observation as a nondemocracy.

4. Ex-Soviet and Ex-Yugoslav countries are coded as nondemocracies before 1990, based on the USSR and Yugoslavia scores before their dissolution.

5. When both Freedom House and Polity are missing (174 observations for 16 countries), we rely on our secondary sources and code our measure of democracy manually.[27]

6. We remove spurious transitions created when countries enter or leave the Freedom House, Polity, or our secondary sources' samples. For instance, these spurious transitions arise when a country appears in (or leaves) the sample for one of our sources that gives it a more (or less) favorable assessment than the others.[28]

7. Finally, we perform an additional refinements of our measure and adjust it to match the dates for permanent democratizations that PS coded. These dates are available for 68 transitions in our sample (recall PS only code permanent transitions), and are based on historical sources.[29]

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office for life. Besides this particular case, there are some countries for which only Freedom House provides information for the years 2009 and 2010 (the CGV and BMR sample ends in 2008 and 2009 respectively). These include Afghanistan, Bahamas, Barbados, Belize, Bosnia & Herzegovina, Brunei Darussalam, Dominica, Grenada, Iceland, Iraq, Kiribati, Luxembourg, Maldives, Malta, Nauru, Palau, Samoa, Seychelles, St. Kitts and Nevis, St. Lucia, St. Vincent & Grens., Suriname, São Tomé & Príncipe, Tonga and Vanuatu. In all of these cases the Freedom House indicator remains the same since 2008, so we assume these countries remain in the same political regime that was in place in 2008.

[27] The first country is Antigua and Barbuda, which is coded as democratic following its independence in 1981. Barbados is set as democratic from its independence in 1966 until it enters the Freedom House sample in 1972, after which Freedom House codes it as democratic. Germany, Iceland, and Luxembourg are coded as always democratic. This matches the Freedom House coding once they enter into its sample. Kuwait is set to nondemocratic in 1961 and 1962, until it enters the Polity sample in 1963 and is also coded as nondemocratic. The Maldives are set as nondemocratic from its independence in 1965, until they enter the Freedom House sample in 1972 and is also coded as nondemocratic. Malta is set as democratic from its independence in 1964, until it enters the Freedom House sample in 1972 and is also coded as democratic. Nauru is set as democratic from its independence in 1968 until it enters the Freedom House sample in 1972, remaining democratic. Syria is coded as nondemocratic in 1960 when it was not in the Polity sample. It remains nondemocratic in the Polity sample. Tonga is coded as nondemocratic since its independence. This matches the Freedom House coding when it enters the sample. Vietnam and Yemen are coded as always nondemocratic, but they are not in Polity and Freedom House prior to their unification. However, they were nondemocratic according to all secondary sources. Samoa is nondemocratic since its independence based on CGV and BMR for years in which Polity and Freedom House are missing. Finally, Zimbabwe is also nondemocratic in 1965-1969, according to our secondary sources.

[28] This is the case for Cyprus, Malaysia, Gambia, and Guyana, which we handled manually. The particular coding of these countries does not affect our results. We follow most sources and code Cyprus as democratic after 1974. Malaysia is coded as nondemocratic throughout. Guyana is coded as nondemocratic between 1966 and 1990 and democratic in all other years. Finally, Gambia is coded as democratic between 1965 and 1993 only.

[29] Some special cases, for which PS transition dates and our coding are not close in time, include Guatemala, El Salvador, Iran, Tanzania, and South Africa. For Guatemala, our coding described above dates a democratization in 1986, while PS code a permanent transition at the end of the civil war in 1996. For El Salvador, we code the democratization episode in 1982 based on Freedom House and Polity, while PS code it in 1994. We do not detect any transition to democracy for Iran and Tanzania. In all of these cases we keep our original coding. Our coding produces a transition to democracy in South Africa during the early 80s based solely on Freedom House and Polity. However, PS and all secondary sources agree that the official democratization was in 1994, so we use this date.

Our dichotomous measure of democracy is available for 183 countries and covers their post-independence period since 1960 and until 2010. Out of the 8,733 country/year observations, we code 3,777 instances of democracy and 4,956 instances of nondemocracy. Out of the 183 countries, 45 are always democratic, 45 are always nondemocratic, and the rest transition in and out of democracy. A total of 122 democratizations and 71 reversals suggest significant within-country variation in our democracy measure.

Figure A1 plots the yearly average of our democracy measure for the whole world, and separately for each of the regions in our sample. We also plot other indices of democracy for comparison (Freedom House and Polity are normalized to lie between 0 and 1 to ease the comparison). All measures show very similar patterns in all regions over time. The correlation between our measure and PS's measure is 0.9054; with CGV it is 0.8880, and with BMR it is 0.9050, suggesting all measures are highly correlated.

In Tables A1 and A2, we list all democratizations and reversals in our sample. We also present the estimated propensity scores for each transition obtained from our semi-parametric analysis in Section 4 and explained in detail in Section A7 of this Online Appendix. The estimated propensity score is missing for countries for which we do not have the GDP data required to compute it.

A1.1 Comparison to Previous Measures of Democracy

We now compare the performance of our measure with other indices used previously in the literature. These include dichotomous versions of Freedom House and Polity, as well as the dichotomous measures by PS, CGV, and BMR.[30]

Table A6 presents our results. Panel A shows the within estimates of our baseline dynamic panel model with four lags of GDP. We display the results for each democracy index in a different column (as indicated in the top row), with the dependent variable always being the log of GDP per capita. Panel B presents 2SLS estimates using the specification in column 2, Panel A, of Table 6. Finally, Panel C presents within estimates that do not control for GDP dynamics. These correspond to traditional differences-in-differences models (in levels) that do not take into account GDP dynamics.

All estimates in Panels A and B show uniformly positive effects of democracy on growth. Our within estimates in Panel A are all significant except for the Polity dummy and the CGV measure of democracy. Moreover, our 2SLS results in Panel B are always significant except for the BMR democracy measure. In this case they are still positive and of a reasonable size, but less precisely estimated. The 2SLS estimates are considerably larger than their OLS counterparts (except for our

[30]The dichotomous version of Freedom House is obtained by coding as democratic countries that are "Free" or "Partially Free". For Polity, we code the countries with a positive score as democratic. Some of these alternative data sources do not assign any score to former Soviet countries before 1991. We follow our procedure and code them as nondemocracies before 1991 (this is also the coding given by all these sources to former Soviet Union countries and Satellite countries).

measure and PS). This supports our claim that the alternative measures are more heavily affected by measurement error than our consolidated measure. Overall, we take these results as suggesting that our results do not strictly rely in the way we coded democracy. Further, the results relying on our consolidated measure are less attenuated by measurement error, which lends support for our approach of constructing a consolidated measure.

Panel C presents traditional differences-in-differences estimates of democracy on GDP levels that do not control for GDP dynamics. In all these cases, independently of the measure used, our estimates for democracy are never positive and always imprecise. The difference between Panel A—in which we control for GDP dynamics—and Panel C—in which we do not—underscores that the bias created by the dip in GDP that precedes a democratization may be large. The failure to adequately control for GDP dynamics when estimating the relationship between democracy and economic growth explains, at least in part, the difference between our positive findings and previous results in the literature.

A1.2 Components of Democracy

In this subsection, we document the institutional variation that our democracy measure captures. All the sources that we use define democracy as an institutional arrangement that comprises several components. These include free elections, the existence of institutional checks on the executive, inclusive participation and representation, that non-ruling parties are organized and compete for political influence regularly, and to a lesser extent civil rights. These basic components constitute the institutional variation captured by our measure of democracy.

Our measure of democracy is highly correlated with specific measures for all of these components.[31] To illustrate which particular institutional components vary with a democratization, Figure A2 plots the behavior of several components of democracy after a transition to democracy in our data. The figure shows that transitions to democracy are characterized by an improvement in all of these basic components of democracy. These patterns suggest that, in our sample, transitions to democracy typically entail a similar set of institutional changes characterized by a greater likelihood of choosing leaders through elections, more constraints on elected officials, and a more open and inclusive political process in which a broad segment of society may participate. To a lesser extent, democratizations are also accompanied by improvements in civil rights.

[31] We construct measures for all components using Polity and Freedom House raw data. We code a country as having free elections when, according to Polity, the executive is chosen via elections (or the executive is dual, and one member is chosen by elections). Moreover, we require the election to be open to challengers. We code a country as having constraints on the executive when, according to Polity, there are substantial limitations for the exercise of power by the chief executive. Finally, we code a country as having inclusive politics when, according to Polity, there are organized political groups outside the government which regularly compete for political influence. We also use the Freedom House index of civil liberties, normalized between 0 and 1.

A2 Alternative GDP measures

Table A7 explores the impact of democracy on GDP using alternative measures of GDP per capita. As indicated by the column headers, in columns 1 through 3, we focus on our three main specifications with no external instruments (the within, Arellano and Bond, and HHK estimates), while in columns 4 and 5 we present instrumental-variables estimates.

Following the advice from Johnson et al. (2009), we start by focusing on measures of real GDP per capita that are constructed from national accounts and do not adjust for changes over time in purchasing power parity (PPP). Panel A presents results using a measure of real GDP from the World Bank, which is computed in constant units of each country's local currency (not in year 2000 dollars as our baseline measure). Panel B presents results using a measure of real GDP provided in version 9 of the Penn World Tables and computed from national accounts, and converted to per capita terms using population data also from the Penn World Tables. The results in these two panels are very similar to our baseline estimates.

In Panel C, for the sake of completeness, we deviate from the advice by Johnson et al. (2009) and present results using a measure of GDP per capita adjusted for changes in PPP over time and across countries from version 7 of the Penn World Tables. These results should be interpreted with caution, since PPP adjustments over time are likely to introduce non-trivial and non-classical measurement error. In this case, the results are similar using the within, Arellano and Bond, and the HHK estimators, but are significantly attenuated in our instrumental-variables specifications in columns 4 and 5.

A3 Alternative Estimates of the Long-Run Impact of Democracy

In this section we present alternative estimates of the long-run impact of a permanent transition to democracy. In the main text we compared a country that permanently transitions to democracy to a counterfactual scenario in which the country remains in nondemocracy throughout. In this Appendix, we provide results in which we assume a counterfactual scenario in which the country may democratize in future periods according to the estimated probability of a transition to democracy.

To compute these alternative counterfactuals, we estimate an $AR(p)$ model for the likelihood of democratization in our sample. This model predicts a gradual increase in democracy for a country that starts nondemocratic at a given year and does not democratize. To compute the counterfactual growth in a country 25 years after failing to democratize, we take the predicted likelihood of democracy from our $AR(p)$ model for each year and multiply it by the expected growth gains from democracy expected from that year to the 25th year after the failure to democratize. We then subtract this counterfactual growth from our estimates in the main text. This calculation takes into account the fact that the country would have democratized with some low probability in any case even if it failed to democratize in a given year.

Table A3 presents our results using different number of lags for the $AR(p)$ model in each column. Overall, we find that once we adjust for the possibility that countries would have democratized anyway, the cumulative effects on GDP 25 years after a transition to democracy are about 25% lower. The Table also reports estimates of the counterfactual probability of a transition to democracy in this 25-year period, which is roughly 36.6% in the AR(4) specification in column 3.

A4 Alternative Strategy for Controlling for High Levels of Persistence in the GDP Process

In this part of the Online Appendix, we provide further evidence that the assumption of a stationary process for GDP is not playing an important role in our results. In particular, we show that if we impose high levels of persistence for our GDP process we obtain similar findings. This allows us to investigate how our estimates behave when we allow GDP to have a near unit root behavior, and provides further robustness checks that deal with the possibility that, because of the Nickell bias, we might under-estimate the persistence of the GDP process.

To do so, we rearrange equation (1) as

$$y_{ct} - \rho y_{ct-1} = \beta D_{ct} + \sum_{j=1}^{p-1} \eta_j (y_{ct-j} - y_{ct-j-1}) + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{A1}$$

where $\rho = \sum_{j=1}^{p} \gamma_j$ is the level of persistence of the GDP process, and $\eta_j = \sum_{i=1}^{j} \gamma_j - \rho$ (with γ_j the coefficients that we defined for the equation in levels).

In our baseline specifications in Table 2, we estimated persistence levels of around 0.95-0.96. We now estimate equation (A1) imposing different values of ρ ranging from 0.95 to 1. Here, $\rho = 1$ corresponds to the extreme case in which the GDP process has a exact unit root, which we also considered in the main text. We only consider processes for GDP with higher persistence because the concern is that because of the Nickell bias we might underestimate ρ.

