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 relies on a dichotomous measure of democracy coded from several sources to reduce measurement error and controls for country fixed effects and the rich dynamics of GDP, which otherwise confound the effect of democracy on economic growth. 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 results when we estimate the effect of democratizations on GDP year-by-year, controlling for the GDP dynamics linearly or using the estimated propensity to democratize based on past GDP dynamics. We obtain comparable estimates using regional waves of democratizations and reversals to instrument for democracy. Our results suggest that democracy increases future GDP by encouraging investment, increasing schooling, inducing economic reforms, improving public good provision, 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 and the eclipse of the Arab Spring, 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, prominent *The New York Times* columnist Tom Friedman argues:[1]

> "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,"

while Robert Barro states this even more boldly:

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

Though, as we discuss below, some recent contributions have estimated positive effects of democracy on growth, this pessimistic view of the economic implications of democracy is still widely shared. For example, Gerring et al. (2005, p. 323) conclude from their review of the academic literature until the mid-2000s 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 by estimating the effects on growth of the unprecedented spread of democracy around the world in the last 50 years.[2] The evidence from a panel of countries between 1960 and 2010 suggests that democracy does cause growth, and its effect is significant and sizable. 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. 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 higher GDP in countries with more secondary education.

Estimating the causal effect of democracy on economic growth faces several challenges. First, existing democracy indices are subject to considerable measurement error, leading to spurious changes in democracy scores without true changes in democratic institutions.

Second, democracies have a range of unobserved characteristics, also impacting their GDP. For instance, democratic and nondemocratic countries differ in many institutional, historical and

[1] New York Times, September 8, 2009. `http://www.nytimes.com/2009/09/09/opinion/09friedman.html` Accessed February 25, 2014.

[2] 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.

cultural aspects. Thus, cross-country regressions such as those in Barro (1996, 1999) are subject to a myriad of biases and are unlikely to reveal the causal effect of democracy on growth. This problem is tackled by several recent studies using differences-in-differences or panel data techniques with country fixed effects.

Third, however, as shown in Figure 1, democratizations are on average preceded by a temporary fall (dip) in GDP. This figure depicts GDP dynamics of democratizers relative to continuing nondemocracies, with year zero corresponding to the year of democratization for the country in question. Since GDP is serially correlated, the pattern in this figure implies that the failure to properly model GDP dynamics, or the propensity to democratize, will lead to biased estimates of democracy on GDP. This clear violation of the equal trends assumption underlying the difference-in-differences or panel data estimates is often overlooked in the literature.

Fourth, democratization may have different impacts on GDP over time, underscoring the need to specify and estimate a dynamic model for GDP, or use estimates that allow the effect of democracy to vary flexibly.

Fifth, even after controlling for year and country fixed effects and GDP dynamics, changes in democracy may be driven by time varying unobservables related to future economic conditions, raising obvious omitted variable bias concerns.

In this paper, we make progress in addressing all these challenges. We build on the important work by Papaioannou and Siourounis (2008) to develop a dichotomous index of democracy and rely on this measure for most of our analysis (in the Online Appendix we show robustness to many other measures). Our measure combines the information in several democracy indices to purge spurious changes in democracy scores.

We use three strategies to tackle the remaining challenges. The first is to specify and estimate a dynamic panel model for GDP including autoregressive dynamics (as well as year and country fixed effects). This approach enables us to parametrically remove both the influence of unobserved fixed country characteristics and the serially-correlated dynamics of GDP. It also generates an implied time-path of effects on GDP from a transition to democracy. We estimate this dynamic panel model using the standard within estimator as well as a variety of moment-based estimators with better asymptotic or finite-sample properties. Our central estimates using this strategy indicate that GDP per capita is approximately 20% higher in the 25 years following a permanent democratization.

Our second strategy is to adopt a "potential outcomes" framework in which democracy (corresponding to the "treatment" in our context) influences the distribution of expected GDP in all future years. In this framework, the pre-democratization GDP dip shown in Figure 1 corresponds to a problem of selection on observables (which here are the lags of GDP). We tackle this selection problem using three complementary approaches: (i) We use a linear model for potential GDP (thus

modeling counterfactual GDP in subsequent years for countries that have democratized). (ii) We model the propensity to democratize based on GDP lags and then reweight the data using the estimated propensity scores. This strategy enables us to estimate the impact of democracy on the entire path of future GDP without having to specify any model for GDP — just relying on the selection equation for democracy. (iii) We utilize a "doubly-robust " estimator (see Imbens and Wooldridge, 2009), which simultaneously estimates the counterfactual GDP of democratizers and re-weights the data using the propensity scores for democratization.

The analog of Figure 1 using the doubly-robust estimator is depicted in Figure 4 in Section 5. It shows that this strategy removes the pre-democratization dip in GDP (or put differently, ensures that democratization is conditionally uncorrelated with past GDP). We can also see from the figure that the flexibly-estimated time-path of the impact of a transition to democracy on GDP increases over time and reaches a 20-25% increase in the 25 years following a democratization.

Our third strategy relies on an instrumental-variables approach to tackle the same challenges, and particularly, the concerns of omitted variable bias. Building on the political science literature emphasizing that transitions to democracy often take place in regional waves (e.g., Huntington 1991, Markoff, 1996), we develop an instrument for democratizations. More specifically, our instrument assumes that democratization in a country spreads to other nondemocratic countries in the same region, but does not have a direct differential impact on economic growth in these countries. Based on this reasoning, we use regional democratization waves as instruments for country-level democracy while also conditioning on lagged levels of country and regional GDP, and various regional covariates that could be correlated with the onset of a democratization wave. This IV strategy leads to similar estimates of the impact of democracy on GDP — in our preferred specification about a 25% increase in the first 25 years following a democratization — though in some specifications, quantitative effects are somewhat larger than those implied by our previous strategies.

We also investigate the channels through which democracy affects 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 public goods, like schooling and healthcare, and reducing social unrest. These results are consistent with, though of course do not prove, the hypothesis that democracy enhances growth by investing in more broad-based public goods and enacting economic reforms that would otherwise be resisted by politically powerful actors. Though they do not imply that nondemocracies never invest in public goods or enact far-ranging economic reforms, these results clearly indicate that, at least in our sample, the average democracy is more likely to do so than the average nondemocracy.

At the end of the paper, we turn to the common claim that democracy becomes a particularly powerful constraint on economic growth for countries with low levels of development (e.g., Aghion,

Alesina and Trebbi, 2008). 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.

The rest of the paper is organized as follows. The next section discusses the previous theoretical and empirical literature on the relationship between democracy and growth. Section 3 describes the construction of our democracy index, and provides data sources and descriptive statistics for our sample. Section 4 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 5 develops and presents results from a semi-parametric strategy for modeling selection into democracy. Section 6 presents our IV results using regional democratization waves. Section 7 presents evidence on potential channels through which democracy affects growth. Section 8 investigates whether democracy has heterogeneous effects depending on the level of economic development and education. Section 9 concludes. We present several additional exercises in the Online Appendix accompanying this paper.

2 Literature

The link between democracy and economic development is the subject of a large literature in political science and economics. Theoretically, the relationship is ambiguous. Several social scientists have argued that democracy and capitalist growth are contradictory (Lindblom 1977, Schumpeter 1942, Wood 2007). In economics, Alesina and Rodrik (1994) and Persson and Tabellini (1994), among others, have argued that democratic redistribution (for example, from the mean to the median voter) is distortionary and will discourage economic growth. March and Olsen (1984) have emphasized the possibility of political gridlock in democracy, while Olson (1982) suggested that interest group politics in democracy can lead to stagnation, particularly after interest groups become sufficiently organized. Counterbalancing these, the literature has also pointed out several advantages of democracy. For example, democratic redistribution may take the form of education or public goods, and increase economic growth (Saint-Paul and Verdier, 1993, Benabou, 1996, Lizzeri and Persico, 2004). Democracy can also have beneficial effects on economic growth by constraining kleptocratic dictators, reducing social conflict or preventing politically powerful groups from monopolizing lucrative economic opportunities.[3] Relatedly, Acemoglu (2008) has argued that democratic institutions may create distortions due to their redistributive tendencies, but may perform better than nondemocracies (oligarchies) in the long run because they avoid the sclerotic entry

[3]This is similar to the argument in Acemoglu and Robinson (2012), though they emphasize "inclusive political institutions," which involve significantly more than democratic institutions, in particular including checks and balances and constraints on executives, legislatures and bureaucrats to ensure a broad distribution of political power in society.

barriers that these other political systems tend to erect to protect politically powerful incumbents.

There is a substantial literature in political science investigating empirical linkages between democracy and economic outcomes, part of which is summarized in Przeworski and Limongi (1993). Cross-country regression analyses, such as Helliwell (1994), Barro (1996, 1999) and Tavares and Wacziarg (2001) have produced negative, though generally inconsistent, results.

More recent work, including Rodrik and Wacziarg (2005), Persson and Tabellini (2008) and Bates, Fayad and Hoeffler (2012), have used standard panel data techniques and found generally positive effects, though Burkhart and Lewis-Beck (1994) and Tabellini and Giavazzi (2005) have estimated insignificant effects on growth using similar strategies. 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 attempt to address the endogeneity of democratizations.[4]

Our work builds on Papaioannou and Siourounis (2008), who construct a new measure of permanent democratizations, and estimate a positive effect of democratization on growth. We construct a similar measure of democratization, but with some important differences as we explain in the next section. Papaioannou and Siourounis start with a specification in which democracy affects *growth* in GDP (thus with changes regressed on levels in terms of our models), even though they do also perform robustness checks related to GDP dynamics. Their preferred specifications show positive effects of democracy on GDP growth and are thus clearly complementary to ours. Nevertheless, their work tackles neither the empirical challenges related to the endogeneity of democracy nor the systematic modeling of GDP dynamics.

Our work also builds on and complements Persson and Tabellini (2009), who exploit variation in neighbors' democracy as well (or more precisely, an inverse distance-weighted average of democracy among "neighbors," see also Ansell, 2010, and Aidt and Jensen, 2012). Persson and Tabellini estimate a model of the impact of a country's "democratic capital" on growth, and use neighbors' democracy to deal with endogenous transitions from democracy to nondemocracy. In addition to differences in question and specification, our instrumental-variables strategy differs from theirs in focusing on regional waves of democratizations and reversals in democracy, and we document below that regional waves have much greater and more robust explanatory power than variation coming from neighbors' democracy. Also related is recent independent work by Myersson (2015), which

[4] A smaller literature looks at the effects of democracy on other growth-related economic outcomes. For example, Grosjean and Senik (2011), Rode and Gwartney (2012), and Giuliano, Mishra, 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; and Reynal-Querol (2005) and Sunde and Cervellati (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 investigating the impact of democracy using within-country, intensive margin differences, see, among others, Martinez-Bravo et al. (2012), Naidu (2012), and Fujiwara (2015).

uses a variety of strategies, some similar to ours, for estimating the effect of coups on economic growth.

Another closely related literature investigates the effect of economic growth on democracy (e.g., Lipsett, 1959). We do not focus on this relationship here, except to note that Acemoglu et al. (2008, 2009) find no evidence of a causal effect from economic growth to democracy.[5]

3 Data and Descriptive Statistics

We construct an annual panel comprising 175 countries from 1960 to 2010, though not all variables are available for all observations. In order to address the issue of measurement error in democracy indices, we develop a consolidated dichotomous measure following Papaioannou and Siourounis (2008). Our index of democracy combines information from several datasets, including Freedom House and Polity IV, and only codes a country as democratic when several sources agree. The full construction of our measure is explained in detail in the Online Appendix, and we just provide an overview here. We code our dichotomous measure of democracy in country c at time t, D_{ct}, as follows. First, we code a country as democratic during a given year if: Freedom House codes it as "Free", or "Partially Free" and it receives a positive Polity IV score. If one of these two main sources is missing, we verify the country is coded as democratic by Cheibub, Ghandi and Vreeland (2010) or Boix, Miller and Rosato (2012). These two datasets extend the popular Przeworski et al. (2000) dichotomous measure of democracy. We also use these measures to code the few instances not included in the Freedom House or Polity samples. Finally, many of the democratic transitions captured by this algorithm are studied in detail by Papaioannou and Siourounis (2008), who code the exact date of permanent democratizations using historical sources. When possible, we also draw on their data to verify the date of democratization, as explained in detail in the Appendix.