Table A4 presents our within estimates (Panel A) and 2SLS estimates (Panel B) obtained by imposing these restrictions on ρ. The dependent variable in each model is $y_{ct} - \rho y_{ct-1}$, and the explanatory variables include lagged growth rates of GDP. Provided that $\sum_{j=1}^{p} \gamma_j < 1.95$, this model has the advantage that all these terms are clearly stationary. Thus, inference in these models is not affected by the possibility of near-unit root dynamics in GDP.

Reassuringly, we find larger short- and long-run effects as $\rho \to 1$, suggesting that, if anything, a highly persistent process for GDP would produce larger effects of democracy on GDP levels.

A5 Monte Carlo Simulations

In this section, we explore the severity of the Nickell bias under high levels of persistence of the GDP process by conducting a Monte Carlo simulation exercise. Although some authors have shown that the Nickell bias is small for panels with a long time dimension (see Judson and Owen, 1999), they do not consider levels of persistence as high as the ones that we deal with in our empirical context.

We simulate 1,000 samples for $\{y_{ct}, D_{ct}\}$ obtained from the following data generating process:

$$\begin{aligned} y_{ct} =& \mu_c + 0.787D_{ct} + 1.238y_{ct-1} - 0.207y_{ct-2} - 0.026y_{ct-3} - 0.043y_{ct-4} + \varepsilon_{ct}, \\ D_{ct} =& \alpha_c + 0.130y_{ct-1} - 0.222y_{ct-2} + 0.007y_{ct-3} - 0.053y_{ct-4} + \upsilon_{ct}. \end{aligned}$$

We assume $\mu_c \sim N(0, 0.0574)$, $\alpha_c \sim N(0, 0.548)$, $\varepsilon_{ct} \sim N(0, 0.0502)$ and $\upsilon_{ct} \sim N(0, 0.28)$. These distributions approximate the estimated variances of the fixed effects in our sample. Each sample comprises 175 countries and 38 observations for each country, which matches the dimensions of our panel.

The persistence of the simulated GDP processes is set to 0.963 (and the coefficients on the lags match our preferred estimates). The coefficient of democracy and the democracy process match the within estimates of our preferred specification.

We first assume that the initial values for GDP and democracy are not mean-stationary. Since many countries enter our sample as transition economies and exhibit considerable catch-up growth, we believe this is the most plausible scenario. Moreover, the vast number of democratizations during our period of analysis suggest that political institutions at the start of our sample were not at their steady-state level. In this case, we draw the starting values for the GDP processes, y_{c0}, from a normal distribution $N(0, 1.4)$, and we draw the starting values for the democracy processes, D_{c0}, from a distribution $N(0, 0.31)$, both of which match their empirical counterparts.

Figure A3 plots the distribution of our within estimates, the t-statistics of the effect of democracy, and the estimated persistence of GDP in our simulations. In each figure, the solid red line corresponds to the average across our simulations and the dashed line corresponds to the "true" value assumed in our data-generating process.

The top panel of Table A5 presents the average estimates, their standard deviations, and the relative bias for the within estimates. In each column we present the results obtained by assuming a different value for the persistence of the GDP process. In particular, column 1 imposes the persistence of 0.963, which is what we estimate in our baseline models. Columns 2 to 4 re-scale the process for GDP so that its persistence increases to 0.97, 0.98 and 0.99, respectively. Columns 5 to 8 reproduce the same exercises for the GMM estimator.

Three messages emerge from these results. First, the within-group estimator and the GMM estimator slightly underestimate the persistence of the GDP process (by less than 1%), which suggest that the Nickell bias may be very small in our context. Second, the average bias in the estimate of the effect of democracy, which is our main focus, is negligible. The reason why the coefficient of democracy is not biased is because the GDP dip that precedes democratizations is only temporary. Thus, its effect on subsequent GDP is sufficiently well approximated by the GDP dynamics that we estimate, which are only subject to a minor bias. Third, if anything, we may underestimate the long-run effect of a permanent democratization on growth by about 15%. Finally, the standard deviation of our estimates in column 1 roughly matches the standard error estimated in Table 2, column 3, which suggests that the asymptotic limit used for traditional inference remains a valid approximation in our context.

As mentioned in the text, two features of our data explain the good performance of the within and GMM estimator in our context. The first is the long time series of roughly 38 observations per country. The second is the fact that country fixed effects (the μ_c terms in the simulated data) exhibit a considerable degree of variation. Coupled with the fact that the initial conditions are not mean-stationary, the heterogeneity in μ_c generates large variation in the extent of catch-up growth that provides traction to identify the persistence of the GDP process. Unobserved heterogeneity also improves the performance of the Arellano and Bond GMM estimator; the level instruments become stronger predictors of subsequent growth even when the persistence of the GDP process is close to 1 (see Alvarez and Arellano, 2003, and Hayakawa, 2009).

We next conducted 1,000 Monte Carlo simulations in which the initial conditions are assumed to be mean-stationarity. Although we believe that mean-stationarity is a restrictive assumption, these results show how large the biases are in a worst-case scenario. Figure A4 presents these results for the within estimator. In this case, the bottom panel of Table A5 shows that the persistence of the GDP process is underestimated by about 5% on average. The average bias in the estimate of the effect of democracy is still negligible, though the long-run impact of democracy on GDP may be severely underestimated.

A6 Additional Tests and Checks for the Dynamic Panel Model Estimates

A6.1 Robustness to Outliers

We investigate the robustness of our baseline within estimates to outliers in Table A8. Column 1 shows estimates for our baseline model for comparison. In column 2 we remove points with a standardized residual (in column 1's model) above 1.96 or below -1.96. In column 3 we remove points with a Cook's distance (in column 1's model) above the rule-of-thumb value of $4/NT$ (four over the number of observations). In column 4 we compute a robust regression estimator following Li (1985). Finally, in the last column we present a Huber M-estimator which is more resilient to outliers.

Overall, the results in Table A8 show that our within estimates are not driven by outliers. Remarkably, the long-run effect of democracy remains broadly unchanged from our preferred specification in Column 1.

A6.2 Additional GMM Estimates

Arellano and Bond's GMM estimator exploits a full set of moment conditions derived from Assumption 1. We now explore the robustness of our results to using different sets of moments in Table A9.

Column 1 presents our preferred within-country estimator, and column 2 shows the usual Arellano and Bond GMM estimator from Table 2. Column 3 replaces the moments formed using lags of democracy with the single moment $E[(\varepsilon_{ct} - \varepsilon_{ct-1})D_{ct-1}] = 0$. This brings the number of moments down to a half, as reported in the bottom rows. The estimated long-run effect of democracy is now 17.93%, which is slightly larger than the baseline GMM estimate and closer to our within estimate. Rather than using all available lags of GDP as instruments, column 4 uses up to the 25th lag of GDP when forming the GMM conditions. The results are again similar, but less precise. Column 5 uses a different approach, and instead of taking first differences of the data, it eliminates country fixed effects by taking orthogonal forward deviations. Moment conditions can then be constructed as in our baseline GMM estimator. This transformation allows us to capture the dynamics of GDP using only up to its fifth lag as instrument, cutting the number of moment conditions down significantly. Both the estimated persistence and the coefficient of democracy are greater in this case, implying a larger long-run effect of 37.56% (this effect is imprecisely estimated because GDP persistence is close to 1 in this case).

As an additional check, we add Ahn and Schmidt's (1995) additional moment conditions, which are non-linear and also derived from Assumption 1 (but not exploited by the Arellano and Bond estimator). The additional moments take the form (in a balanced panel)

$$\mathbb{E}[\varepsilon_{cT}(\varepsilon_{ct} - \varepsilon_{ct-1})] = 0 \forall t = 2, \ldots, T-1.$$

Columns 6, 7, and 8 present GMM estimators adding the Ahn and Schmidt moment conditions to the moment conditions exploited in columns 2, 3, and 4, respectively.[32] These additional nonlinear moment conditions improve our estimates of GDP dynamics and imply a somewhat larger persistence for GDP. Overall, we find slightly larger, but still plausible, long-run effects of democracy.

A6.3 Separating the Effect of Democratizations and Reversals

As noted in the main text, our dynamic panel model forces democratizations and reversals to have effects of the same magnitude but of opposite sign. Here, we relax this restriction and allow democ-

[32] We estimate these models using an iterative procedure. We start with the estimates obtained using the linear conditions, and at each step, we add the nonlinear conditions computed with the previous estimated coefficients. We iterate the procedure 15 times, which is sufficient for the estimates to converge in our case.

ratizations and reversals to have different coefficients in equation (1).

To do so, we let

$$\begin{aligned} DC_{ct} &\equiv \sum_{t' \leq t} \Delta D_{ct} 1\{\Delta D_{ct} = 1\} \\ RC_{ct} &\equiv \sum_{t' \leq t} \Delta D_{ct} 1\{\Delta D_{ct} = -1\}, \end{aligned}$$

denote the cumulative number of democratizations and reversals for country c at time t. Notice that $\Delta DC_{ct} = 1$ if there is a democratization at t, and $\Delta RC_{ct} = 1$ if there is a reversal, while $\Delta DC_{ct} = \Delta RC_{ct} = 0$ otherwise. This implies that democracy can be decomposed as $D_{ct} = DC_{ct} - RC_{ct}$, with DC_{ct} capturing the within-country variation in D_{ct} driven by democratizations and RC_{ct} capturing the within-country variation in D_{ct} driven by reversals.

Using this terminology, we consider the following generalization of our model:

$$y_{ct} = \beta^d DC_{ct} + \beta^r RC_{ct} + \sum_{j=1}^{p} \gamma_j y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}. \tag{A2}$$

Equation (1) now corresponds to the special case of this equation that imposes the restriction $\beta = \beta^d = -\beta^r$—so that democratizations and reversals have opposite effects on GDP of equal magnitudes.

Table A10 presents estimates of this model. Column 1 presents within estimates of equation (A2), controlling for four lags of GDP. Column 2 adds up to eight lags of GDP to allow for the possibility that reversals may be preceded by more long-lasting declines in GDP. Columns 3 and 4 present GMM estimates in which we instrument DC_{ct} and RC_{ct} using their lags. Columns 5 and 6 present results from the HHK estimator, in which we also instrument DC_{ct} and RC_{ct} using their lags.

Our results suggest that permanent democratizations are associated with an increase in GDP per capita of about 20% in the long run, and this effect is precisely estimated in most specifications. We find a similar long-run effect for reversals, though of the opposite sign and less precise. Interestingly, in no case can we reject the hypothesis that $\beta^d = -\beta^d$. These results imply that the estimates of equation (1) presented in the main text are driven by both the gains in growth from a democratization and the losses from reversals to nondemocracy. These findings, combined with the semi-parametric results for reversals in Section A7 of this Online Appendix, also suggest that democratizations, and not any transition to a new political regime, impacts GDP.

A7 Additional Checks for our Semi-Parametric Estimates

We start by presenting our estimates for reversals (transitions to nondemocracy). As in the main text, we focus on the average effect on growth on the treated (i.e., in countries that experienced a reversal to nondemocracy) and present results using the three estimation approaches outlined in the text. Figure

A5 depicts our estimates. Though our yearly estimates are now less precise, and pre-democratization behavior is somewhat noisier, these results on the whole suggest that reversals reduce GDP by about 20%, 20 to 25 years after they occur.

In the main text we focused on the average treatment effect on countries that democratized. Alternatively, here we present estimates of the average effect of a democratization on subsequent GDP growth. Figure A6 presents our semi-parametric estimates for the average treatment effect using the doubly-robust estimator. As anticipated in the main text, because the computation of the ATE requires a stronger form of the overlap assumption and precise estimates of potential outcomes for the few treated countries, these estimates are less precise and exhibit poor finite sample behavior.[33] Despite these shortcomings, the estimated average treatment effects exhibit a similar pattern to the one reported for the average effect on democratizers in Figures 3, 4, and 5, with GDP increasing gradually following a democratization and reaching a level 20% higher after 20 years.

We next present several estimates of the probit model for democratizations and reversals, which we use to compute the propensity scores. Our model for democratizations (defined analogously for reversals) is given by

$$\mathbb{P}\left(D_{ct}=1|D_{ct-1}=0,\{y_{ct-j}\}_{j\geq 1}\right)=\Phi\left(\frac{\delta_t+\sum_{j=1}^{p}\pi_j y_{ct-j}}{\sigma}\right),$$

with Φ the cumulative normal distribution.

The results reported in the main text are based on the predicted propensity score of the above model with $p=4$. We present alternative estimates of this model in the top panel (columns 1-5) of Table A11.

To underscore the role of temporary changes in GDP leading to a democratization, we rearrange the coefficients above and report the implied marginal effect of $\delta y_{ct-1}, \delta y_{ct-2}, \ldots, \delta y_{ct-p+1}$, and $\sum_{j=1}^{p} y_{ct-j}$, separately. We interpret the coefficient on the sum of the lags as the effect of a permanent increase in income on the likelihood of a democratization. Columns 1 to 4 present models with p ranging from 1 to 4.

Column 4 is our preferred specification, and the one used to compute our semi-parametric estimates in the main text. These estimates suggest that a 10% decrease in GDP at $t-2$ has the largest impact on the likelihood of a democratization, increasing it by 1.3 percentage points (standard error=0.45). This effect is quantitatively large, if we take into account that the average probability of a democratization in our sample is of 1.84 percentage points. In contrast, a permanent increase in GDP does not raise

[33]Because the ATE involves a separate regression to predict counterfactual outcomes for transitions to democracy and nondemocracy (whereas estimating average effects on the treated requires only the former) and because we have fewer transitions to nondemocracy, we cannot include year effects in the regression adjustment. For the same reason and because the overlap assumption starts failing, we could only compute these estimates for the first 20 years following a transition.

the likelihood of democracy, consistent with the evidence in Acemoglu et al. (2005), and our discussion of Assumption 2 in the main text.

One potential concern with our preferred estimates of the propensity score is that they may have poor finite sample performance if GDP is nonstationary (see Park and Phillips, 2000). To address this issue, the model in column 5 sets the permanent effect of GDP to zero and only allows changes in GDP to impact the likelihood of democratization. The estimated propensity score remains roughly unchanged, and its correlation with our baseline propensity score is .9965. This is not surprising, as our previous results implied that the effect of the *level* of GDP on the likelihood of a democratization is zero.

We also present several estimates of the propensity to revert to nondemocracy in columns 6-10 of Table A11. Contrary to democratizations, we find that GDP has a strong level effect on the likelihood of a reversal. In principle, this does not represent any threat to the validity of our empirical strategy provided that the propensity score is correctly specified. Nevertheless, as noted above, the propensity score estimates may have poor finite sample properties if GDP dynamics have a very high degree of persistence or an exact unit root.

Figure A7 plots the estimated density both for the propensity to transition to democracy (top panel) or to nondemocracy (bottom panel). The black line plots the smoothed density for "treated" countries in each case, and the gray line for "control" countries. Though the estimated propensities of a regime change are low, the figure reveals a considerable level of overlap (in particular, control observations cover the support of the treatment's propensity scores), providing support for strategies relying on the propensity score, and especially for estimates of the average treatment effects on the treated.

A8 Two Illustrative Examples of Democratization

In this Appendix subsection, we discuss two examples of transitions to democracy that illustrate our findings: the end of the Portuguese Estado Novo in 1974 and the South Korean transition to democracy in 1988.

In Portugal, the 1974 coup replaced Salazar's right-wing dictatorship with a left-wing dictatorship which, after a series of further coups, eventually gave way to democracy. Portugal held its first elections in 1976 (which is when we code it as a democracy). As emphasized by the low propensity score of this democratization episode in Table A1 (0.018), democracy was not an ex ante likely outcome in Portugal. There was no economic crisis precipitating the downfall of Salazar's dictatorship. Rather, democratization resulted from mounting discontent with, and the internal crisis of, the military regime (e.g., Fearon and Laitin, 2005, Gil Ferreira and Marshall, 1986, and Chilcote, 2010).

Similarly, in South Korea democracy was by no means a foregone conclusion, as reflected in the estimated propensity score of 0.02 (see again Table A1). The dictatorship's succession announcement on June 10, 1987 triggered large student protests. Nevertheless, large and even more daring pro-democracy protests had been decisively repressed earlier in the decade, notably in the Gwangju uprising of 1980. Repression was eschewed by the government this time, in part because of world image concerns in anticipation of the 1988 Olympics, and the regime acquiesced to holding elections (see Cumings, 1997).

The long-run growth effects of the resulting democratic transitions are evident in both cases. Portugal's real GDP per capita in 1975 was \$5,400, and grew at a 2.4% annual growth rate between 1976 and 2006. All of our estimators, and most clearly the semi-parametric ones in the previous subsection, compute the effects of transitions to democracy by comparing such growth experiences to those of countries with similar GDP (or GDP dynamics). For Portugal, the six countries with the closest GDP per capita in 1975 (Barbados, Gabon, Oman, Trinidad and Tobago, Uruguay, and Venezuela) had an average growth rate of 0.5% during the same period. South Korea's growth was even more impressive following its democratization, at 4.7% per year between 1988 and 2008, compared to an average of 2.6% among the six countries with the closest GDP per capita to South Korea in 1987 (St. Kitts and Nevis, Malta, Czechoslovakia/the Slovak Republic, Trinidad and Tobago, Uruguay, and Venezuela).

Also relevant to our discussion of mechanisms in Section 6, both countries undertook important reforms after their transition to democracy, in particular expanding health and education. The democratic Portuguese government created the National Health Scheme in 1979, and expanded rural primary health centers, cutting infant mortality in half (Gil Ferreira and Marshall, 1986). The Korean government similarly instituted universal health care one year after the transition to democracy. Portuguese secondary school enrollment increased from 55% to 97% over the 30 years after democratization, while newly democratic Korea stopped repressing unions, deregulated finance, and reformed regulations concerning competition and the *chaebols'* ownership of firms in the early 1990s (Lee, 2005).

A9 Additional Checks and Material for the IV Estimates

A9.1 Role of Regional Diffusion Patterns in Democracy and Political Discontent

In this subsection we document that democracy spreads more strongly within region $\times$ initial regime cells—as assumed in our IV strategy—than to countries depending solely on their distance, as economic shocks potentially do.

The top panel of Table A12 presents our results. In particular, it presents estimates obtained by regressing own country democracy on its own lag, a lagged jackknifed average of democracy in its region $\times$ initial regime cell (lagged regional democracy for simplicity), average democracy in other countries weighed by the inverse of their distance, and average democracy on neighboring countries. All these models include a full set of country and year fixed effects. Our findings suggest that innovations to democracy are highly correlated with lagged regional democracy, but not so much with distance-based averages of democracy or neighbors' democracy. When we include all these variables together, lagged regional democracy explains the bulk of the variation in the innovation, while distance-based measures of democracy have small and insignificant effects. Panel B shows that the same holds for unrest, which we view as a proxy for political discontent. Finally, in Panel C we do not find evidence of strong regional correlation or distance-based correlation in GDP shocks.

The findings in this section suggest that, as emphasized in classic accounts of the democratization process, historical, cultural, and political commonalities among countries in one region are more important than geographic distance in mediating the spread of democracy and political discontent. This provides further support for our choice of instruments. Moreover, the fact that we do not find such strong correlation in GDP within region $\times$ initial regime cells, suggests (but does not prove) that the commonalities that are useful for the diffusion of democracy are not so relevant for the spread of economic shocks, as required by our exclusion restriction.

A9.2 Robustness to Outliers (IV Estimates)

We now explore the robustness of our IV estimates to outliers in Table A13. We focus on our preferred IV specification presented in column 2, Panel A of Table 6. Column 1 reproduces these estimates for comparison. Columns 2-4 show estimates in which we identify outliers in the second stage. In column 2 we identify observations whose second-stage standardized residual is above 1.96 or below -1.96, and re-estimate the 2SLS model without these observations. In column 3 we identify observations whose second-stage Cook's distance is above the rule of thumb value of $4/NT$ (four over the number of observations), and re-estimate the 2SLS model without these observations. In column 4 we compute robust regression weights for the second stage following Li (1985) and re-estimate the 2SLS model using these weights. Overall, our results remain roughly unchanged, suggesting that our IV estimates are not driven by outliers in the second stage.

In the remaining columns, we present estimates in which we take into account the influence of outliers in both the first and second stage. To do so, we replace the first stage by an estimator that is robust to outliers, compute the predicted values using this robust estimator for the whole estimation sample, and estimate the second stage with the same robust estimator. We compute standard errors using a Sandwich estimator formula presented in Stefanski and Boos (2002) and that builds on Murphy and Topel (1985), which works for our two-step procedure. Column 5 presents results in which we remove observations with standardized errors above 1.96 or below -1.96 at each stage. Column 6 presents results in which we remove observations with a Cook's distance above four over the number of observations at each stage. Column 7 presents results estimating each stage using Li's (1985) procedure. Finally, column 8 presents results using a Huber $M-$estimator at each stage. We find similar long-run effects of democracy on growth, except in column 7. Overall, the evidence suggests that outliers have little effect on our IV estimates.

A9.3 Alternative Construction of Regional Instruments

In this section we show that our 2SLS estimates do not hinge on our particular construction of the democratic waves' instrument.

For our baseline instrument we define $D_{ct_0} = 1$ for countries that were democratic during the first five years they appear in our sample (recall that our estimation sample excludes periods in which countries were not independent). Though we find this definition intuitive, we explore the robustness of our results to using three different definitions of the initial regime D_{ct_0}. Columns 1-4 of Table A14 present the results.

In the first column, we code $D_{ct_0} = 1$ if a country is democratic from 1960-1964. In this coding, non-independent countries are coded as nondemocracies, $D_{ct_0} = 0$. Column 2 presents our 2SLS estimates using four lags of the instrument obtained with this alternative coding of the initial regime cells. The coefficient on democracy and the estimated long-run effect are larger than our baseline estimates in column 1, but still plausible.

Our second alternative is to code $D_{ct_0} = 1$ for countries that are always democratic in our sample. This has the drawback of using future information in the construction of the instrument, but has the advantage of putting together in one region $\times$ initial regime cell countries that eventually had transitions, which increases the predictive power of the instrument. Column 3 presents our 2SLS estimates using four lags of the instrument obtained with this alternative coding of the initial regime cells. The coefficient of democracy and the estimated long-run effect are larger than our baseline estimates in column 1, but still plausible and more precisely estimated.

Finally we explored a broader definition of initial regimes based on country characteristics in 1960. In particular we classified countries as British colonies, French colonies, civil dictatorships, military dictatorships, mixed and presidential democracies, parliamentary democracies, royal dictatorships and socialist regimes. We constructed the instrument as in equation (4), using this alternative region $\times$ initial regime classification (in this case we have 34 region $\times$ regime cells). The results using four lags of this alternative instrument are presented in column 4 and imply somewhat larger effects of democracy.

We also explore an alternative way of capturing regional waves other than the one presented in equation (4). In particular, we construct a set of instruments of the form

$$Z_{ct}^{ar} = 1\{D_{ct_0} = a, c \in r\} \times \frac{1}{N_r - 1} \sum_{c' \in r, c' \neq c} D_{c't},$$

with r indexing the seven geographic regions in our analysis and N_r the number of countries in each. Thus, the number of instruments equals the number of region $\times$ initial regime cells. The motivation for this construction is that regional democracy waves may have a differential effect on each region $\times$ initial regime cell.

Columns 5-8 of Table A14 present results using this alternative constructions of the instruments. We use four lags of the instruments as before. Column 5 presents 2SLS estimates obtained using our baseline definition of initial regimes. Columns 6-8 present results using this alternative construction of the instrument and each of the three alternative definitions of initial regime used in columns 2-4, respectively. All these 2SLS estimates produce results in the ballpark of our baseline 2SLS results.

Overall, the results suggest that our 2SLS results are not driven by the particular details or construction of our instrument.

A10 Appendix: Additional Heterogeneous Effects

Table A15 presents within estimates in which we also estimate the interaction of democracy with other measures of education. Columns 1-4 focus on the share of the population with primary education from the Barro-Lee dataset, while columns 5-8 present results using the share with tertiary education. We do not find evidence of a consistent interaction between democracy and these alternative measures of education.

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Table A1: Transitions to democracy in our sample.