In all, our democracy index is available for 183 countries. Out of 8,733 country/year observations, 3,777 are coded as democratic while 4,956 are nondemocratic. This procedure gives us 122 instances of democratization and 71 reversals, which are shown in Appendix Tables A.1 and A.2. Furthermore, Figure A.1 presents the evolution of our democracy measure, as well as five other democracy indices, for the world and separately in each one of the seven regional groupings we use in our analysis (Africa, East Asia and the Pacific, Eastern Europe and Central Asia, Western Europe and Offshoots, Latin America and the Caribbean, Middle East and North Africa, and South Asia). Unsurprisingly, our democracy measure is highly correlated with the other indices, including the two most major ones, Freedom House and Polity.

[5]See, however, Barro (2012) for a dissenting view. See also Cervellati et al. (2014) for evidence that the effect of income on democracy is heterogeneous by colonial status, with a positive effect in non-colonized countries and a negative effect in colonized countries. Finally, Bonhomme and Manresa (2014) find that the effect of income on democracy is zero or, at best, very small, even after allowing for grouped patterns of unobserved heterogeneity.

The major difference between our index of democracy and that of Papaioannou and Siourounis is that they only code *permanent* transitions to democracy. One drawback of their approach is that by only considering democratizations that are not reversed, their index encodes information on the future state of democratic institutions, exacerbating endogeneity concerns when it is included as a right-hand side variable in GDP regressions. Instead, we code all transitions to democracy and reversals (transitions to nondemocracy).[6]

Our dichotomous index measures a bundle of institutions common to most modern democracies, including free and competitive elections, institutional checks on executive power, and the inclusiveness of the political process. To a lesser extent, our measure also captures a expansion of civil rights reflected in the Freedom House's assessment. Figure A.2 in the Online Appendix shows that these components covary strongly. In particular, following a transition to democracy we observe a sharp increase in the likelihood of free and competitive elections, the introduction of some institutional constraints on the executive and an increase in the openness of the political system. These patterns imply that our estimates will capture the impact of this bundle of democratic institutions.

Our main outcome variable, log GDP per capita in 2000 constant dollars, is from the World Development Indicators, and is available (with gaps) from 1960 to 2010 for 175 countries, which make up our baseline sample. As controls or in our investigation of mechanisms in Section 7, we also use data on investment, trade (exports plus imports), secondary and primary enrollment, and infant mortality from the World Development Indicators; financial flows (net foreign assets over GDP) from Lane and Milesi-Ferretti (2007); TFP from the Penn World Tables; tax revenues from Hendrix (2010); and an average index of economic reforms coded by Giuliano, Mishra and Spilimbergo (2013), which combines indices of product market, agriculture, trade, financial system, current account and capital account reforms. Finally, we create a dichotomous measure of social unrest capturing the occurrence of riots and revolts using Banks and Wilson's (2013) Cross-National Time-Series Data Archive (CNTS).

Descriptive statistics for all variables used in the main sample are reported in Table 1 separately for democracies and nondemocracies for our sample period of 1960-2010. This table shows several well-known patterns; for example, democracies are richer and have more educated populations.

4 Dynamic Panel Estimates

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

[6]For example, we code Argentina as a democratization in 1973 and a reversal in 1975, and a democratization again in 1983, whereas Papaioannou and Siourounis code only its "permanent" transition to democracy in 1983. We code Belarus as having a brief democratic period from 1991 to 1994, whereas Papaioannou and Siourounis's measure, by construction, ignores this brief interlude of democracy.

4.1 Baseline Results

Our main linear regression model takes the form

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

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 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.

We impose the following assumption:

Assumption 1 (sequential exogeneity): $\mathbb{E}(\varepsilon_{ct}|y_{ct-1}, \ldots, y_{ct_0}, D_{ct}, \ldots, D_{ct_0}, \alpha_c, \delta_t) = 0.$

This is the standard assumption when dealing with dynamic panel models. It implies that democracy and past GDP are orthogonal to contemporaneous and future error terms, and that the error term ε_{ct} is serially uncorrelated.

Assumption 1 effectively requires sufficiently many lags of GDP to be included in equation (1) both to eliminate residual serial correlation in the error term of this equation and to remove the pre-democratization dip in GDP.

We also assume (and test below) that GDP and democracy are stationary processes (conditional on country and year fixed effects). Under Assumption 1 and stationarity, equation (1) can be estimated using the standard within estimator.[7] Columns 1-4 of Table 2 report the results of this estimation controlling for different numbers of lags on our baseline sample of 175 countries between the years of 1960 and 2010. Throughout, the reported coefficient of 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 on the right-hand side. In a pattern common with all of the results we present in this paper, there is a sizable amount of persistence in GDP, with a coefficient on lagged (log) GDP of 0.973 (standard error = 0.006).

[7] 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 but not strictly exogenous, and when both series are cointegrated (this is the case if both are stationary as assumed). Thus, for long panels, as the one we use, the within estimator provides a natural starting point.

But crucially, this coefficient continues to be significantly less than 1. The adjusted t-statistic from Levin, Lin and Chu's (2002) test for a unit root in a panel setting is also reported at the bottom.[8] In column 1, the statistic is -4.971, comfortably rejecting a unit root in the empirical process for log GDP per capita and supporting our assumption about GDP being stationary.

The democracy variable is also estimated to be positive and highly significant, with a coefficient of 0.973 (standard error = 0.294). This parameter estimate implies that in the year of a transition to democracy, GDP per capita increases by about 1%. The dynamic process for GDP specified in equation (1) fully determines the effect of a *permanent* transition to democracy. In particular, given our estimate of GDP persistence, such a permanent transition increases GDP per capita by about 1.97% one year after democratizations, by about 2.9% the year after, and so on. Iterating this calculation, the long-run effect of such a transition on GDP is

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

where a hat ("ˆ") denotes the parameter estimates. (For future reference, this formula is written for the general case with multiple lags on the right-hand side.) 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 after 25 years, which is 17.8% in this case.[9]

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, shown in the row at the bottom, is close to that found in column 1, and the Levin, Lin and Chu test again comfortably rejects a unit root. The effect of democracy is slightly lower, but still highly significant, at 0.651 (standard error =0.248).

Column 3, which is our preferred specification, includes four lags of GDP per capita. The overall pattern is very similar, with both the degree of persistence and the long-run effect being very close to the estimates in 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.

[8]The Levin, Lin and Chu (2002) test imposes the two assumptions we maintain during our estimation: that the persistence of the GDP process is the same for all countries; and the only type of cross-sectional dependence can be fully absorbed by year fixed effects. This test statistic is computed after partialling out covariates and year effects and is then adjusted to ensure an asymptotic t-distribution in the presence of a unit root. We manually compute the t-statistic for our unbalanced panel and then use the adjustment factors from Levin, Lin and Chu for the average length of our panel (38.8 years). We report this test statistic for all of our within estimates.

[9]An alternative would be to simultaneously estimate a separate democracy equation and evaluate the long-run impact of a shock to the democracy equation. Provided that the democracy equation enables some countries to be nondemocratic in the long run, this computation would imply a zero long-run effect, because any shock to the democracy equation would not change the long-run distribution of democracy for the affected country. For this reason, we find the *conditional* computation — corresponding to the impact of a *permanent* transition to democracy — more informative.

Figure 2 plots the estimated impact of a permanent transition to democracy s years after the event, with s ranging from 0 to 30, together with a 95% confidence interval. These estimates are derived (extrapolated) directly from the dynamic process for GDP estimated in our preferred specification. They imply that most of the long-run gain from democracy will have been realized and GDP will be about 20% higher in 30 years.

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 dynamics. The overall degree of persistence and the long-run effect of democracy on GDP per capita are very similar to the estimates in column 3.

The within group estimates of the dynamic panel model in columns 1-4 have an asymptotic bias of order $1/T$, which is known, after Nickell (1981), as the Nickell bias. This bias results from the failure of strict exogeneity in models with lagged dependent variables on the right-hand side as in our equation (1) (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, motivating our use of the model in columns 1-4 as the baseline.[10]

The rest of Table 2 reports various GMM estimators that deal with the Nickel bias, and produce consistent estimates of the dynamic panel model for finite T. Sequential exogeneity 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 slightly smaller persistence for the GDP process, leading to smaller long-run effects. For example, column 7, which corresponds to the GMM estimates analogous to our preferred specification in column 3, shows a long-run impact of 16.45% (standard error=8.436%) on GDP per capita following a permanent transition to democracy.

In addition, we also report the p-values of a test for serial correlation in the residuals of equation (1) which, as required by Assumption 1, tests whether there is AR2 correlation in the differenced residuals. The p-values for this test indicate that this assumption is not rejected when we include

[10] Returning to footnote 7, this bias can be understood as a consequence of the fact that for fixed T_c, the term $\frac{1}{T_c}\sum_s \varepsilon_{cs}$ in the transformed error is mechanically correlated with y_{ct-j} and D_{ct}. Clearly as T_c tends to infinity, this bias disappears as long as the process for GDP and democracy are weakly dependent over time, as we have assumed. In the text, we simplify the discussion by referring to T, the average number of times a country appears in the panel.

Judson and Owen's (1999) Monte Carlo results suggest that, in standard macro datasets, the Nickell bias is of the order of 1% for $T = 30$, further suggesting that it is not a major concern in our empirical setting.

four or more lags of GDP as in our preferred specifications in columns 7 and 8 (that this assumption is rejected with only one or two lags included 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).

One potential drawback of Arellano and Bond's GMM estimator is that the number of moment conditions is of the order of T^2. Thus, as T grows, we have a version of the "too many instruments" problem, leading to an asymptotic bias of order $1/N$ (see Arellano and Alvarez, 2003).[11] An alternative estimator is proposed by Hahn, Hausman and Kuersteiner (2002) and relies on forward orthogonal differences. Under Assumption 1 and stationarity, this estimator is asymptotically unbiased. Intuitively, Hahn et al. note that Arellano and Bond's GMM procedure corresponds to a minimum distance estimator combining $T-1$ 2SLS estimates. They propose replacing each 2SLS estimate with a Nagar-type estimator, which is robust to the use of many instruments, and combining these estimates using a minimum distance estimator.[12] We refer to this estimator as HHK 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 estimated long-run effect of a permanent transition to democracy on GDP is 25.03% (standard error=9.03%).

In the rest of the paper we focus on the specification with four lags of GDP as our benchmark. As already noted and shown in Table 2, consistent with Assumption 1, conditional on four lags there is no further serial correlation in the residuals. Moreover, Table A.3 in the Appendix shows that

[11] This is also the reason why we use Arellano and Bond's baseline *ad hoc* weighting matrix with 2's on the main diagonal and -1's on the two main subdiagonals above and below. As shown in Arellano and Alvarez (2003) and Hayakawa (2008), the estimator with the *ad hoc* weighting matrix is more reliable than the efficiently weighted GMM estimator when T is large. In particular, the estimator with the *ad hoc* weighting matrix used here remains consistent under "large N, large T" asymptotics.

[12] More specifically, Arellano and Bond's GMM estimator is a 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

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

Hahn, Hausman and Kuersteiner (2002) propose estimating the equation for time t in forward orthogonal differences using a Nagar estimator with $\{y_{cs},D_{cs}\}_{s=1}^{t-1}$ as instruments. This is given by

$$\widehat{\beta}=(X'(I-kM_{Z})X)^{-1}X'(I-kM_{Z})Y,$$

with k equal to $1+\frac{L}{N}$, and L being the degree of overidentifying restrictions and N the number of countries ($k=1$ yields the usual 2SLS estimator). Here X is the vector of the endogenous right-hand side variables, Z the instruments, Y the dependent variable, and M_{Z} denotes orthogonal projections on Z for the equation at time t. HHK show that the minimum distance estimator combining the $T-1$ Nagar estimates is asymptotically unbiased for large T. We compute standard errors using 100 bootstrap repetitions.

further GDP lags do not have significant explanatory power for current democracy. The table also shows that the predicted residuals from the estimation of equation (1), the $\widehat{\varepsilon}_{ct}$'s, are uncorrelated with lags of democracy, as also required by Assumption 1. These patterns bolster our confidence that democratizations are not taking place in anticipation of future GDP changes and that our dynamic panel data model with four lags adequately captures pre-democratization GDP dynamics.

4.2 Robustness

Table 3 probes the robustness of our results to the inclusion of various covariates. We focus on time-varying variables, since any time-invariant covariate is already absorbed by the country fixed effects.

Table 3 comprises three panels: the top one uses the within estimator, the middle one Arellano and Bond's GMM, and the bottom one the HHK estimator. To conserve space, we only report the coefficient estimates on democracy, the long-run effects and the effect after 25 years. Column 1 reproduces our baseline estimates for comparison.

In column 2, we report results from a specification in which we include a full set of interactions between a dummy for the quintile of the GDP per capita rank of the country in 1960 and a full set of year effects.[13] This specification is useful for two reasons. First, it controls for potentially time-varying effects of baseline differences across countries. Second, it only exploits differences within groups of countries with relatively similar levels of GDP per capita at the beginning of the sample. These controls have relatively little effect on our estimates. For example, the within estimate for the coefficient of democracy is 0.718 (standard error=0.249), and the long-run effect is 22.17%. These estimates are remarkably close to our baseline specification presented in column 1. Arellano and Bond's GMM and HHK estimates remain similar once these controls are included, though the effects of democracy are slightly smaller.

Column 3 adds interactions between a dummy for Soviet and Soviet satellite countries and dummies for the years 1989, 1990, 1991, and post-1992 in order to control for the major economic changes that took place in these countries following the fall of the Berlin wall. These controls have little impact on our results, and the long-run effect of democracy increases slightly to 24.86%.

Columns 4 and 5 add four lags of unrest and trade (import plus exports over GDP) as controls. These covariates control for the potential effect of unrest before democratization or for the possibility that external shocks are driving both growth and democracy. These additional controls have a limited impact on our estimates in all panels.

Column 6 includes a full set of 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

[13]To compute the GDP per capita rank in 1960 we use Angus Maddison's estimates, since the World Bank data do not contain estimates for the GDP per capita of several countries in 1960.

with the same initial regime (democracy or nondemocracy). This specification, which is motivated by the use of regional democratization waves in our IV strategy in Section 6, thus fully controls for any omitted variable at the region × initial regime level. Reassuringly, it leads to very similar estimates to our baseline results.[14]

Finally, another concern is that the estimated effect of democracy is a mere reflection of greater financial or aid flows following democratization in some countries. To alleviate this concern, column 7 controls for lags of external financial flows. Though this specification both reduces the number of observations because of missing data and creates a potential bias against finding an impact of democracy because financial flows are likely to be endogenous to democracy, we continue to find a significant, even if less precisely estimated, impact of democracy on GDP per capita using all three of our estimators.

We also report several additional robustness checks in the Appendix.

First, in Table A.4 we explore if 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 also find positive, though imprecise, estimates using a dichotomous measure based on the Polity index and Cheibub, Gandhi, and Vreeland's democracy-dictatorship measure. Importantly, the table also shows that not controlling for GDP lags leads to negative and inconsistent estimates using any of these measures (including our measure), underscoring the role of correctly specifying and estimating the GDP dynamics.

Second, in Table A.5 in the Appendix, we explore the sensitivity of our baseline results to outliers. In particular, we re-estimate our preferred specification excluding countries with a standarized residual above 1.96 or below -1.96, and also excluding those with a Cook's distance above a common rule-of-thumb threshold (four divided by the number of observations). Finally, we also report results using a robust regression estimator following Li (1985) and Huber's M-estimator. In all cases, the results, especially 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 A.6 we present several alternative GMM estimators based on different sets of moment conditions. In particular, given the possibility of finite-sample bias due to "too many instruments", we estimate models truncating the number of lags used to form moment conditions

[14]The estimates are also very similar to our baseline 2SLS results contained in Table 5 below, even though they exploit an orthogonal source of variation.

We have also explored (but do not report) several specifications motivated by the robustness checks on our IV specifications reported in Section 6, where we use regional democracy waves as instruments. In particular, we controlled 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×initial regime cells to take into account regional shocks among countries with similar political characteristics. These controls had practically no impact on our key estimates.

in Arellano and Bond's GMM estimator. We also add Ahn and Schmidt's (1995) nonlinear moment conditions to those exploited by Arellano and Bond. The estimates are again very similar to those in Table 2 and show that our results are not sensitive to the particular set of moment conditions used by the Arellano and Bond GMM estimator.

Fourth, in Table A.7, we explore separately the effect of democratizations and reversals (transitions from democracy to nondemocracy). Both democratizations and reversals in democracy yield consistent results — democratizations increase GDP and reversals reduce it. Though our estimates for reversals are less precise, our estimates do not reject the restriction that they are of equal size to the effects of a democratization.

Finally, recall that because the Levin, Lin and Chu test reported in Table 2 rejects nonstationarity of GDP, in the text we have focused on models specified in levels. Table A.13 in the Appendix shows a similar but somewhat larger effect of democracy on GDP when we instead assume that there is a unit root in GDP and estimate a specification with changes on levels. Relatedly, in Table A.12 we pursue an alternative strategy to deal with the high degree of persistence in the empirical process for GDP per capita: we impose different levels of persistence of the GDP process ranging from 0.95 to 1. More specifically, 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 the specified number, while estimating each of these coefficients to capture the dynamics of GDP. The resulting estimates show that our findings are robust to imposing plausible levels of persistence for the GDP process. Moreover, because in this procedure the left-hand side variable, $y_{ct} - \left(\sum_{j=1}^{p} \gamma_j\right) y_{ct-1}$, is clearly stationary, these results also show that unit roots or near-unit roots in the GDP play no role in our estimates or conclusions.

5 Semi-Parametric Estimates

In the previous section, we controlled for GDP dynamics using a linear dynamic panel data model. This strategy allowed us to remove the confounding influence of the pre-democratization GDP dip shown in Figure 1 and compute the over-time effects of a permanent transition to democracy. Though this approach is closely related to that used in the previous literature and enables efficient estimation under its maintained assumptions, it does make heavy use of the linearity assumption. This forces the effects of transitions to and from democracy to be the same in absolute value. Moreover, the time-path of democracy's impact on GDP shown in Figure 2 heavily relies on extrapolating the linear process for GDP into the future.

In this section we propose an alternative, semi-parametric strategy to flexibly estimate the effects of a transition to democracy (or a reversal to nondemocracy) on subsequent GDP. We next explain this approach and then present our estimates.

5.1 Potential Outcomes and Semi-Parametric Identification

Let $\Delta y_{ct}^s(d) = y_{ct}^s(d) - y_{ct-1}$ denote the "potential"change in (log) GDP per capita at time $t+s$ relative to its level at time $t-1$ for a country with a change in political regime $d \in \{0,1\}$. In particular, for a country transitioning to democracy at t, we have $d = 1$ ($D_{ct} = 1, D_{ct-1} = 0$) and for one remaining in nondemocracy, we have $d = 0$ ($D_{ct} = D_{ct-1} = 0$). With analogy to the standard potential outcomes setups, we can think of $d \in \{0,1\}$ as corresponding to a "treatment".

The causal effect of a transition to democracy on GDP s periods after it occurs for the set of treated countries (i.e., countries transitioning to democracy) is given by

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

Unlike the estimates in the previous section, these effects are defined without making any parametric assumptions on the persistence of GDP or democracy.[15]

The challenge in estimating β^s is that countries selecting into democracy may be different in terms of their potential outcomes than those remaining in nondemocracy. Our key assumption in this section is that this selection can be modeled as a function of observables (lags of GDP and time effects):

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

This assumption allows for transitions to democracy to be correlated with lags of GDP, so that, for instance, such a transition may be more likely following sharp declines in GDP. But it also imposes that there are no other confounding factors that impact the propensity to democratize and that are related to potential outcomes. It is instructive to compare this assumption to Assumption 1 used in the previous section. Both assumptions condition on lags of GDP to ensure identification and both rule out the presence of omitted variables simultaneously affecting democracy and GDP. But they incorporate the dynamics of GDP and unobserved fixed characteristics differently. Assumption 1 imposes linear dynamics conditional on fixed unobserved country characteristics (modeled as country fixed effects). Assumption 2, on the other hand, imposes that conditional on $D_{ct-1} = 0$ and lags of GDP, any unobserved heterogeneity does not affect the difference $\Delta y_{ct}^s(d) = y_{ct}^s(d) - y_{ct-1}$. This seems plausible in light of the evidence in Acemoglu et al. (2005), which shows that factors causing GDP growth do not predict democratization. The two

[15]The conditioning on $D_{ct} = 1$ highlights that we are focusing on the the average effect on the "treated," meaning on those transitioning to democracy. This focus is for simplicity as its estimation requires somewhat less restrictive assumptions, and the resulting estimates have better finite sample properties given the relatively low average probability of "treatment" (i.e., transitioning to democracy) in our sample. Figure A.4 in the Online Appendix plots the corresponding average treatment effects.

assumptions would be mutually consistent either if (i) t is large so that GDP is near its steady state and the country fixed effects, the α_c's in equation (1), do not affect $\Delta y^s_{ct}(d)$ along the transition path,[16] or more plausibly, if (ii) the time-invariant country characteristics in the GDP equation (1), modeled as country fixed effects, the α_c's, do not affect the likelihood of a democratization among countries that are currently nondemocratic.

5.2 Estimation under Selection on Observables

Given Assumption 2, there are several plausible ways of proceeding. We outline three alternatives here. The first builds on Kline (2011) and uses (parametric) regression adjustment. Specifically, it models the conditional expectation of $\Delta y^s_{ct}(0)$, which corresponds to the counterfactual outcome, as follows

$$\mathbb{E}\left(\Delta y^s_{ct}(0)|D_{ct-1}=0, y_{ct-1}, y_{ct-1}, y_{ct-2}, y_{ct-3}, y_{ct-4}, t\right) = \delta^s_t + \sum_{j=1}^{4} \gamma^s_j y_{ct-j}.$$

This equation can then be used to construct the counterfactual path of GDP without a transition to democracy. Assumption 2 implies that consistent estimates of counterfactual GDP can be obtained from the OLS estimation of

$$\Delta y^s_{ct} = X'_{ct}\pi^s + e^s_{ct} = \delta^s_t + \sum_{j=1}^{4} \gamma^s_j y_{ct-j} + e^s_{ct} \tag{3}$$

on the set of countries that remain nondemocracies (i.e., on the sample of countries with $D_{ct} = D_{ct-1} = 0$). Here X_{ct} includes all the covariates in (3), and π^s denotes the corresponding coefficient vector.

The parameter β^s can then be estimated 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 $\widehat{\pi}^s$ are the OLS estimates of the vector $\widehat{\pi}^s$ in equation (3), and $\widehat{\mathbb{E}}(X)$ denotes the sample mean of X.