Country	Year	Propensity score	Country	Year	Propensity score	Country	Year	Propensity score
Albania	1992	0.1687	Guinea-Bissau	2005	0.0669	Pakistan	1972	0.0158
Albania	1997	0.0169	Greece	1975	0.0126	Pakistan	1988	0.0351
Argentina	1973	0.0279	Grenada	1984	0.0117	Pakistan	2008	0.0523
Argentina	1983	0.0411	Guatemala	1966	0.0194	Panama	1994	0.0595
Armenia	1991	n.a.	Guatemala	1986	0.0283	Peru	1963	n.a.
Armenia	1998	0.0129	Guyana	1992	0.0725	Peru	1980	0.0160
Azerbaijan	1992	n.a.	Honduras	1982	0.0462	Peru	1993	0.1107
Burundi	2003	0.0195	Croatia	2000	0.0453	Philippines	1987	0.0195
Benin	1991	0.1196	Haiti	1990	n.a.	Poland	1990	n.a.
Burkina Faso	1977	0.0149	Haiti	1994	n.a.	Portugal	1976	0.0180
Bangladesh	1991	0.0975	Haiti	2006	0.0505	Paraguay	1993	0.1052
Bangladesh	2009	0.0167	Hungary	1990	0.0669	Romania	1990	0.0836
Bulgaria	1991	0.1115	Indonesia	1999	0.1128	Russia	1993	0.1532
Belarus	1991	n.a.	Kenya	2002	0.0386	Sudan	1965	0.0292
Bolivia	1982	0.0498	Kyrgyz Republic	2005	0.0434	Sudan	1986	0.0439
Brazil	1985	0.0263	Kyrgyz Republic	2010	0.0449	Senegal	2000	0.0467
Bhutan	2008	0.0410	Cambodia	1993	n.a.	Serbia & Montenegro	2000	n.a.
Central African Rep.	1993	0.1439	Korea	1988	0.0200	Solomon Islands	2004	0.0361
Chile	1990	0.0513	Lebanon	2005	0.0426	Sierra Leone	1996	0.0553
Côte d'Ivoire	2000	0.0514	Liberia	2004	0.0689	Sierra Leone	2001	0.0267
Congo, Republic of	1992	0.0758	Lesotho	1993	0.1022	El Salvador	1982	0.0823
Comoros	1990	0.0866	Lesotho	1999	0.0909	São Tomé & Príncipe	1991	n.a.
Comoros	1996	0.0561	Lithuania	1993	n.a.	Suriname	1988	0.0592
Comoros	2002	0.0383	Latvia	1993	0.2413	Suriname	1991	0.0755
Cape Verde	1991	0.0868	Moldova	1994	0.2090	Slovak Republic	1993	0.1690
Cyprus	1974	n.a.	Madagascar	1993	0.1503	Slovenia	1992	n.a.
Czech Republic	1993	n.a.	Mexico	1997	0.0395	Taiwan	1992	n.a.
Djibouti	1999	0.1158	Macedonia, FYR	1991	n.a.	Thailand	1974	0.0143
Dominican Republic	1978	0.0531	Mali	1992	0.0866	Thailand	1978	0.0473
Ecuador	1979	0.0443	Mongolia	1993	0.1734	Thailand	1992	0.0454
Spain	1978	0.0529	Mozambique	1994	0.1031	Thailand	2008	0.0485
Estonia	1992	0.0955	Mauritania	2007	0.0131	Turkey	1961	n.a.
Ethiopia	1995	0.0191	Malawi	1994	0.0973	Turkey	1973	0.0275
Fiji	1990	0.0642	Niger	1991	0.1173	Turkey	1983	0.0266
Georgia	1995	0.1025	Niger	1999	0.0958	Uganda	1980	n.a.
Ghana	1970	0.0193	Niger	2010	0.0581	Ukraine	1994	0.1402
Ghana	1979	0.0453	Nigeria	1979	0.0539	Uruguay	1985	0.0356
Ghana	1996	0.0435	Nigeria	1999	0.1001	South Africa	1994	0.0890
Guinea	2010	0.0564	Nicaragua	1990	0.1258	Zambia	1991	0.1177
Guinea-Bissau	1994	0.0900	Nepal	1991	0.0955	Zimbabwe	1978	0.0888
Guinea-Bissau	1999	0.1559	Nepal	2006	0.0394			

Notes: This table summarizes all democratization events in our sample. Democratizations are identified as transitions from nondemocracy to democracy using our dichotomous measure. For each democratization we report the country and the year in which it took place. The table also reports the estimated propensity score of each event based on lags of GDP and our model in Column 4, in the top panel of Table A11. Here, n.a. indicates insufficient GDP data to estimate the propensity score. The overall sample probability of a democratization following a period of nondemocracy is 0.0184.

Table A2: Reversals to nondemocracy in our sample.

Country	Year	Propensity score	Country	Year	Propensity score
Albania	1996	0.0252	Lebanon	1975	n.a.
Argentina	1976	0.0365	Lesotho	1998	0.0537
Armenia	1996	0.0777	Madagascar	2009	0.1156
Azerbaijan	1993	n.a.	Myanmar	1962	n.a.
Burkina Faso	1980	0.3021	Mauritania	2008	0.0286
Bangladesh	1974	0.1664	Niger	1996	0.1383
Bangladesh	2007	0.0189	Niger	2009	0.1274
Belarus	1995	0.0268	Nigeria	1966	0.1026
Brazil	1964	0.0393	Nigeria	1984	0.1212
Central African Rep.	2003	0.0592	Nepal	2002	0.0696
Chile	1973	0.0459	Pakistan	1977	0.1151
Côte d'Ivoire	2002	0.0261	Pakistan	1999	0.0365
Congo, Republic of	1963	n.a.	Panama	1968	0.0626
Congo, Republic of	1997	0.0251	Peru	1962	n.a.
Comoros	1976	n.a.	Peru	1968	0.0934
Comoros	1995	0.0484	Peru	1992	0.0143
Comoros	1999	0.0654	Philippines	1965	0.0758
Djibouti	2010	0.0354	Russia	2004	0.0050
Ecuador	1961	n.a.	Sudan	1969	0.1589
Ethiopia	2010	0.0984	Sudan	1989	0.1178
Fiji	1987	0.0224	Solomon Islands	2000	0.0237
Fiji	2006	0.0140	Sierra Leone	1967	0.2412
Ghana	1972	0.2532	Sierra Leone	1997	0.0449
Ghana	1981	0.0721	Somalia	1969	n.a.
Gambia, The	1994	0.0344	Suriname	1980	0.0657
Guinea-Bissau	1998	0.0842	Suriname	1990	0.0276
Guinea-Bissau	2003	0.0927	Thailand	1976	0.1459
Greece	1967	0.0289	Thailand	1991	0.0207
Grenada	1979	n.a.	Thailand	2006	0.0100
Guatemala	1974	0.0858	Turkey	1971	0.0340
Haiti	1991	n.a.	Turkey	1980	0.0526
Haiti	2000	0.0462	Uganda	1985	n.a.
Haiti	2010	0.0608	Uruguay	1972	0.0408
Kyrgyz Republic	2009	0.0970	Venezuela, Rep. Bol.	2009	0.0090
Cambodia	1995	n.a.	Zimbabwe	1987	0.1505
South Korea	1961	n.a.			

Notes: This table summarizes all reversal events in our sample. Reversals are identified as transitions from democracy to nondemocracy using our dichotomous measure. For each reversal we report the country and the year in which it took place. The table also reports the estimated propensity score of each event based on lags of GDP and our model in Column 4, in the bottom panel of Table A11. Here, n.a. indicates insufficient GDP data to estimate the propensity score. The overall sample probability of a reversal following a period of democracy is 0.0121.

Table A3: Empirical process for GDP and democracy used in the alternative counterfactual for our long-run estimates.

	AR(1) models (1)	AR(2) models (2)	AR(4) models (3)	AR(8) models (4)
	Panel A. Democracy equation:			
Propensity to democratize	0.025	0.025	0.025	0.026
	(0.003)	(0.002)	(0.002)	(0.003)
Democracy first lag	0.956	0.903	0.900	0.892
	(0.005)	(0.018)	(0.018)	(0.019)
Democracy second lag		0.055	0.002	0.000
		(0.017)	(0.024)	(0.026)
Democracy third lag			0.019	0.020
			(0.028)	(0.029)
Democracy fourth lag			0.041	0.005
			(0.018)	(0.027)
Democracy fifth lag				0.012
				(0.037)
Democracy first lag				0.046
				(0.029)
Democracy seventh lag				-0.034
				(0.024)
Democracy eight lag				0.024
				(0.014)
	Panel B. Estimation of Counterfactuals:			
Effect of democracy after 25 years	17.791	13.800	16.895	17.715
	(5.627)	(5.528)	(5.275)	(5.430)
Counterfactual likelihood of democracy	0.382	0.374	0.366	0.377
	(0.026)	(0.026)	(0.026)	(0.030)
Counterfactual growth	4.269	3.368	4.071	4.417
	(1.384)	(1.370)	(1.292)	(1.376)
Effect of democracy relative to counterfactual	13.521	10.432	12.824	13.299
	(4.300)	(4.194)	(4.042)	(4.132)
Observations	6,790	6,642	6,336	5,688
Countries in sample	175	175	175	175

Notes: This table reports a joint estimation of the GDP equation and an equation for democracy. The top panel presents estimates of the GDP equation. The bottom panel presents estimates of a model with democracy as dependent variable and lags of democracy as explanatory variables. All these models include a full set of country and year fixed effects. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A4: Effect of democracy on (log) GDP per capita. Estimates obtained by imposing the persistence of the GDP process to lie between 0.95 and 1.

Imposed persistence $\rho = \sum \gamma_j$:	$\rho = 0.95$ (1)	$\rho = 0.96$ (2)	$\rho = 0.97$ (3)	$\rho = 0.98$ (4)	$\rho = 0.99$ (5)	$\rho = 1$ (6)
			Panel A: Within estimates.			
Democracy	0.638	0.752	0.867	0.982	1.097	1.212
	(0.247)	(0.228)	(0.218)	(0.216)	(0.223)	(0.239)
Long-run effect of democracy	12.750	18.811	28.913	49.116	109.724	.
	(4.943)	(5.712)	(7.255)	(10.795)	(22.342)	.
Effect of democracy after 25 years	11.477	15.511	20.574	26.927	34.888	44.844
	(4.455)	(4.735)	(5.232)	(6.071)	(7.393)	(9.346)
Observations	6,336	6,336	6,336	6,336	6,336	6,336
Countries in sample	175	175	175	175	175	175
			Panel B: 2SLS estimates.			
Democracy	0.483	0.974	1.464	1.955	2.445	2.936
	(0.575)	(0.527)	(0.509)	(0.523)	(0.567)	(0.635)
Long-run effect of democracy	9.662	24.341	48.806	97.735	244.525	.
	(11.509)	(13.182)	(16.956)	(26.138)	(56.709)	.
Effect of democracy after 25 years	8.698	20.060	34.683	53.448	77.442	107.989
	(10.367)	(10.915)	(12.231)	(14.743)	(18.849)	(24.908)
Exc. Instruments F-stat.	34.86	34.86	34.86	34.86	34.86	34.86
Observations	6,309	6,309	6,309	6,309	6,309	6,309
Countries in sample	174	174	174	174	174	174

Notes: This table presents estimates of the effect of democracy on GDP per capita, imposing the persistence level of the GDP process at the top of each column. The coefficient on democracy is multiplied by 100. Panel A presents within estimates controlling for four lags of GDP per capita. Panel B presents 2SLS estimates instrumenting democracy with four lags of regional democracy waves and the F statistic for the excluded instruments. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A5: Summary of the Monte Carlo simulations for panels with different levels of persistence.