Figure 3 plots the estimates $\widehat{\beta}^s$ for $s = -15, -14, \ldots, 30$, with year 0 corresponding to the year of democratization. The estimates for negative values of s are included as a specification test (since they should not be affected by a subsequent democratization). The solid line plots the estimated effects of a democratization on GDP (in log points) over time, and the dotted lines plot the 95%

[16] This requirement is a consequence of the fact that Assumption 1 models the level of GDP conditional on fixed effects, while Assumption 2 models its change. An alternative bringing the two assumptions closer would be to formulate Assumption 1 for a model in changes (with a stationary error term for this change equation). We do not do this since the version in levels is closer to the model (implicitly or explicitly) used in the literature. In any case, assuming and using the changed version of Assumption 1 would lead to very similar estimates.

confidence interval.[17] In reassuring contrast to Figure 1 in the Introduction, we see no differential behavior of GDP preceding democratizations. Thereafter, there is a gradual increase in GDP, plateauing between 20 and 25 years at about 25%.

These estimates are also presented in Panel A of Table 4. They confirm the lack of significant effects before democratization, and show an estimate of a 24% increase between 20 and 25 years (standard error= 7.7%).

Our second approach follows Angrist et al. (2013) and models explicitly the stochastic process for transitions to democracy, but remains agnostic about the functional form of the conditional expectation for GDP (and this is the reason why it is "semi-parametric"). In particular, let P_{ct} be the probability of a transition to democracy in country c at time t conditional on $D_{ct-1} = 0$, y_{ct-1}, y_{ct-2}, y_{ct-3}, y_{ct-4} and t. We refer to this probability as the *propensity score*, as is conventional in the treatment effects literature.

We estimate P_{ct} using a probit model for democratization with y_{ct-1} , y_{ct-2}, y_{ct-3}, y_{ct-4} and year fixed effects as covariates, and conditioning on $D_{ct-1} = 0$. Though there could be unobserved fixed country characteristics determining the propensity to democratize, P_{ct}, Assumption 2 imposes that these characteristics should be orthogonal to the change $\Delta y^s_{ct}(d)$. The results from this model (and other alternative specifications for the propensity to democratize) and the implied propensity scores are presented in Table A.8 and Figure A.5 in the Appendix.

Using the estimated propensity scores, $\widehat{P}_{ct}$, we estimate β^s as a weighted average of the observed growth rates given by

$$\widehat{\beta}^s = \widehat{\mathbb{E}}\left(\Delta y_{ct+j} \cdot \widehat{w}_{ct} \mid D_{ct-1} = 0\right),$$

with the weights given by

$$\widehat{w}_{ct} = \frac{1}{\widehat{\mathbb{E}}(D_{ct})}\left(1\{D_{ct} = 1\} - 1\{D_{ct} = 0\}\frac{\widehat{P}_{ct}}{1 - \widehat{P}_{ct}}\right). \tag{4}$$

This estimator corresponds to reweighting the data according to the propensity score, using the efficient weighting scheme of Hirano, Imbens and Rider's (2003). The intuitive idea is to give greater weight to observations in the control group (non-democratizers) exhibiting similar dynamics in GDP to those preceding democratization. This produces a control group comparable to the set of democratizing countries, providing a suitable counterfactual for estimating the effect of transitions to democracy on subsequent changes in GDP.

Figure 4 plots our estimates, $\widehat{\beta}^s$, in this case. The pattern is similar to that in Figure 3, with no pre-trends before democratizing and an impact plateauing at about 24% between 20 and 25

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

years later. These estimates are also presented in Panel B of Table 4, and are similar to the ones obtained in Panel A.

Our third and final approach is to combine the propensity score reweighing with the explicit model for counterfactual outcomes in equation (3), obtaining a doubly-robust estimator (see Imbens and Wooldridge, 2009). More specifically, we estimate β^s as

$$\widehat{\beta}^s = \widehat{\mathbb{E}}\left(\left(\Delta y_{ct+j} - X'_{ct}\widehat{\pi}^s\right) \cdot \widehat{w}_{ct} \mid D_{ct-1} = 0\right),$$

with the weights given by equation (4).[18] This estimator is consistent as long as either the linear model for potential outcomes in (3) or the probit model for democratizations (or both) is valid — hence its double robustness. Intuitively, this estimator partials out the influence of covariates linearly and reweights the data using the inverse propensity score to get a control group comparable to the set of democratizers.

Figure 5 and Panel C of Table 4 present our estimates from this doubly-robust approach, which are similar to those obtained from the other two strategies. Once again, there is no evidence of pre-trends and the effects plateau at about 24% between 20 and 25 years.

It is worth noting that estimates from these three approaches not only produce remarkably comparable effects at the 25 year horizon, but also that they are very similar to those we obtained from the linear models in the previous section. We view this pattern as quite reassuring since it suggests that the specific parametrization of the GDP process is not overly important for our results and that the linear model in (1) appears to be a good approximation to the potentially richer dynamics of GDP.

Figure A.3 in the Online Appendix presents analogous estimates of the effect of transitions to nondemocracy on GDP. Though these estimates are somewhat less precise, they clearly illustrate that transitions to nondemocracy produce comparable (in absolute value) effects on GDP as transitions to democracy; hence it is not only that transitions to democracy lead to higher GDP, but also that transitions away from democracy lead to lower GDP.

5.3 Two Illustrative Examples

In this 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

[18] In this case, the OLS estimates for π^s are also obtained from a weighted regression in the sample of non-democratizers, with weights given by $\sqrt{\widehat{w}_{ct}}$. These weights ensure that the distribution of covariates in the sample of non-democratizers is the same as in the sample of democratizers, improving finite sample properties of the estimator.

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 A.1 (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, 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 A.1). The dictatorship's succession announcement on June 10, 1987 triggered large student protests. Nevertheless, large and even more daring pro-democracy protests had erupted but been decisively repressed earlier in the decade, notably 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 hold 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 $5400, 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, obtain 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 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 7 below, both countries undertook important reforms after democratization. They both expanded 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 *chaebol* ownership in the early 1990s (Lee, 2005). We will examine these mechanisms systematically in Section 7.

6 IV Estimates: Democratization Waves

The estimation strategies adopted so far control for GDP dynamics and the influence of fixed unobserved characteristics. In this section, we develop an instrumental-variables (IV) strategy to deal with time-varying omitted variables which may simultaneously affect the likelihood of transitioning to democracy and subsequent GDP growth. A successful IV strategy would also alleviate concerns related to measurement error in our democracy variable and provide a different and complementary approach to the issue of endogenous selection into democracy (which our previous strategies confronted by conditioning on past GDP growth).

6.1 IV Strategy and Exclusion Restriction

As highlighted by the recent Arab Spring experience, democratizations often occur in regional waves. Many countries in Latin America and the Caribbean underwent a transition from democracy to nondemocracy in the 1970s, followed by a wave of democratizations in the 1980s and early 1990s in what Huntington (1991) dubbed the "The Third Wave" (see also Markoff, 1996). This also coincided with democratization in Eastern Europe, Central Asia and Africa in the 1990s following the fall of the Soviet Union.[19] Though there is no consensus on the factors creating such waves, the existing evidence suggests that they are not explained just by economic trends. For instance, Bonhomme and Manresa (2014) find that, after conditioning on GDP, transitions to democracy are significantly correlated within regions. The most reasonable hypothesis is that this regional pattern reflects the fact that the demand for democracy (or dissatisfaction with nondemocracy) spreads across countries within a region, which tend to have similar histories, political cultures, practical problems, and close informational ties (e.g., Ellis and Fender, 2010, Kuran, 1989, and Lohmann, 1994, for theoretical models of informational spread of political demand or protests, and Aidt and Jensen, 2012, and Buera, Monge-Naranjo and Primiceri, 2011 for empirical evidence).

Motivated by these observations, we exploit regional waves of democratization (and reversals in democracy) as a source of exogenous variation in democracy. This strategy is related to, but also very different from, Persson and Tabellini (2009), who use neighbors' (inverse distance-weighted) democracy to control for endogenous transitions while estimating the effect of a country's own democratic capital on growth. Our approach differs from theirs both because it exploits regional waves rather than regressions of an individual country's democracy on its neighbors' democracy, and because it utilizes this approach for an IV strategy.[20]

[19] Przeworski et al. (2000) challenge the existence of democratization waves, but the consensus in political science is that such waves are important (e.g., Doorenspleet, 2000, Strand et al., 2012, Brinks and Coppedge, 2006, and and Treisman, 2013). Our first-stage results confirm the importance of such waves.

[20] Regional waves are not only emphasized in classic accounts of the democratizations process as mentioned above, but appear to be more important than the spatial spread of democracy (i.e., the impact of neighbors' democracy,

We document the existence of democratization waves in Figure 6. The top panel depicts the evolution of average democracy among countries that were initially nondemocracies in each of the seven regions described in Section 3 following the first democratization in the region.[21] For comparison, we show average democracy among initial nondemocracies in the other regions (by construction, these start with a higher level of average democracy). Following the first democratization in a region, further democratizations occur more frequently than in the comparison group, illustrating the existence of waves of democratization. The bottom panel presents a similar figure for reversals, showing similar waves of reversals in democracy.

To formally investigate these patterns and estimate our first stage, we define Z_{ct} as the jackknifed average democracy in a region$\times$ initial regime cell, which leaves out the own country (country c) observation:

$$Z_{ct} = \frac{1}{N_{rinit} - 1} \sum_{c' \in r, D_{c'init} = D_{cinit}, c' \neq c} D_{c't}. \tag{5}$$

Here, r designates one of our seven regions, $D_{cinit} \in \{0, 1\}$ is a dummy variable indicating if the country was initially democratic ($D_{cinit} = 1$) or nondemocratic ($D_{cinit} = 0$) at the beginning of our sample, and N_{rinit} denotes the number of countries in that region$\times$ initial regime cell. This construction, which also conditions on the initial regime, is motivated by the fact that demands for democracy spread to nondemocracies, and conversely, discontent about democracy can only spread to democracies. We use lags of Z_{ct} as our instruments.

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

$$\begin{aligned} y_{ct} &= \beta D_{ct} + \textstyle\sum_{j=1}^{p} \gamma_j y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct} \\ D_{ct} &= \textstyle\sum_{j=1}^{q} \pi_j Z_{ct-j} + \sum_{j=1}^{p} \phi_j y_{ct-j} + \theta_c + \eta_t + \upsilon_{ct}. \end{aligned} \tag{6}$$

This is identical to our dynamic panel model above, except that democracy will be treated as endogenous, and will be instrumented with the lags of Z_{ct}.

Our key assumption in this section is:

Assumption 3 (exclusion restriction): $\mathbb{E}(\varepsilon_{ct} | y_{ct-1}, \ldots, y_{ct_0}, Z_{ct-1}, \ldots, Z_{ct_0}, \alpha_c, \delta_t) = 0.$

The economic justification for this exclusion restriction is that, conditional on lags of GDP and year and country fixed effects, regional democratization waves captured by the variable Z_{ct-j} have

or the spread of democracy mediated by geographic distance alone). In Table A.9 in the Appendix, we use the same formulation of regional waves introduced below and show that regional waves have greater and more robust explanatory power for own-country democracy than neighbors' democracy, or democracy of other countries weighted by the inverse of (geographic) distance. Moreover, supporting ideas related to the spread of democratic demands or discontent with nondemocratic regimes, we also find a major regional component to social unrest in the same table. In contrasts, GDP does not exhibit such marked patterns of geographical spillovers.

[21] Naturally, the first democratization that defines a wave is excluded from the average to avoid a mechanical finding .

no direct effect on GDP per capita. In our specification checks, we also condition on other regional economic and political trends (to ensure that regional democratization waves do not capture the effects of regionally-correlated GDP changes).

Assumption 3 differs from Assumptions 1 and 2 as it allows for time-varying unobserved country heterogeneity, but requires such heterogeneity not to be related to past regional waves of democratization. Though different from Assumptions 1 and 2, the exclusion restriction in Assumption 3 still requires the correct modeling of GDP dynamics because if GDP lags were not appropriately controlled for, the residual in the GDP equation could be correlated with regional democratization waves, and this would lead to biased estimates. For this reason, we continue to model GDP dynamics carefully and also show the robustness of our 2SLS estimates to different sets of controls.

6.2 First-Stage and 2SLS Estimates

The first-stage relationships underlying our 2SLS estimates are shown in Panel B of Table 5. Our estimates suggest democratization or reversal waves have a strong predictive power for democracy, as shown by the F-statistics for the excluded instruments, which are all above 10. In terms of time patterns, the largest impact is from the one-year lag, though the effects continue for at least three years.

Panel A of Table 5 presents our 2SLS estimates of equation (6). These estimates are consistent for large T as in the dynamic panel model presented in Section 4. 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 non-IV estimates for the dynamic panel model. The implied long-run effect of a permanent democratization on GDP per capita is similar to our non-IV model, 26.32% (standard error=17.07%), and as usual, the 25-year impact is slightly smaller.

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 the possibility that some of the potential downward biases mentioned above were important or that there was attenuation due to measurement error. The inclusion of several lags of Z_{ct} as instruments further allows us to perform a Hansen-type overidentification test, which it passes comfortably.

In column 3, we control for a full set of interactions between GDP quintiles in 1960 and year dummies. These control for common shocks related to the initial level of development of different countries, which may 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 is useful to verify that our results are not driven solely by transitions away from socialism. Both specifications lead to only modest changes in our first-stage and 2SLS estimates.

In the remaining columns, we address concerns related to omitted regionally-correlated economic variables influencing our estimates. Column 5 adopts the simplest approach and incorporates unobserved regional heterogeneity by including region-specific trends. Panel B shows that these region-specific trends have little impact on our first-stage relationships, bolstering our confidence that regional democratization waves are not just capturing other regional trends. The resulting estimates are somewhat larger in this case than before, but the implied long-run effects remain similar.

Column 6 controls 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 in other countries within the same cell. We address these concerns by controlling for average GDP and trade in each cell. Because contemporaneous values of these variables are clearly endogenous, we instrument them using four of their lags.[22] Panel B once again shows a robust and very similar first stage, and the 2SLS estimate in Panel A is now again larger than the baseline, but with only modestly greater long-run effects.

Regional correlation in noneconomic variables, such as unrest or political instability, can also invalidate our exclusion restriction if they also spread across countries. To deal with this concern, column 7 goes one step further and adds average unrest in a region$\times$ initial regime cell, instrumented using its lags, to the specification in column 6. This is a demanding specification, likely to attenuate the impact of democracy on GDP, since our results in Section 7 suggest that social unrest is endogenous to democracy. Nevertheless, the results remain similar to the baseline specification in column 2, albeit with larger standard errors.

Columns 8 and 9 develop a complementary approach and explicitly model the spatial correlation of GDP shocks. We assume that there is a spatial pattern of correlation of GDP shocks (innovations) across countries. Specifically, let ε_t denote the column vector of time t error term ε_{ct} in the GDP equation (6). We assume that ε_t satisfies the spatial auto-regressive process

$$\varepsilon_t = \lambda \mathbb{W}^d \varepsilon_t + \zeta_t, \tag{7}$$

where ζ_t is an error term that is independent across countries, and $\mathbb{W}^d$ is a matrix of inverse distances between countries (with zeros on the diagonal), mediating the spatial patterns of correlation. This specification allows a fairly flexible pattern of correlation in GDP across countries. It is important to note that this type of correlation does not absorb our regional democracy waves because GDP

[22] The results are similar if we use the lags of regional GDP and trade as controls directly rather than as instruments for contemporaneous values.

shocks are specified purely as a function of geographic distance (without reference to regions). We find it plausible that the correlation of GDP shocks depends on geographic distance, while triggers for democratizations are correlated within regions since, as discussed in footnote 20, protests and discontent with nondemocracies appear to have a marked regional element.[23]

We estimate the system in equation (6) with the error structure in equation (7) by including the spatial lags $\mathbb{W}^d y_t$ and $\mathbb{W}^d D_t$ as controls, and instrument them using their (temporal) lags. In column 8, we start by including only the spatial lags of GDP, $\mathbb{W}^d y_t$. The results in this case continue to be precisely estimated and similar to our baseline findings. In column 9, we also add $\mathbb{W}^d D_t$ as a control, once again instrumented using its lags. This leads to a slightly lower, and only marginally significant, 2SLS estimates of the effect of democracy. Overall, despite the difficulty of separately estimating the spatial GDP correlation and the effect of regional democratization waves, we interpret the results in columns 8 and 9 as supporting our exclusion restrictions.

Panel C presents corresponding estimates from the HHK estimator described in Section 4 using the same external instruments for democracy — lags of regional democratization waves.[24] This estimator is consistent for finite T as long as our exclusion restriction in Assumption 3 continue to hold.[25] The results are broadly similar to our 2SLS estimates.

Table A.10 in the Appendix reports a number of further robustness checks, focusing on the specification in column 2. Similar to our robustness checks in Section 4, we explored the sensitivity of our 2SLS results to outliers in several ways, and found that outliers have little effect on our estimates. In addition, we investigated the sensitivity of our results to different constructions of the instrument in Table A.11. For example, constructing instruments 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) lead to similar results with somewhat

[23]This presumption is supported by the first stages shown in Panel B, which indicate that the relationship between regional democratization waves and country-level transitions to democracy is essentially unaffected by the inclusion of these spatial GDP correlations.

[24]In particular, using the notation from footnote 12, 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.

[25]In particular, using the notation from footnote 12, 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 $Z_{ct-1}, \ldots, Z_{ct-q}$ as instruments. These $T-1$ estimators are consistent (even with many instruments) and are again combined with efficient weights.

larger estimates of the impact of democracy on GDP. We also constructed an alternative instrument computed as a jack-knifed average democracy in each region interacted with a full set of region× initial regime dummies. This instrument also produced similar results.

Overall, we conclude that 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, though typically somewhat larger than, our non-IV results.

7 Mechanisms

In this section we explore the mechanisms through which democracy causes economic growth. With this aim in mind, we estimate models of the form

$$m_{ct} = \beta D_{ct} + \sum_{j=1}^{p} \gamma_j y_{ct-j} + \sum_{j=1}^{p} \eta_j m_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{8}$$

where m_{ct} is one of our potential channels described below. These models are thus very similar to our main linear regression equation, (1), except that the left-hand side variable is different and we also control for lags of GDP per capita (as well as lags of the dependent variable) on the right-hand side.

We estimate (8) using the within estimator (corresponding to column 3 of Table 2), our preferred specification for the 2SLS estimator (corresponding to column 2 of Table 5, Panel A, with $p = 4$), and our preferred specification for the HHK estimator using these external instruments for democracy and lags of the dependent variable as internal instruments (corresponding to column 2 of our IV table, Table 5, Panel C). These results are presented in Table 6.

The intermediating variables we investigate in this section are (log) investment share in GDP, (log) TFP, a measure of economic reforms (corresponding to the mean index of the reforms considered in Giuliano et al., 2013, normalized between 0 and 100), (log) trade share in GDP, (log) taxes share in GDP, primary school enrollment, secondary school enrollment, log child mortality, and the social unrest variable already used above.

Though the results for some of these variables are not as clear-cut as our baseline findings for GDP per capita, there are some noteworthy patterns. We find in all specifications that democracy increases the likelihood of economic reforms, tax revenues, primary education 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 estimates for the effect of democracy on investment, openness to trade, tax revenue and primary enrollment, and negative estimates on social unrest, but these estimates are not equally precise in each panel. Finally, we find no evidence of an impact on TFP.

Overall, we take these results as suggesting that democracy might be working through a number of channels, in particular, by encouraging economic reforms, increasing investment in human capital, and raising state capacity and some aspects of public services (especially related to health) as well as reducing social unrest. Of course, our strategy does not allow us to conclusively establish that these are the most important mechanisms, but the fact that these variables increase following a democratization even controlling for lags of GDP per capita suggests they are prime candidates for the channels through which democracy might be causing higher GDP.

8 Does Democracy Need Development?

As already hinted at 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, in Richard Posner's words:[26]

> "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 argued in the wake of the Egyptian coup of 2013 that:[27]

> "It's not that Egypt doesn't have a recipe for a democratic transition. It seems to lack even the basic mental ingredients."

We next investigate this hypothesis by considering interactions between democracy and the level of economic development (as proxied by log 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 each case, and the main effect of democracy for low economic development or for low schooling countries to be negative.

The results of this exercise are presented in Table 7. We focus on the same three estimators as in Table 6 (the within estimator, the 2SLS estimator and the HHK estimator instrumenting for democracy and its interactions). The first four columns present interactions with the log of GDP per capita and the second four with the share of the population with secondary schooling. In each case, we evaluate the main effect of democracy at the bottom 25th percentile of the interaction

[26] `http://www.becker-posner-blog.com/2010/10/autocracy-democracy-and-economic-welfareposner.html`. Accessed February 4, 2014.

[27] New York Times, July 4, 2013. `http://www.nytimes.com/2013/07/05/opinion/brooks-defending-the-coup.html`.

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). For the interaction variable we use the baseline values in 1960, 1970 or 1980, or the contemporaneous value of the variable.[28]

The patterns in Table 7 are fairly clear. There is no significant interaction between democracy and the level of economic development in any of the specifications. Thus the impact of democracy does not seem to depend on the level of development. Moreover, in contrast to popular claims in the literature, democracy does not have a negative effect for countries with low levels of economic development. In fact, all of the main effects of democracy for the lowest 25th percentile country reported in columns 1-4 are positive and some are significant.

The only set of interactions that appears to be significant are those with the share of the population with secondary schooling, which are reported in columns 5-8, and indicate that the positive effects of democracy are greater for high human capital countries (though we do not find a similar pattern when we look at primary and tertiary education). Nevertheless, these interactions are quantitatively small, so that the effect of democracy is not negative even for low human capital countries.

Our strategy does not reveal what drives the interaction with secondary schooling. It may be because, as some experts believe, democracy works better with a more literate, 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 softens the distributional conflicts in society, making democracy more stable. Our preferred interpretation is the latter, partly because we do not find any evidence of significant interactions with other modernization-related variables as noted above.

9 Conclusion

Skepticism about the performance of democratic institutions is as old as democracy itself. Plato, for example, denigrated democracy as the second worst form of government after tyranny. 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* correlation between democracy and future GDP per capita. This result remains true in GMM estimates that account for any bias due

[28]For log GDP per capita, "contemporaneous" means its lagged level, since contemporary GDP appears on the right-hand side. For share of the population with secondary schooling, this means the value of this variable in the corresponding five-year interval. When we use interactions with the current value of the share of the population with secondary schooling, we also include this variable in the regression so that its possible main effect does not load onto the interaction term.

to lags dependent variables, as well as with semi-parametric estimators modeling the propensity to transition to democracy (and nondemocracy) using lagged of log GDP. Our preferred specifications imply that long-run GDP increases by about 20-25% in the 25 years following democratization.

We also document regional waves of democratization, and use this fact to generate a new instrument for democracy. We show that the probability of a country transitioning to democracy or nondemocracy is strongly correlated with the same transition recently occurring in other countries in the same region. Using this instrument, we find that democracy again increases GDP, controlling for lags of GDP and a variety of regional controls.