	Within estimator				GMM estimator			
Assumed persistence of GDP:	$\rho = 0.963$ (1)	$\rho = 0.97$ (2)	$\rho = 0.98$ (3)	$\rho = 0.99$ (4)	$\rho = 0.963$ (5)	$\rho = 0.97$ (6)	$\rho = 0.98$ (7)	$\rho = 0.99$ (8)
	Panel A: Assuming non-stationary initial conditions.							
Average persistence of GDP	0.9558	0.9637	0.9750	0.9864	0.9586	0.9662	0.9771	0.9881
(standard deviation)	(0.0016)	(0.0015)	(0.0012)	(0.0010)	(0.0019)	(0.0017)	(0.0014)	(0.0010)
Relative bias (Nickell bias):	-0.75%	-0.65%	-0.51%	-0.36%	-0.46%	-0.39%	-0.29%	-0.19%
Average coefficient of democracy	0.7772	0.7772	0.7771	0.7769	0.7889	0.7888	0.7886	0.7884
(standard deviation)	(0.2228)	(0.2228)	(0.2228)	(0.2230)	(0.2355)	(0.2355)	(0.2355)	(0.2355)
Average long-run effect of democracy	17.671	21.536	31.264	57.731	19.141	23.489	34.714	67.082
(standard deviation)	(5.266)	(6.460)	(9.537)	(18.452)	(5.908)	(7.301)	(10.991)	(22.476)
	Panel B: Assuming stationary initial conditions.							
Average persistence of GDP	0.9171	0.9238	0.9333	0.9485	0.9129	0.9175	0.9247	0.9539
(standard deviation)	(0.0049)	(0.0047)	(0.0045)	(0.0042)	(0.0096)	(0.0096)	(0.0095)	(0.0075)
Relative bias (Nickell bias):	-4.7%	-4.8%	-4.8%	-4.2%	-5.2%	-5.4%	-5.6%	-3.6%
Average coefficient of democracy	0.7688	0.7672	0.7642	0.7628	0.7499	0.7455	0.7394	0.7558
(standard deviation)	(0.2144)	(0.2141)	(0.2136)	(0.2133)	(0.2386)	(0.2383)	(0.2376)	(0.2376)
Average long-run effect of democracy	9.310	10.114	11.524	14.921	8.744	9.189	10.013	16.971
(standard deviation)	(2.686)	(2.929)	(3.361)	(4.404)	(3.032)	(3.226)	(3.587)	(6.438)

Notes: This table presents the average estimates obtained from 1,000 Monte Carlo simulations of samples for GDP and democracy that satisfy the same empirical properties as in our dataset. The persistence of GDP is set to the level indicated in the top row. The top panel presents results in which we assume that the initial level of GDP is independent of its stationary level. The bottom panel presents results in which we assume that the initial level of GDP is given by its stationary level. Columns 1 to 4 present results for the within estimator, and columns 5 to 8 present results for the GMM estimator. All the estimates and $t-$statistics are obtained using the within estimator and its standard asymptotic limit.

Table A6: Effect of democracy on (log) GDP per capita. Estimates obtained with alternative dichotomous measures of democracy.

Measure of democracy:	Ours (1)	PS (2)	Freedom House (3)	Polity IV (4)	CGV (5)	BMR (6)
	Panel A: Within estimates controlling for GDP dynamics.					
Democracy	0.787	0.785	0.652	0.152	0.323	0.530
	(0.226)	(0.287)	(0.222)	(0.251)	(0.259)	(0.271)
Long-run effect of democracy	21.240	21.457	13.332	4.406	8.835	14.654
	(7.215)	(8.515)	(4.577)	(7.463)	(7.437)	(7.910)
Effect of democracy after 25 years	16.895	16.967	11.938	3.462	6.996	11.700
	(5.297)	(6.440)	(4.040)	(5.774)	(5.774)	(6.128)
Persistence of GDP process	0.963	0.963	0.951	0.966	0.963	0.964
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
Observations	6,336	5,736	5,587	5,630	5,994	5,783
Countries in sample	175	153	174	153	175	174
	Panel B: 2SLS estimates controlling for GDP dynamics.					
Democracy	1.149	1.040	4.179	1.139	1.440	1.088
	(0.554)	(0.424)	(1.594)	(0.537)	(0.760)	(0.668)
Long-run effect of democracy	31.521	28.605	72.043	34.515	40.413	30.403
	(17.425)	(13.791)	(30.453)	(19.336)	(23.993)	(20.649)
Effect of democracy after 25 years	24.866	22.538	67.680	26.553	31.581	24.145
	(12.978)	(10.090)	(28.112)	(13.588)	(17.719)	(15.639)
Persistence of GDP process	0.964	0.964	0.942	0.967	0.964	0.964
	(0.005)	(0.005)	(0.007)	(0.005)	(0.005)	(0.005)
Observations	6,309	5,736	5,185	5,577	5,962	5,775
Countries in sample	174	153	174	151	174	174
	Panel C: Within estimates in levels ignoring GDP dynamics.					
Democracy	-10.112	-8.387	5.414	-11.377	-7.116	-4.225
	(4.316)	(6.746)	(3.150)	(4.091)	(4.713)	(4.482)
Observations	6,934	6,328	5,840	6,179	6,588	6,372
Countries in sample	175	153	174	154	175	174

Notes: This table presents estimates of the effect of democracy on GDP per capita, using alternative measures of democracy listed in the top row. PS stands for Papaioannou and Siourounis (2008), CGV stands for Cheibub, Gandhi, and Vreeland (2010), and BMR stands for Boix, Miller, and Rosato (2012). The coefficient of democracy is multiplied by 100. Panel A presents within estimates controlling for four lags of GDP per capita. Panel B presents 2SLS estimates instrumenting democracy with four lags of regional democracy waves and the F statistic for the excluded instruments. Panel C presents within estimates that do not control for GDP dynamics. In all specifications we control for a full set of country and year fixed effects. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A7: Effect of democracy on (log) GDP per capita. Estimates obtained with alternative measures of GDP.

	Within estimates	Arellano & Bond estimates	HHK estimates	2SLS estimates	HHK with IV estimates
	(1)	(2)	(3)	(4)	(5)
	Panel A. GDP per capita from the World Bank in constant local currency				
Democracy	0.858	1.202	0.712	1.430	0.565
	(0.237)	(0.487)	(0.387)	(0.680)	(0.610)
Long-run effect of democracy	21.110	17.326	9.265	35.699	10.782
	(7.106)	(8.027)	(5.475)	(20.321)	(12.415)
Effect of democracy after 25 years	17.307	16.006	8.570	29.044	8.675
	(5.248)	(7.148)	(4.977)	(15.459)	(9.788)
Persistence of GDP process	0.959	0.931	0.923	0.960	0.948
	(0.007)	(0.012)	(0.008)	(0.007)	(0.010)
Observations/Countries	6,377/181	6,195/181	6,195/181	6,350/180	6,350/180
	Panel B. GDP per capita from the Penn World Tables 9.0				
Democracy	0.473	1.141	1.268	0.497	1.139
	(0.236)	(0.528)	(0.496)	(0.727)	(0.695)
Long-run effect of democracy	11.638	18.436	19.609	12.869	22.931
	(5.782)	(8.987)	(8.598)	(19.308)	(15.188)
Effect of democracy after 25 years	8.863	16.128	16.827	9.776	18.000
	(4.371)	(7.664)	(7.024)	(14.535)	(11.388)
Persistence of GDP process	0.959	0.938	0.935	0.961	0.950
	(0.006)	(0.008)	(0.011)	(0.006)	(0.010)
Observation/Countries	7,204/183	7,021/183	7,021/183	7,159/182	7,159/182
	Panel C. GDP per capita from the Penn World Tables 7.0 (PPP adjusted)				
Democracy	0.646	1.068	0.705	0.674	0.471
	(0.280)	(0.604)	(0.406)	(0.787)	(0.645)
Long-run effect of democracy	15.483	15.897	8.529	17.025	8.530
	(6.781)	(10.303)	(5.149)	(20.747)	(11.820)
Effect of democracy after 25 years	11.399	13.795	7.777	12.436	6.902
	(4.858)	(8.601)	(4.615)	(14.895)	(9.519)
Persistence of GDP process	0.958	0.933	0.917	0.960	0.945
	(0.007)	(0.012)	(0.011)	(0.006)	(0.010)
AR2 test p-value		0.38			
Observations/Countries	6,990/181	6,809/181	6,809/181	6,945/180	6,945/180

Notes: This table presents estimates of the effect of democracy on log GDP per capita. Each panel presents results using a different measure of GDP. The reported coefficient on democracy is multiplied by 100. Column 1 presents results using the within estimator. Column 2 presents results using Arellano and Bond's GMM estimator. Column 3 presents results using the HHK estimator. Columns 4 presents 2SLS estimates instrumenting democracy with four lags of regional democracy waves and the p-value of a Hansen overidentification test. Column 5 presents results using the HHK estimator instrumenting democracy with four lags of regional democracy waves. In all specifications we control for a full set of country and year fixed effects. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

Table A8: Effects of democracy on (log) GDP per capita. The estimates control for the influence of outliers.

	(1)	(2)	(3)	(4)	(5)
Democracy	0.787	0.558	0.596	0.397	0.490
	(0.226)	(0.178)	(0.173)	(0.143)	(0.171)
log GDP first lag	1.238	1.225	1.234	1.229	1.240
	(0.038)	(0.015)	(0.016)	(0.011)	(0.009)
log GDP second lag	-0.207	-0.197	-0.212	-0.205	-0.209
	(0.046)	(0.022)	(0.022)	(0.017)	(0.015)
log GDP third lag	-0.026	-0.028	-0.020	-0.034	-0.031
	(0.028)	(0.018)	(0.016)	(0.014)	(0.014)
log GDP fourth lag	-0.043	-0.029	-0.029	-0.013	-0.026
	(0.017)	(0.010)	(0.010)	(0.009)	(0.009)
Long-run effect of democracy	21.240	19.423	21.983	18.086	19.003
	(7.215)	(7.039)	(7.418)	(7.019)	(6.919)
Effect of democracy after 25 years	16.895	13.055	14.276	9.999	12.074
	(5.297)	(4.338)	(4.334)	(3.672)	(4.249)
Persistence of GDP process	0.963	0.971	0.973	0.978	0.974
	(0.005)	(0.003)	(0.003)	(0.002)	(0.002)
Observations	6,336	6,046	6,027	6,160	6,336

Notes: This table presents within estimates of the effect of democracy on log GDP per capita. The coefficient on democracy is multiplied by 100. Column 1 presents our baseline within estimates. Column 2 removes observations with a a standardized residual estimated above 1.96 or below -1.96. In Column 3 we remove observations with Cook's distance above the rule of thumb value of four over the number of observations. Following Li (1985), in Column 4 we compute a robust regression estimator that assigns outliers a lower weight. In Column 5 we present a Huber M estimator, which is less sensitive to the presence of outliers. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A9: Effect of democracy on (log) GDP per capita. GMM estimates that exploit alternative sets of moment conditions.

	Within estimator	Arellano & Bond, different set of moments				Adding Ahn & Schmidt moments from columns 2 to 4		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy	0.787	0.875	0.994	1.034	1.268	1.107	1.257	1.461
	(0.226)	(0.374)	(0.554)	(0.700)	(0.607)	(0.336)	(0.508)	(0.661)
log GDP first lag	1.238	1.204	1.204	1.176	1.238	1.230	1.241	1.237
	(0.038)	(0.041)	(0.047)	(0.048)	(0.051)	(0.039)	(0.043)	(0.043)
log GDP second lag	-0.207	-0.193	-0.193	-0.183	-0.207	-0.202	-0.204	-0.203
	(0.046)	(0.045)	(0.047)	(0.046)	(0.049)	(0.046)	(0.047)	(0.047)
log GDP third lag	-0.026	-0.028	-0.027	-0.026	-0.027	-0.029	-0.029	-0.030
	(0.028)	(0.028)	(0.028)	(0.027)	(0.028)	(0.028)	(0.029)	(0.028)
log GDP fourth lag	-0.043	-0.036	-0.039	-0.038	-0.039	-0.039	-0.045	-0.045
	(0.017)	(0.020)	(0.020)	(0.022)	(0.017)	(0.019)	(0.020)	(0.021)
Long-run effect of democracy	21.240	16.448	17.930	14.526	37.564	27.928	33.321	36.386
	(7.215)	(8.436)	(11.679)	(10.810)	(30.953)	(10.787)	(17.133)	(20.106)
Effect of democracy after 25 years	16.895	14.713	16.307	13.885	28.391	22.743	26.965	30.193
	(5.297)	(7.128)	(10.191)	(10.184)	(18.483)	(7.917)	(12.562)	(15.440)
Persistence of GDP process	0.963	0.947	0.945	0.929	0.966	0.960	0.962	0.960
	(0.005)	(0.009)	(0.011)	(0.013)	(0.015)	(0.006)	(0.008)	(0.008)
AR2 test p-value		[0.51]	[0.45]	[0.53]	[0.32]	[0.46]	[0.38]	[0.39]
Moments		2,509	1,266	941	231	2,555	1,312	987
Observations	6,336	6,161	6,161	6,161	6,161	6,161	6,161	6,161
Countries in sample	175	175	175	175	175	175	175	175

Notes: This table presents different GMM estimates of the effect of democracy on log GDP per capita. The coefficient on democracy is multiplied by 100. Column 1 presents our baseline within estimates. Columns 2-4 remove the country fixed effects by taking first differences of the data and estimates the model by GMM. Column 2 uses Arellano and Bond's moment conditions, while columns 3 and 4 use different subsets of moment conditions described in the appendix. In Column 5 we remove fixed effects using forward orthogonal differences, and estimate the model using fewer moment conditions. In Columns 6-8 we add Ahn and Schmidt (1995) non-linear moment conditions to the models in columns 2-4. The AR2 row reports the p-value for a test of serial correlation in the residuals of the GDP series. The number of moments used by each estimator is reported below it. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A10: Effect of transitions in and out of democracy on (log) GDP per capita.