The channels via which democracy raises growth include greater economic reforms, greater investment in primary schooling and better health, and may also include greater investment, greater taxation and public good provision, and lower social unrest. In contrast to the equally 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 low human capital countries.

These results taken together 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 changes economic incentives and organizations, and pinpointing what aspects of democratic institutions are more important for economic success is an obvious fruitful area for future research.

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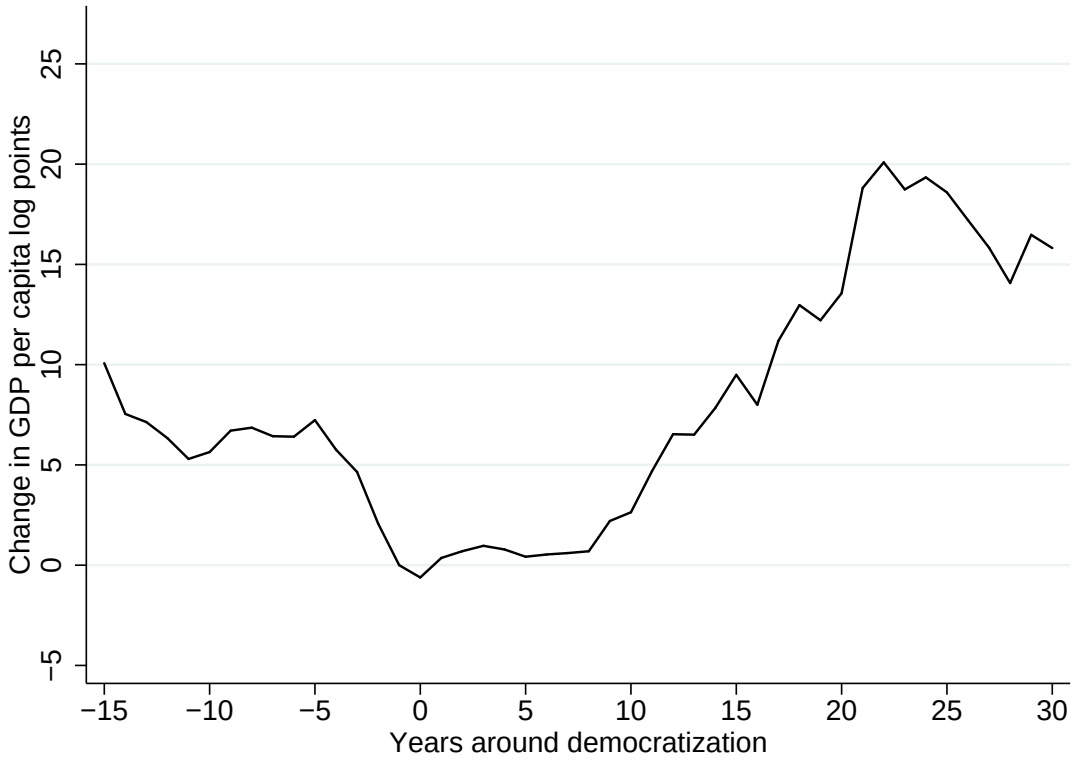
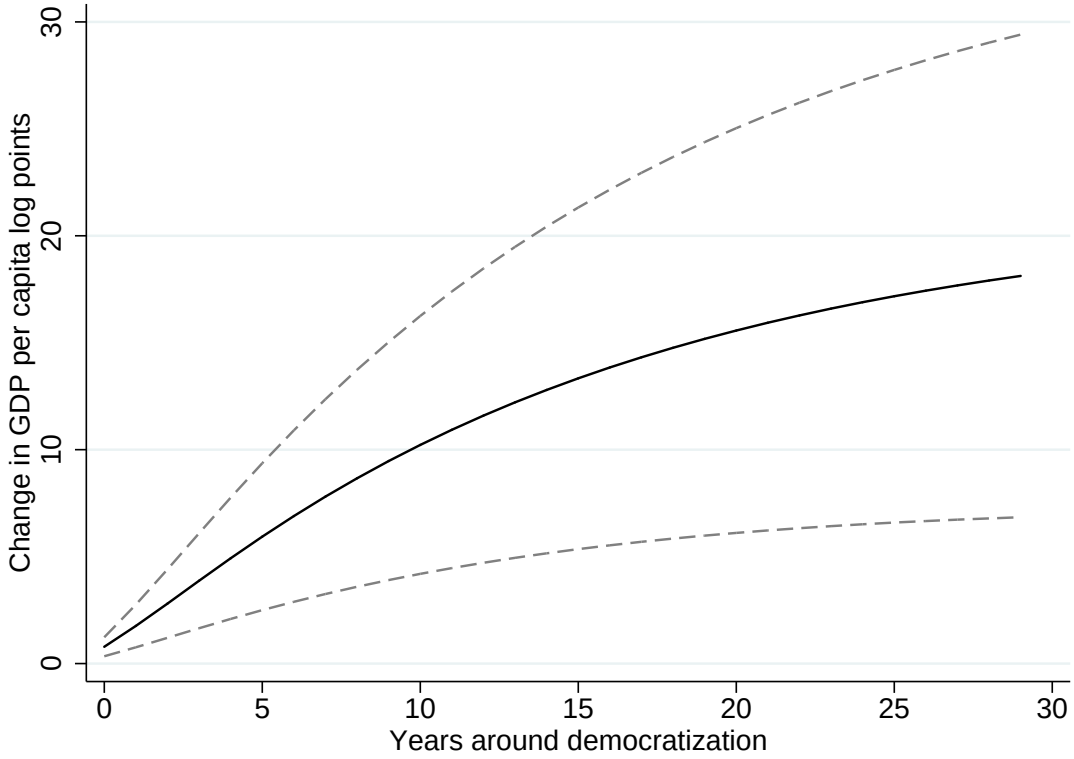
FIGURE 1: GDP PER CAPITA AROUND A DEMOCRATIZATION.



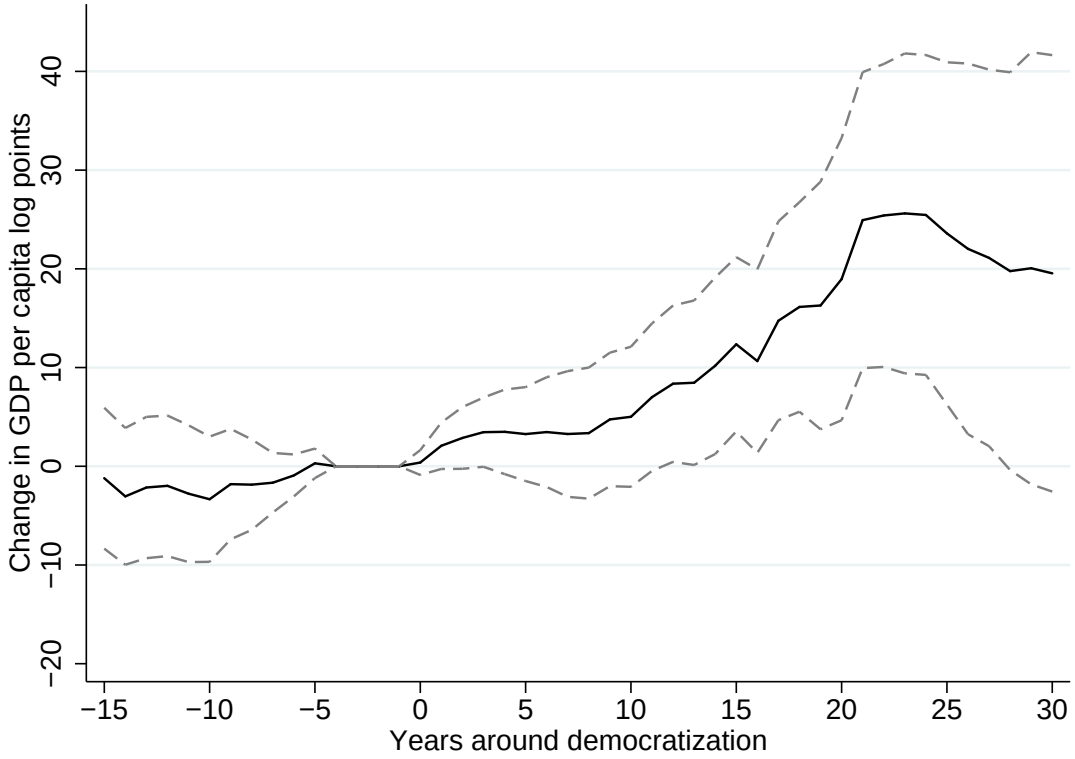
Notes: This figure plots GDP per capita in log points around a democratic transition. We normalize log GDP per capita to zero in the year preceding the democratization. Time (in years) relative to the year of democratization runs on the horizontal axis.

FIGURE 2: ESTIMATED EFFECTS OF DEMOCRACY ON THE LOG OF GDP PER CAPITA IMPLIED BY THE DYNAMIC PANEL MODEL



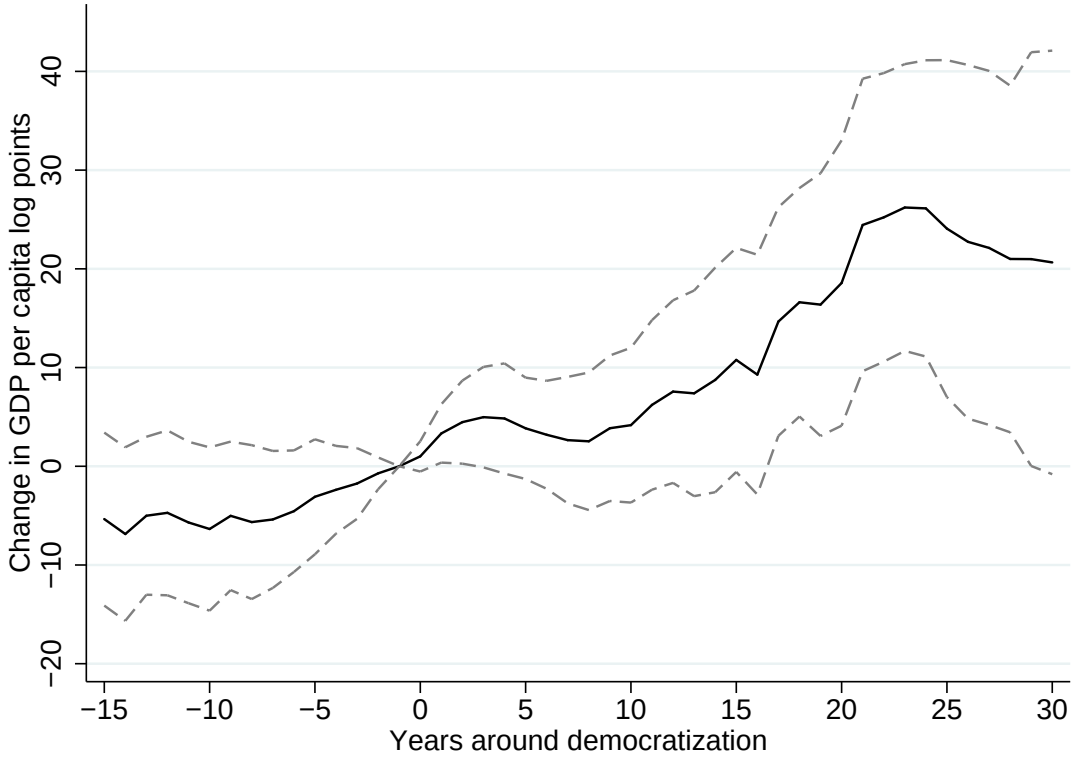
Notes: This figure plots the estimated change in the log of GDP per capita caused by a permanent transition to democracy. The effects are obtained by forward iteration of the estimated process for GDP modeled in equation (1). A 95% confidence interval obtained using the delta method is presented in dotted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

FIGURE 3: SEMI-PARAMETRIC ESTIMATES OF A TRANSITION TO DEMOCRACY ON LOG GDP PER CAPITA OVER TIME. AVERAGE EFFECT ON THE TREATED USING REGRESSION ADJUSTMENT.



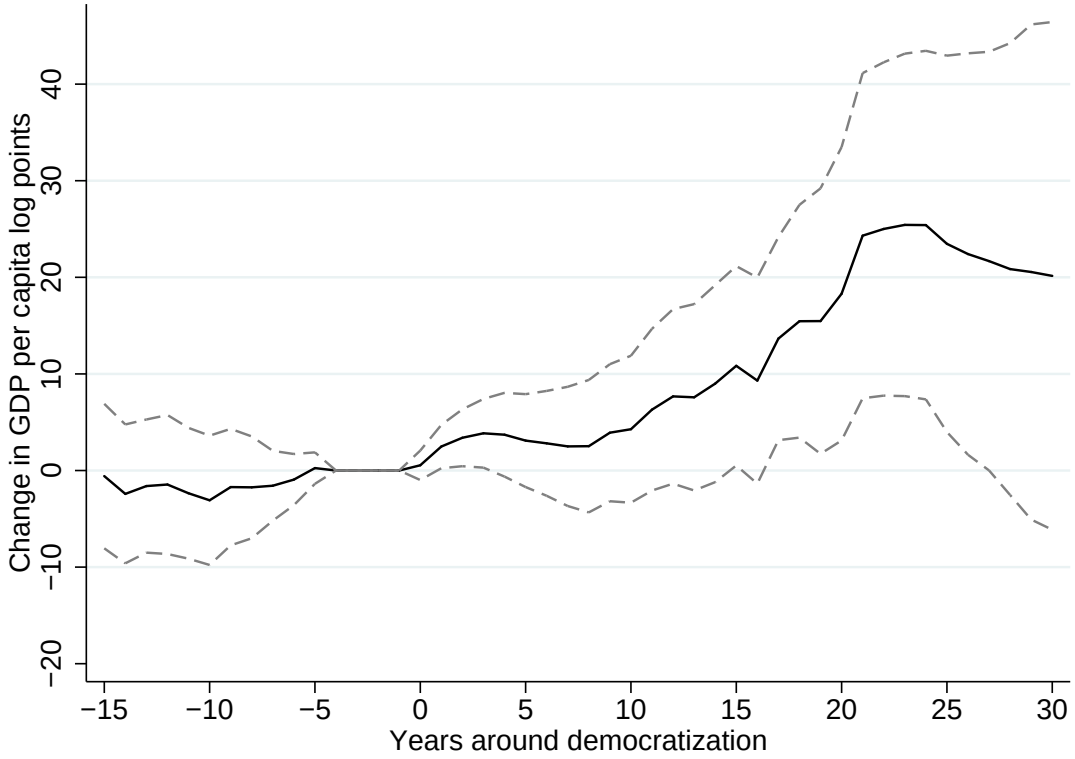
Notes: This figure plots semi-parametric estimates of the effect of democratizations on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), together with a 95% confidence interval in dashed lines. Time (in years) 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 5 explains our approach in full detail.

FIGURE 4: SEMI-PARAMETRIC ESTIMATES OF A TRANSITION TO DEMOCRACY ON LOG GDP PER CAPITA OVER TIME. AVERAGE EFFECT ON THE TREATED USING INVERSE PROPENSITY SCORE REWEIGHTING.



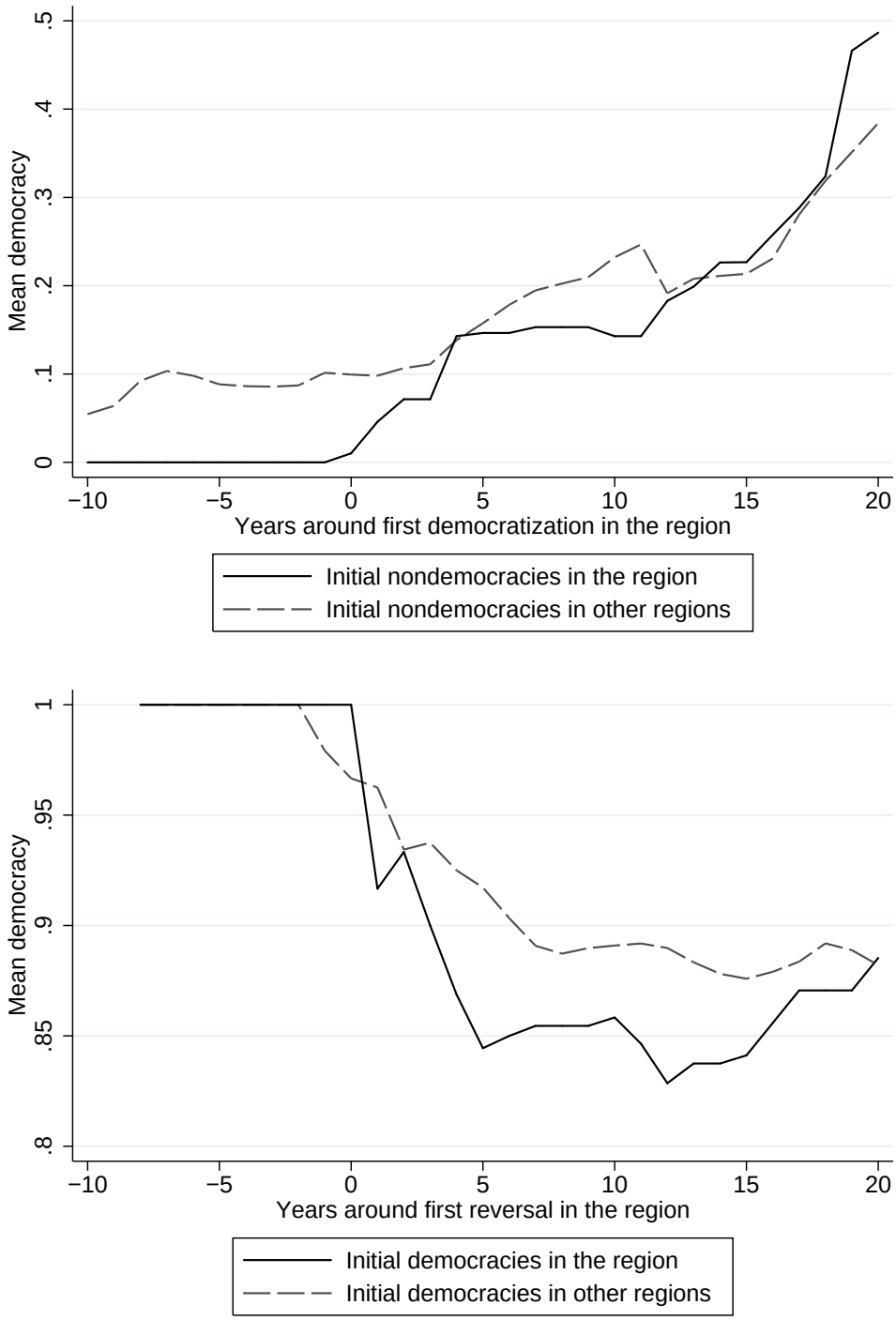
Notes: This figure plots semi-parametric estimates of the effect of democratizations on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), together with a 95% confidence interval in dashed lines. Time (in years) 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 5 explains our approach in full detail.

FIGURE 5: SEMI-PARAMETRIC ESTIMATES OF A TRANSITION TO DEMOCRACY ON LOG GDP PER CAPITA OVER TIME. AVERAGE EFFECT ON THE TREATED USING THE DOUBLY-ROBUST ESTIMATOR.



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 5 explains our approach in full detail.

FIGURE 6: DEMOCRATIZATION 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.

TABLE 1: SUMMARY STATISTICS FOR THE MAIN VARIABLES USED IN OUR ANALYSIS.

	Non-Democracies			Democracies		
Variable	*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	1.700
	(0.294)	(0.248)	(0.226)	(0.245)	(0.477)	(0.417)	(0.374)	(0.378)	(0.467)	(0.357)	(0.346)	(0.321)
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	1.156
	(0.006)	(0.038)	(0.038)	(0.039)	(0.009)	(0.041)	(0.041)	(0.038)	(0.009)	(0.046)	(0.047)	(0.038)
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)		(0.043)	(0.055)	(0.043)
log GDP third lag			-0.026	-0.021			-0.028	-0.020			-0.030	-0.041
			(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)			(0.017)	(0.028)
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	35.826
	(13.998)	(8.595)	(7.215)	(7.740)	(10.609)	(9.152)	(8.436)	(7.829)	(7.790)	(6.269)	(9.031)	(9.597)
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	30.043
	(5.649)	(5.550)	(5.297)	(5.455)	(7.281)	(7.371)	(7.128)	(6.653)	(6.086)	(5.311)	(6.814)	(6.815)
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	0.953
	(0.006)	(0.005)	(0.005)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Unit root test adjusted $t-$stat	-4.791	-3.892	-4.127	-6.991								
$p-$ value (rejects unit root)	[0.000]	[0.000]	[0.000]	[0.000]								
AR2 test p-value					0.01	0.08	0.51	0.95				
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. 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 (LOG) GDP PER CAPITA ADDING COVARIATES.

Country covariates:	(1)	GDP in 1960 quintiles × year effects (2)	Soviet dummies (3)	Lags of Unrest (4)	Lags of Trade (5)	Region × regime × year effects (6)	Lags of financial flows (7)
	Panel A: Within estimates.						
Democracy	0.787	0.718	0.911	0.705	0.595	0.834	0.926
	(0.226)	(0.249)	(0.251)	(0.224)	(0.264)	(0.264)	(0.244)
Long-run effect of democracy	21.240	22.173	24.860	17.000	14.593	16.651	23.870
	(7.215)	(8.702)	(7.783)	(5.980)	(7.122)	(5.546)	(8.211)
Effect of democracy after 25 years	16.895	16.261	19.587	13.567	11.500	14.532	18.149
	(5.297)	(5.982)	(5.724)	(4.644)	(5.336)	(4.726)	(5.435)
Persistence of GDP process	0.963	0.968	0.963	0.959	0.959	0.950	0.961
	(0.005)	(0.005)	(0.005)	(0.004)	(0.006)	(0.005)	(0.006)
Unit root test adjusted $t-$stat	-4.127	-5.075	-3.643	-4.847	-4.043	-4.092	-2.513
$p-$ value (rejects unit root)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.006]
Observations	6,336	5,523	6,336	5,643	5,750	6,336	4,950
Countries in sample	175	149	175	171	172	175	171
	Panel B: Arellano and Bond estimates.						
Democracy	0.875	0.730	1.073	0.693	1.034	1.217	1.017
	(0.374)	(0.387)	(0.403)	(0.396)	(0.469)	(0.420)	(0.373)
Long-run effect of democracy	16.448	14.865	20.006	9.871	17.926	18.209	18.607
	(8.436)	(8.998)	(8.981)	(6.479)	(9.021)	(6.746)	(7.842)
Effect of democracy after 25 years	14.713	12.759	17.874	9.159	15.659	16.861	15.903
	(7.128)	(7.350)	(7.564)	(5.768)	(7.593)	(6.050)	(6.327)
Persistence of GDP process	0.947	0.951	0.946	0.930	0.942	0.933	0.945
	(0.009)	(0.008)	(0.009)	(0.012)	(0.009)	(0.010)	(0.007)
AR2 test p-value	0.51	0.90	0.28	0.62	0.72	0.70	0.34
Observations	6,161	5,374	6,161	5,467	5,570	6,161	4,779
Countries in sample	175	149	175	171	172	175	171
	Panel C: HHK estimates.						
Democracy	1.178	0.722	1.059	1.198	1.117	1.404	2.012
	(0.346)	(0.369)	(0.355)	(0.373)	(0.342)	(0.455)	(0.325)
Long-run effect of democracy	25.032	15.731	21.648	25.530	23.923	27.291	32.811
	(9.031)	(8.678)	(8.299)	(9.897)	(8.786)	(9.792)	(7.037)
Effect of democracy after 25 years	20.853	12.719	18.313	20.696	19.148	23.215	28.931
	(6.814)	(6.689)	(6.556)	(6.997)	(6.329)	(7.683)	(5.489)
Persistence of GDP process	0.953	0.954	0.951	0.953	0.953	0.949	0.939
	(0.008)	(0.007)	(0.008)	(0.007)	(0.008)	(0.009)	(0.007)
Observations	6,161	5,374	6,161	5,467	5,570	6,161	4,779
Countries in sample	175	149	175	171	172	175	171

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. 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 4: SEMI-PARAMETRIC ESTIMATES OF THE EFFECT OF DEMOCRATIZATIONS ON (LOG) GDP PER CAPITA OVER TIME.