	Within estimator		Arellano & Bond GMM estimator		HHK estimator	
	(1)	(2)	(3)	(4)	(5)	(6)
Democratizations	0.803	0.894	1.470	0.846	0.947	1.132
	(0.235)	(0.256)	(0.543)	(0.524)	(0.544)	(0.488)
Reversals	-0.705	-0.853	-1.313	-1.123	-0.465	-0.753
	(0.335)	(0.376)	(0.957)	(0.860)	(0.796)	(0.743)
Long-run effect of democracy	21.770	22.199	27.377	15.141	18.955	24.317
	(7.635)	(8.186)	(12.982)	(11.165)	(13.581)	(13.790)
Effect of democracy after 25 years	17.283	17.855	24.617	13.471	16.204	20.492
	(5.560)	(5.743)	(10.786)	(9.370)	(10.640)	(10.269)
Long-run effect of reversal	-19.116	-21.200	-24.450	-20.089	-9.301	-16.187
	(9.302)	(9.785)	(17.763)	(15.466)	(16.526)	(16.403)
Effect of reversal after 25 years	-15.177	-17.051	-21.985	-17.872	-7.951	-13.642
	(7.256)	(7.587)	(16.098)	(13.627)	(13.937)	(13.617)
Persistence of GDP process	0.963	0.960	0.946	0.944	0.950	0.953
	(0.005)	(0.007)	(0.011)	(0.012)	(0.013)	(0.013)
Observations	6,336	5,688	6,161	5,513	6,161	5,513
Countries in sample	175	175	175	175	175	175
Number of GDP lags:	4	8	4	8	4	8

Notes: This table presents estimates of the effect of democracy on GDP per capita, allowing democratizations and reversals to have different effects. The coefficient on democratizations and reversals is multiplied by 100. Columns 1 and 2 present within estimates. Columns 3 and 4 present Arellano and Bond GMM estimates. Columns 5 and 6 present HHK estimates. Even columns add up to eight lags of GDP as controls. In all specifications we control for a full set of country and year fixed effects, as well as four lags of GDP. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A11: Marginal effects of GDP lags on the propensity to democratize.

	(1)	(2)	(3)	(4)	(5)
	Panel A: Probability of a democratization.				
Change in GDP at $t-1$		-0.126	-0.086	-0.076	-0.075
		(0.042)	(0.047)	(0.050)	(0.051)
Change in GDP at $t-2$			-0.121	-0.128	-0.129
			(0.045)	(0.048)	(0.048)
Change in GDP at $t-3$				-0.011	-0.013
				(0.049)	(0.049)
GDP level effect	-0.002	-0.002	-0.002	-0.002	
	(0.003)	(0.003)	(0.003)	(0.003)	
Observations	2,832	2,752	2,706	2,616	2,616
	Panel B: Probability of a reversal.				
Change in GDP at $t-1$		-0.094	-0.133	-0.100	-0.106
		(0.044)	(0.046)	(0.050)	(0.064)
Change in GDP at $t-2$			0.074	0.080	0.079
			(0.062)	(0.069)	(0.091)
Change in GDP at $t-3$				-0.077	-0.138
				(0.054)	(0.062)
GDP level effect	-0.017	-0.017	-0.017	-0.017	
	(0.002)	(0.002)	(0.002)	(0.002)	
Observations	2,882	2,836	2,741	2,552	2,552

Notes: This table presents the estimated marginal effects derived from a Probit model of the propensity to democratize (top panel) or revert to nondemocracy (bottom panel) based on past dynamics of GDP. In the top panel, the sample comprises the countries that were nondemocracies at time $t-1$. In the bottom panel, the sample comprises the countries that were democracies at time $t-1$. For each sample we estimate the probability of a transition based on past levels of GDP and year effects. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A12: Spatial patterns of diffusion for democracy, unrest, and GDP per capita.

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Spatial diffusion patterns for democracy.					
Lagged democracy	0.812	0.837	0.835	0.810	0.811	0.810
	(0.015)	(0.013)	(0.013)	(0.015)	(0.015)	(0.015)
Lagged regional democracy	0.143			0.150	0.147	0.150
	(0.022)			(0.024)	(0.023)	(0.024)
Lagged distance-weighted democracy		0.130		-0.029		-0.027
		(0.056)		(0.058)		(0.058)
Lagged neighbors' average democracy			0.024		-0.003	-0.001
			(0.013)		(0.013)	(0.013)
Observations	6,799	6,730	6,730	6,700	6,700	6,700
Countries in sample	174	174	174	173	173	173
	Panel B: Spatial diffusion patterns for unrest.					
Lagged unrest	0.291	0.284	0.284	0.283	0.283	0.283
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Lagged regional unrest	0.103			0.079	0.101	0.080
	(0.051)			(0.054)	(0.053)	(0.055)
Lagged distance-weighted unrest		0.211		0.142		0.176
		(0.130)		(0.140)		(0.152)
Lagged neighbors' average unrest			0.007		-0.002	-0.014
			(0.021)		(0.021)	(0.023)
Observations	7,027	6,730	6,730	6,708	6,708	6,708
Countries in sample	174	174	174	173	173	173
	Panel C: Spatial diffusion patterns for GDP.					
Lagged GDP	0.972	0.972	0.972	0.970	0.970	0.970
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Lagged regional GDP	0.007			0.007	0.007	0.007
	(0.006)			(0.006)	(0.006)	(0.006)
Lagged distance-weighted GDP		0.003		0.001		-0.002
		(0.017)		(0.017)		(0.017)
Lagged neighbors' average GDP			-0.002		-0.002	-0.002
			(0.001)		(0.001)	(0.001)
Observations	6,941	6,730	6,730	6,703	6,703	6,703
Countries in sample	174	174	174	173	173	173

Notes: This table reports estimates of the association between innovations to democracy and lagged regional democracy (by initial regime), lagged average democracy weighted by inverse distance and lagged neighbors' democracy. Panel B and C present analogous estimates for unrest and GDP. All models include a full set of country and year fixed effects. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Table A13: Effects of democracy on (log) GDP per capita. 2SLS estimates that also control for the influence of outliers.

		Robust second stage			Robust first and second stage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy	1.149	0.869	0.813	0.836	1.098	0.716	0.507	0.843
	(0.554)	(0.446)	(0.454)	(0.395)	(0.500)	(0.388)	(0.268)	(0.385)
log GDP first lag	1.238	1.228	1.235	1.231	1.332	1.244	1.232	1.242
	(0.038)	(0.015)	(0.016)	(0.011)	(0.020)	(0.017)	(0.008)	(0.009)
log GDP second lag	-0.205	-0.195	-0.207	-0.204	-0.307	-0.219	-0.206	-0.209
	(0.046)	(0.021)	(0.022)	(0.017)	(0.033)	(0.024)	(0.013)	(0.015)
log GDP third lag	-0.029	-0.034	-0.032	-0.039	-0.023	-0.029	-0.038	-0.035
	(0.028)	(0.017)	(0.016)	(0.013)	(0.024)	(0.018)	(0.012)	(0.015)
log GDP fourth lag	-0.040	-0.027	-0.022	-0.009	-0.032	-0.021	-0.009	-0.022
	(0.018)	(0.010)	(0.010)	(0.008)	(0.015)	(0.011)	(0.008)	(0.009)
Long-run effect of democracy	31.521	30.743	31.227	39.697	36.859	28.677	23.529	33.757
	(17.425)	(16.896)	(19.210)	(20.397)	(19.517)	(18.020)	(14.029)	(16.508)
Effect of democracy after 25 years	24.866	20.547	19.755	21.298	27.861	17.691	12.844	21.002
	(12.978)	(10.776)	(11.386)	(10.235)	(13.571)	(10.045)	(7.002)	(9.808)
Persistence of GDP process	0.964	0.972	0.974	0.979	0.970	0.975	0.978	0.975
	(0.005)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.002)
Observations	6,309	6,015	6,000	6,133	5,967	5,612	6,309	6,309

Notes: This table presents 2SLS estimates of the effect of democracy on GDP per capita that instrument democracy with four lags of regional democracy. The coefficient on democracy is multiplied by 100. Column 1 presents our baseline 2SLS estimates. In Column 2 we remove observations with a standardized residual above 1.96 or below -1.96 in the second stage. In Column 3 we remove points with estimated Cook's distance above the rule of thumb value of four over the number of observations in the second stage. In Column 4 we compute robust regression weights for the second stage following Li (1985), and re-estimate the model by 2SLS using these weights. In Column 5 we estimate the first and second stage manually excluding at each step countries with a standardized residual estimated above 1.96 or below -1.96. In Column 6 we estimate the first and second stage manually, excluding at each step countries with Cooks' distance above 4 over the number of observations. In Column 7 we estimate each stage using a robust estimator following Li (1985). In Column 8 we estimate each stage using a Huber M estimator. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses. Standard errors for our two step procedures in columns 5 to 8 are obtained following the adjustments proposed by Stefanski and Boos (2002) and Murphy and Topel (1985). We report the estimated persistence of the GDP process and the $p-$value for this being less than 1. We also report the estimated long-run effect of democracy and the $p-$value for this being different from 0.

Table A14: Effect of democracy on (log) GDP per capita. 2SLS estimates using alternative definitions of the regional democratization waves.

Instrument construction:	Baseline				Alternative			
Initial regime measured at:	Base	1960-65	All years	Various	Base	1960-65	All years	Various
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy	1.149	1.598	1.672	1.996	0.849	0.988	1.041	0.939
	(0.554)	(0.674)	(0.552)	(0.909)	(0.512)	(0.606)	(0.547)	(0.539)
Long-run effect of democracy	31.521	44.573	46.118	56.717	23.028	26.926	28.027	25.646
	(17.425)	(22.706)	(19.516)	(32.291)	(15.878)	(18.381)	(17.293)	(16.425)
Effect of democracy after 25 years	24.866	34.853	36.229	43.962	18.275	21.313	22.297	20.299
	(12.978)	(16.384)	(13.743)	(22.659)	(11.880)	(13.850)	(12.819)	(12.378)
Persistence of GDP process	0.964	0.964	0.964	0.965	0.963	0.963	0.963	0.963
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.006)	(0.005)
Exc. instruments F-stat.	254.5	167.5	302.8	121.9	28.7	20.4	28.1	16.1
Observations	6,309	6,270	6,330	5,906	6,309	6,270	6,330	5,906
Countries in sample	174	173	175	164	174	173	175	164

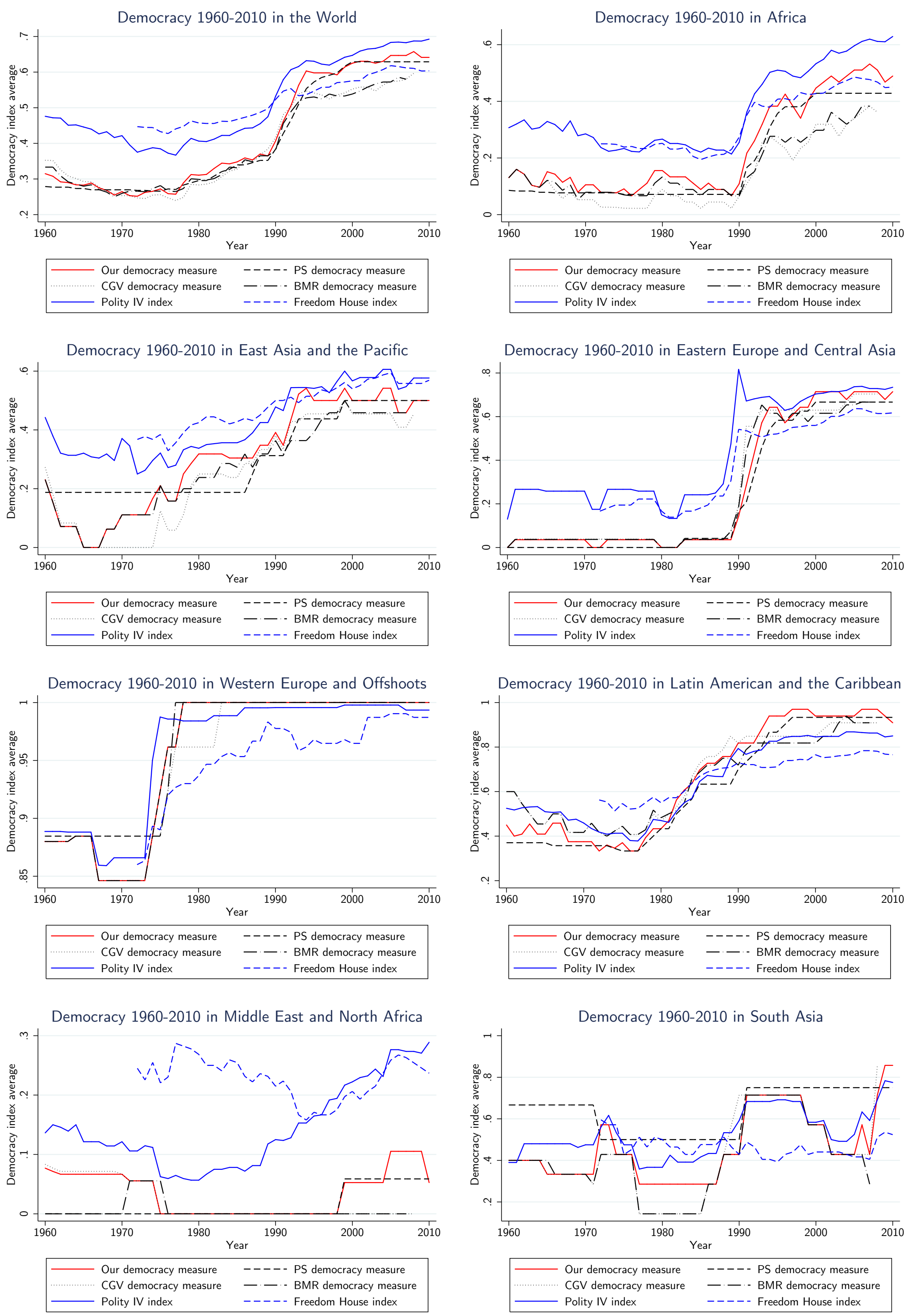
Notes: This table presents 2SLS estimates of the effect of democracy in GDP per capita using alternative constructions of the regional democracy instrument. The coefficient on democracy is multiplied by 100. In all models we instrument democracy using four lags of the alternative instruments. In columns 1-4, we use the baseline construction of the instrument. In columns 5-8 we use the alternative instruments described in the appendix. In columns 1 and 5 we use the baseline definition of initial regimes. In columns 2 and 6 we define initial regimes based on whether they were democratic during 1960-1964. We consider countries that were not independent as nondemocratic. In columns 3 and 7 we define initial regimes based on whether they were democratic throughout the sample. In columns 4 and 8 we use a richer set of initial regimes described in the text to construct the instrument. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses. We report the estimated persistence of the GDP process and the $p-$value for this being less than 1. We also report the estimated long-run effect of democracy and the $p-$value for this being different from 0. The F statistic for the excluded instruments is reported below each estimate.

Table A15: Heterogeneous effects of democracy on (log) GDP per capita (additional estimates).

Interaction with:	Share with primary:				Share with tertiary:			
Measured at:	1960	1970	1980	Lagged	1960	1970	1980	Lagged
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democracy	0.573	0.537	0.537	0.443	0.531	0.507	0.537	0.660
	(0.271)	(0.279)	(0.268)	(0.257)	(0.252)	(0.253)	(0.260)	(0.269)
Interaction	0.008	0.008	0.010	0.016	0.182	0.136	0.073	0.031
	(0.007)	(0.007)	(0.007)	(0.008)	(0.099)	(0.070)	(0.046)	(0.042)
Long-run effect of democracy	17.730	16.561	16.488	13.481	16.532	15.746	16.624	20.037
	(9.493)	(9.667)	(9.302)	(8.693)	(8.592)	(8.558)	(8.882)	(9.081)
Effect of democracy after 25 years	12.952	12.115	12.099	9.936	12.041	11.480	12.141	14.804
	(6.460)	(6.628)	(6.370)	(6.041)	(5.914)	(5.925)	(6.109)	(6.307)
Persistence of GDP process	0.968	0.968	0.967	0.967	0.968	0.968	0.968	0.967
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)
Observations	5,300	5,300	5,300	5,300	5,300	5,300	5,300	5,300
Countries in sample	138	138	138	138	138	138	138	138

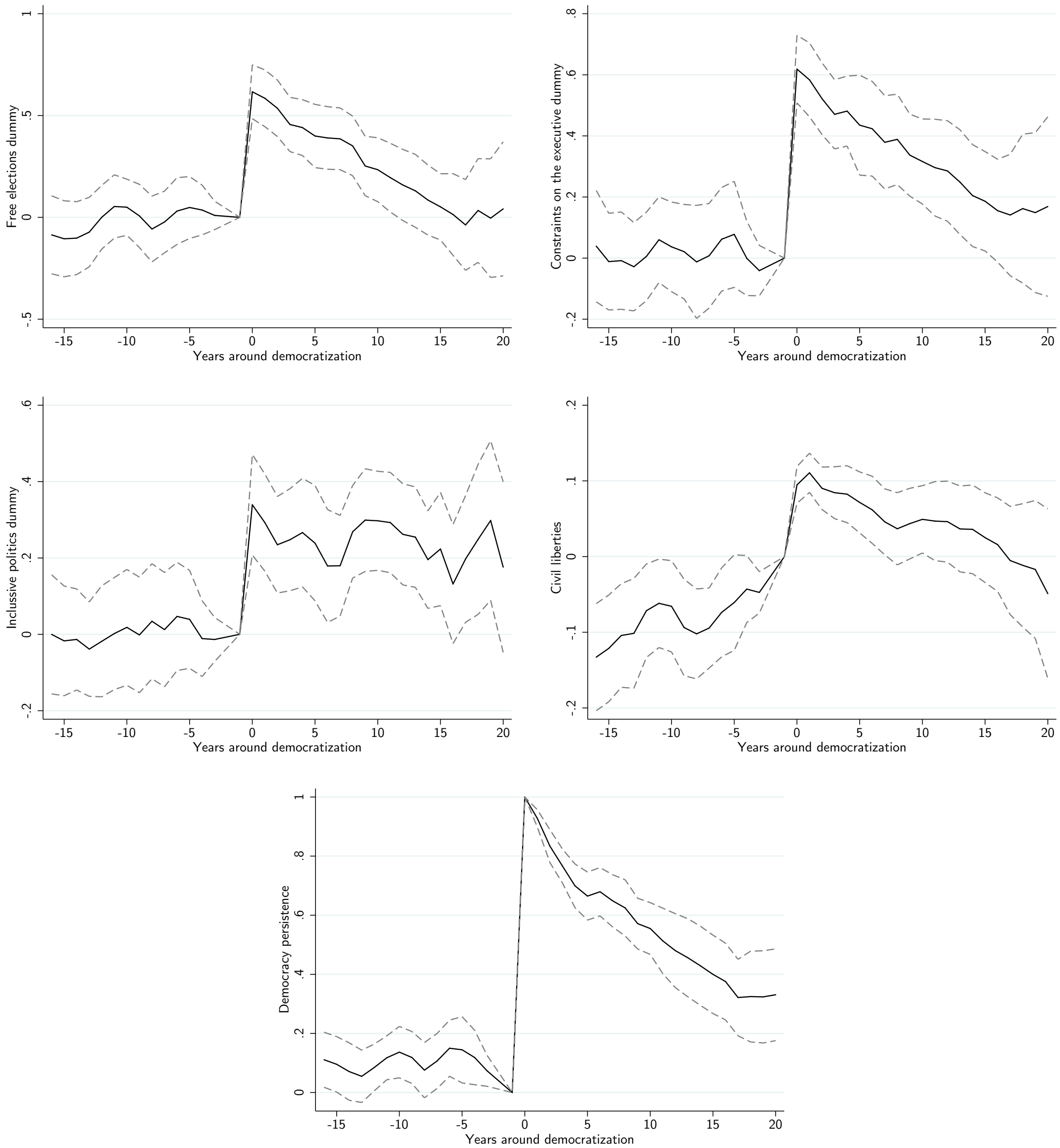
Notes: This table presents within estimates of the effect of democracy on log GDP per capita and its interaction with other country characteristics. The column labels specify the variable interacted with democracy in each model. The reported coefficients on democracy and the interaction are multiplied by 100. We report main effects and long-run effects evaluated at the 25th percentile of the interacted variable. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

Figure A1: Different measures of democracy averaged across regions and worldwide.



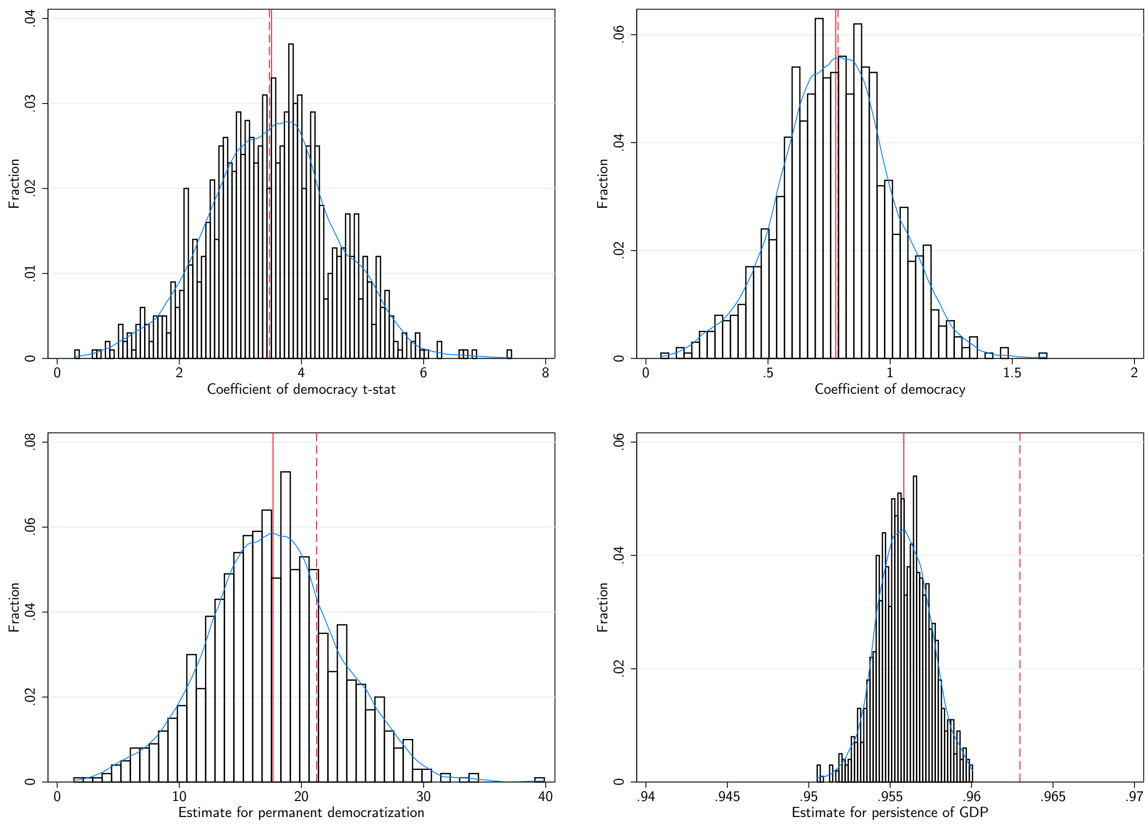
Notes: The figures present the evolution over time of the several democracy measures for each of the seven regions used in the paper, as well as for the whole world.

Figure A2: Institutional changes that follow an episode of democratization.



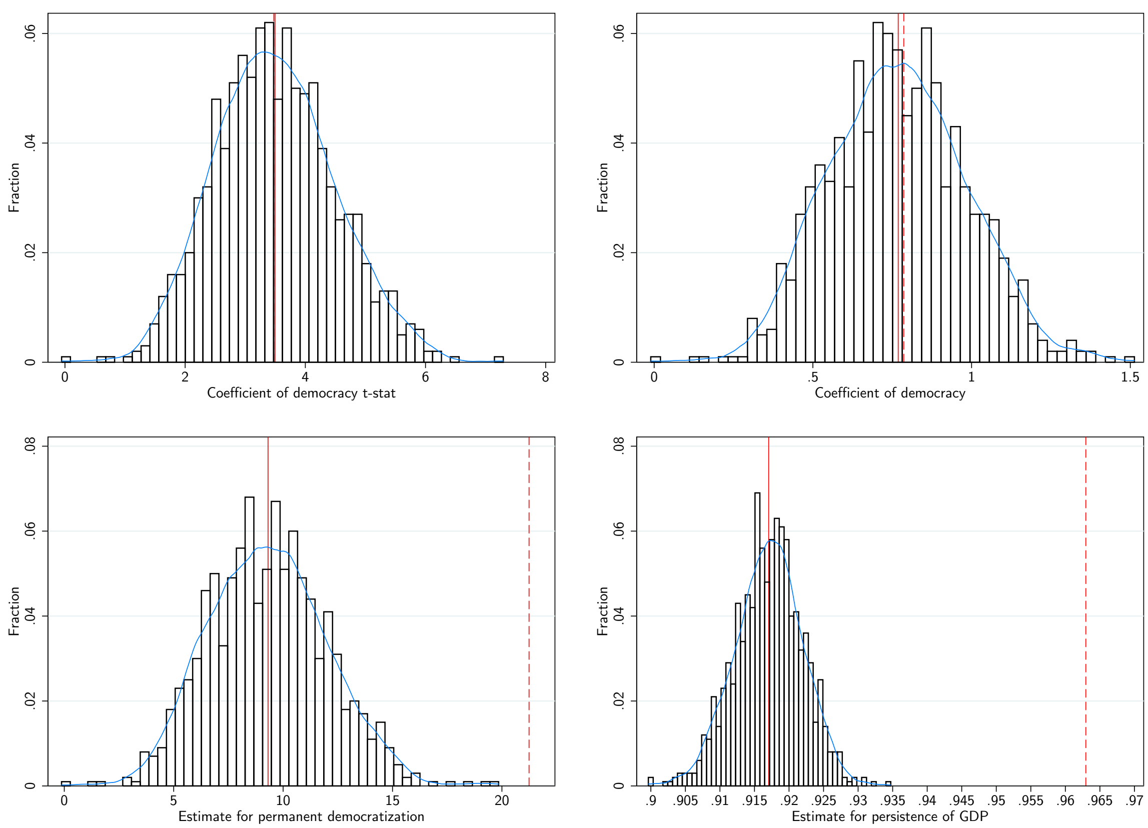
Notes: The figures plots the behavior of different components of democracy around a democratization (relative to continuing nondemocracies). Time (in years) relative to the year of democratization runs on the horizontal axis. See the text for a detailed explanation of how we measure these components separately from Polity IV and Freedom House raw data.

Figure A3: Results of our Monte Carlo simulations, non-stationary initial conditions.



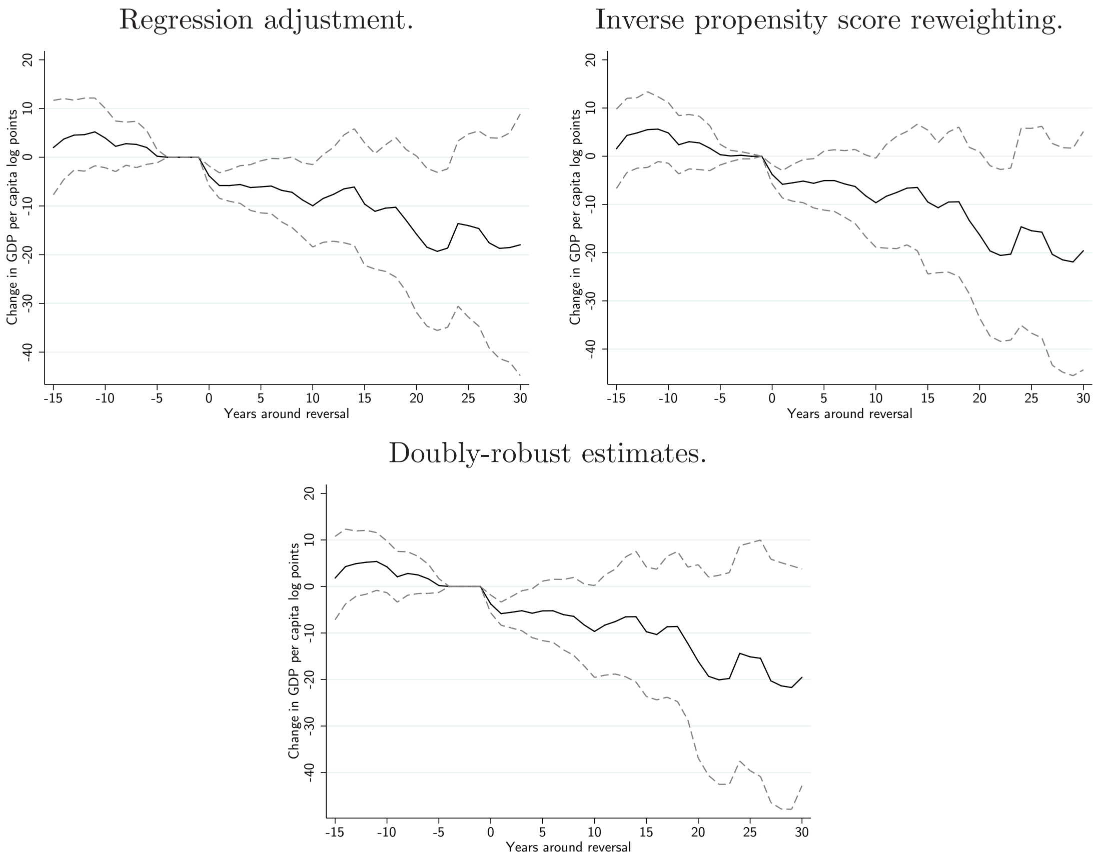
Notes: These figures plot the histograms and smoothed densities that we obtained for several estimates in our Monte Carlo simulation. The dashed red line indicates the population paremeters, and the solid red line indicates the average estimate over 1,000 simulations. In this case, the simulations assume that the initial GDP in each country is independent of the level implied by its GDP process.

Figure A4: Results of our Monte Carlo simulations, stationary initial conditions.



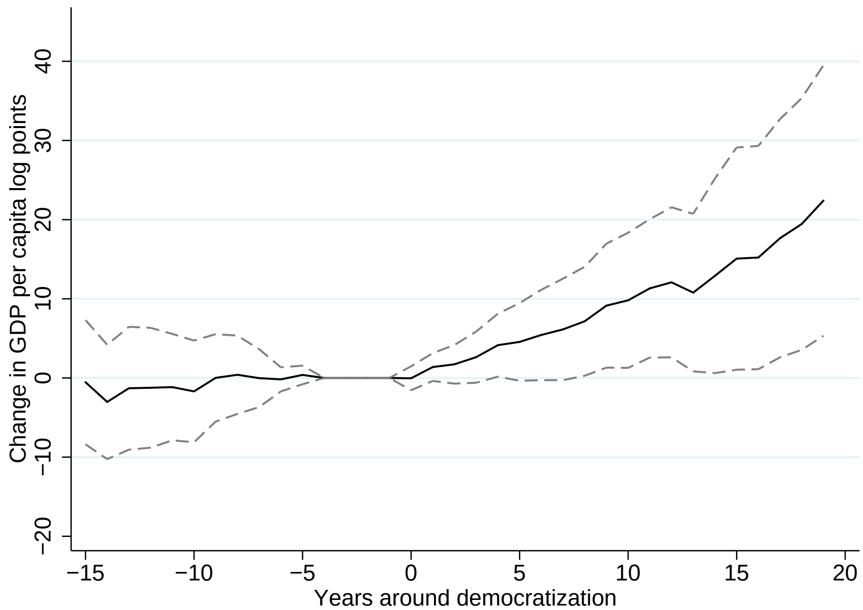
Notes: These figures plot the histograms and smoothed densities that we obtained for several estimates in our Monte Carlo simulation. The dashed red line indicates the population paremeters, and the solid red line indicates the average estimate over 1,000 simulations. In this case, the simulations assume that the initial GDP in each country is given by the level implied by its GDP process.

Figure A5: Semi-parametric estimates of the over-time effects of a reversal to nondemocracy on the log of GDP.



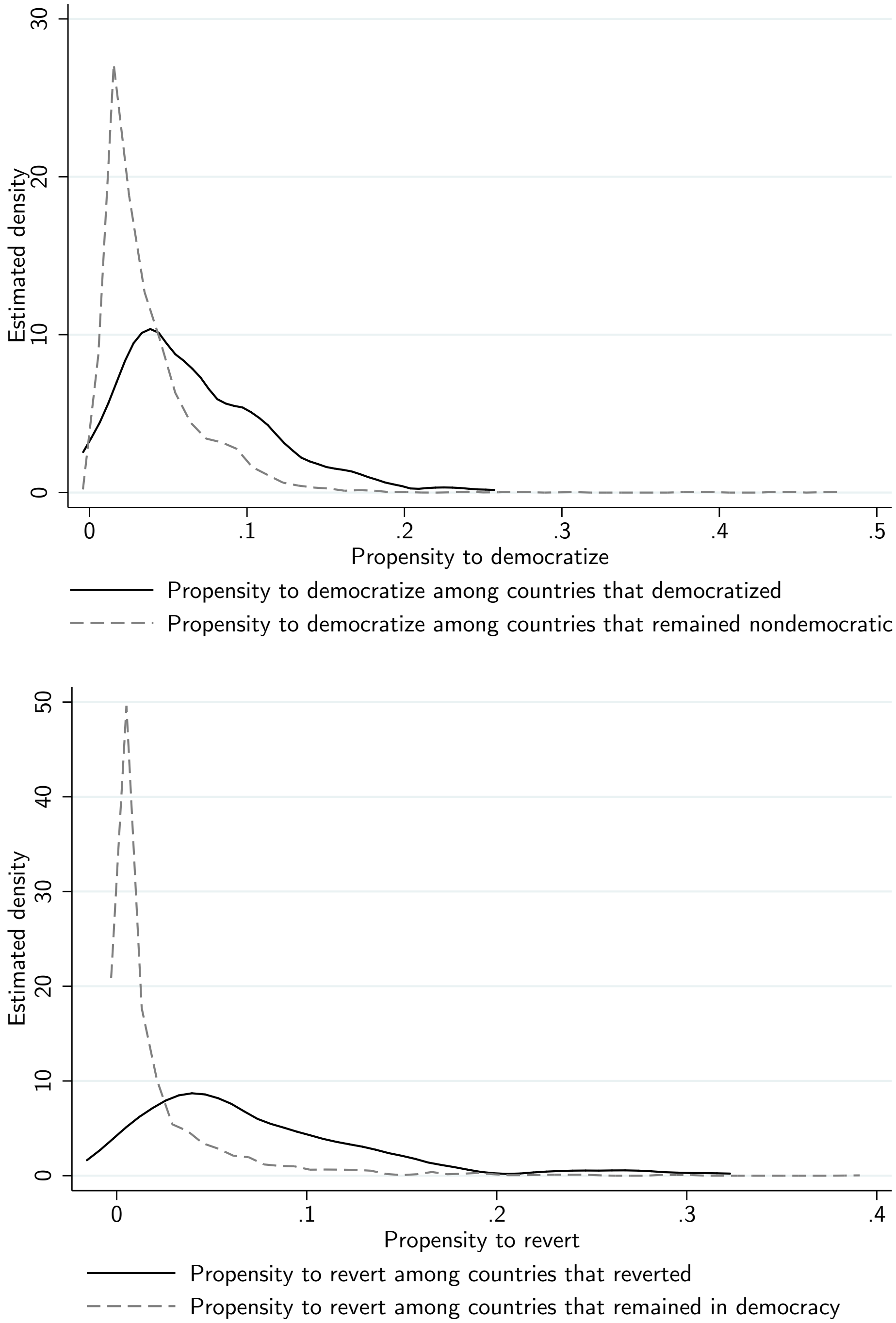
Notes: These figures plot semi-parametric estimates of the effect of a reversal to nondemocracy on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita (in log points) on countries that reverted, together with a 95% confidence interval in dashed lines. Time (in years) relative to the year of reversal runs on the horizontal axis.

Figure A6: Semi-parametric estimates of the over-time effects of a democratization on the log of GDP. Doubly-robust estimates for the average treatment effect.



Notes: This figure plots semi-parametric estimates of the effect of democratizations on GDP per capita in log points, using the doubly-robust estimator. The solid line plots the estimated average effect on GDP per capita (in log points), together with a 95% confidence interval in dashed lines. Time (in years) relative to the year of democratization runs on the horizontal axis.

Figure A7: Smoothed density for the estimated propensity to democratize or revert to nondemocracy.



Notes: These figures plots the smoothed density of the estimated propensities to democratize (top figure) and revert (bottom figure). The black line plots the density for democratizers and countries experiencing reversals, respectively, while the gray line plots the density for the control countries in each case, which experienced no regime change. We smooth the densities using a standard Epanechnikov kernel.