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
	(0.152)	(1.383)	(3.023)	(3.959)	(4.864)	(7.699)	(9.771)
	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
	(1.552)	(1.823)	(3.055)	(4.720)	(5.673)	(7.356)	(9.055)
	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
	(0.166)	(1.390)	(3.043)	(4.489)	(5.539)	(8.564)	(11.147)

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 5: INSTRUMENTAL VARIABLES ESTIMATES OF THE EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA.

Covariates:	(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 DGP 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	0.989
	(0.558)	(0.554)	(0.689)	(0.651)	(0.885)	(0.663)	(0.656)	(0.536)	(0.537)
Long-run effect of democracy	26.315	31.521	35.226	35.723	36.788	41.544	25.016	37.482	27.952
	(17.075)	(17.425)	(23.846)	(19.997)	(20.657)	(17.157)	(16.002)	(17.836)	(16.966)
Effect of democracy after 25 years	20.836	24.866	25.618	27.929	32.051	35.350	21.386	29.217	21.721
	(12.862)	(12.978)	(16.538)	(14.944)	(17.703)	(14.017)	(13.342)	(12.894)	(12.565)
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)	(0.005)
Hansen p-value		0.21	0.18	0.32	0.28	0.25	0.09	0.04	0.05
Observations	6,312	6,309	5,496	6,309	6,309	6,309	6,309	6,181	6,181
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	12.7
	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	0.595
	(0.073)	(0.101)	(0.130)	(0.099)	(0.092)	(0.104)	(0.102)	(0.103)	(0.096)
Democracy wave t-2		0.133	0.109	0.133	0.129	0.117	0.115	0.136	0.183
		(0.081)	(0.094)	(0.080)	(0.081)	(0.079)	(0.078)	(0.078)	(0.083)
Democracy wave t-3		0.227	0.270	0.223	0.228	0.221	0.223	0.224	0.236
		(0.067)	(0.077)	(0.065)	(0.070)	(0.069)	(0.070)	(0.070)	(0.076)
Democracy wave t-4		-0.087	-0.119	-0.075	-0.123	-0.083	-0.064	-0.072	-0.124
		(0.110)	(0.126)	(0.110)	(0.106)	(0.113)	(0.113)	(0.113)	(0.112)
	Panel C: HHK estimates.								
Democracy	0.690	0.948	1.435	0.722	0.822	1.311	0.845	1.034	0.817
	(0.671)	(0.513)	(0.569)	(0.549)	(0.526)	(0.444)	(0.376)	(0.533)	(0.520)
Long-run effect of democracy	14.503	25.004	46.767	18.515	16.432	24.042	15.978	30.570	26.781
	(15.076)	(15.641)	(24.225)	(15.120)	(11.879)	(9.598)	(7.905)	(18.560)	(19.209)
Effect of democracy after 25 years	11.761	18.807	31.039	14.074	13.782	21.101	13.639	21.758	17.619
	(11.859)	(10.855)	(13.175)	(11.033)	(9.475)	(7.846)	(6.415)	(11.824)	(11.531)
Persistence of GDP process	0.952	0.962	0.969	0.961	0.950	0.945	0.947	0.966	0.970
	(0.010)	(0.008)	(0.009)	(0.007)	(0.010)	(0.009)	(0.009)	(0.007)	(0.007)
Observations	6,161	6,161	5,374	6,161	6,161	6,161	6,161	6,132	6,132
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 6: EFFECTS OF DEMOCRACY ON POTENTIAL MECHANISMS.

	Dependent variable:								
	log of investment share in GDP (1)	log of TFP (2)	Index of economic reforms (3)	log of trade share in GDP (4)	log of tax revenue share in GDP (5)	log of primary enrollment (6)	log of secondary enrollment (7)	log of child mortality (8)	Dummy for unrest (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	6.602	0.394	1.120	1.253	4.335	1.369	2.150	-0.306	-3.590
	(1.250)	(0.342)	(0.399)	(0.799)	(1.915)	(0.344)	(0.596)	(0.070)	(2.916)
Long-run effect of democracy	25.466	7.625	22.657	10.158	22.391	41.081	45.134	-54.800	-5.665
	(5.396)	(6.637)	(10.267)	(6.306)	(9.766)	(13.948)	(16.809)	(17.163)	(4.621)
Effect of democracy after 25 years	25.403	6.847	15.693	9.784	21.940	28.828	37.870	-29.148	-5.665
	(5.370)	(6.057)	(5.719)	(6.103)	(9.559)	(7.849)	(11.654)	(6.432)	(4.621)
Persistence of outcome process	0.741	0.948	0.951	0.877	0.806	0.967	0.952	0.994	0.366
	(0.019)	(0.008)	(0.019)	(0.013)	(0.029)	(0.008)	(0.014)	(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 7: HETEROGENEOUS EFFECTS OF DEMOCRACY ON (LOG) GDP PER CAPITA.

Interaction with:	log GDP per capita:				Share with secondary:			
Measured at:	1960	1970	1980	Current	1960	1970	1980	Current
	(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	0.240	0.237	0.144	1.621	1.132	0.875	0.808	1.715
	(0.427)	(0.427)	(0.404)	(0.520)	(0.580)	(0.567)	(0.565)	(0.507)
Interaction	0.003	-0.000	0.001	0.002	0.087	0.089	0.056	0.016
	(0.003)	(0.003)	(0.003)	(0.004)	(0.041)	(0.035)	(0.027)	(0.011)
Long-run effect of democracy	8.363	7.505	4.480	48.611	34.597	25.456	25.055	50.100
	(15.772)	(13.909)	(12.784)	(23.499)	(19.773)	(17.799)	(19.160)	(21.894)
Effect of democracy after 25 years	5.283	5.134	3.054	34.409	25.223	19.184	18.276	36.361
	(9.583)	(9.340)	(8.581)	(13.221)	(13.129)	(12.586)	(12.989)	(12.511)
Persistence of GDP process	0.971	0.968	0.968	0.967	0.967	0.966	0.968	0.966
	(0.007)	(0.009)	(0.010)	(0.008)	(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.

ONLINE APPENDIX FOR "DEMOCRACY DOES CAUSE GROWTH"

A.1 Details for our Democracy Measure

A.1.1 Construction of our Democracy Measure

We construct our consolidated democracy measure using Freedom House and Polity IV as our main sources. We also use secondary sources to resolve ambiguous cases or those without data coverage in Freedom House or Polity IV (this is particularly important for years before 1972 when only the Polity dataset is available, and for small countries that are in the Freedom House but not in the Polity sample). The secondary sources are the dichotomous measures by Cheibub, Gandhi, and Vreeland's (2010) — henceforth CGV — and Boix-Miller-Rosato's (2012) — henceforth BMR.[29] Both measures extend and refine Przeworski et al.'s (2000) work. Finally, we use Papaioannou and Siourounis's (2008) — henceforth PS — data, which contains the exact year of permanent transition to democracy. At least one of the above sources is available for 183 countries. Our dichotomous democracy index is available for these countries, and covers their post-independence period since 1960 and until 2010.

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

1. We code a country/year observation as democratic ($D_{ct} = 1$) if its Freedom House status is "Free" or "Partially Free" and its Polity score is positive. This constitutes the bulk of the variation in our democracy measure.[30]

2. For small countries which only appear in the Freedom House sample, we code an observation as democratic if its Freedom House status is "Free" or "Partially Free," and either CGV or BMR code it as democratic. There is overwhelming agreement between Freedom House, CGV and BMR in all such cases, making our coding straightforward.[31]

[29] 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.

[30] 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.

[31] 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

3. There is no information from Freedom House before 1972. For these years, we code a country as democractic if it has a positive Polity score and either CGV or BMR code it as democratic.[32]

4. 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 manually.[33]

6. We check that our coding scheme does not produce spurious transitions when countries enter or leave the Freedom House, Polity, or our secondary sources' samples.[34]

7. Finally, we perform an additional refinements of our measure and adjust it to match the transition dates coded by PS for permanent democratizations. These dates are available for 68 transitions in our sample (recall PS only code permanent transitions), and are based on historical sources.[35]

to 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 just code a continuation of the regime in place since 2008.

[32]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.

[33]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 and is coded 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 Polity's 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.

[34]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.

[35]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 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 resulting democracy index covers the post-independence period for 183 countries from 1960 to 2010. Out of the 8,733 country/year observations, we code 3,777 instances of democracy and 4,956 instances of non-democracy. 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 A.1 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 A.1 and A.2, we list all democratizations and reversals in our sample. We also present the estimated propensity scores from our semi-parametric analysis in Section 5 and explained in detail in Section A.3 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.

A.1.2 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.[36]

Table A.4 presents our results. Panel A shows within country estimates of our baseline dynamic panel model with four lags of GDP. We arrange the results using each democracy index in a different column (as indicated in the top row), and the dependent variable is always the log of GDP. Panel B presents 2SLS estimates using the specification in column 2, Panel A, Table 5. 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 larger than their OLS counterparts (except for our measure and PS). This is consistent with our view that these alternative measures are more heavily affected by

[36]The dichotomous version of Freedom House is obtained by coding as democratic countries that are "Free" or "Partially Free". For Polity, we code as democratic countries with a positive score. 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 Satellites).

measurement error than our consolidated measure. Overall, we take these results as suggesting that our estimates produce positive coefficients of democracy on GDP consistently for most measures of democracy, but the results relying on our consolidated measure purging measurement error suffer less from attenuation.

Panel C present traditional differences-in-differences estimates of democracy on GDP levels, without controlling for GDP dynamics. This is the approach underlying previous analysis in the literature. In all these cases, independently of the measure used, our estimates for democracy are negative and imprecise. As discussed in the Introduction, this underscores the need to control for GDP dynamics in order to obtain consistent estimates of the effect of democracy on growth. The failure to adequately control for GDP dynamics on the relationship between democracy and economic growth explains, at least in part, the difference between our positive findings and previous results in the literature.

A.1.3 Components of Democracy

In this subsection, we explore the institutional variation captured by our democracy measure. All the indices define (and attempt to measure) democracy as an institutional arrangement encompassing several components. These include free elections, the existence of institutional checks on the executive, inclusive participation and representation (in particular, non-ruling parties are organized and compete for political influence regularly), and to a lesser extent civil rights. These basic components are featured in most definitions of a modern democracy, and constitute the institutional variation captured by our measure.

Consistent with this definition, we find that our democracy measure is highly correlated with measures of all of these components. Figure A.2 illustrates this by plotting the behavior of the above components around a democratic transition in our data,[37] and shows that transitions to democracy are characterized by an improvement of all these basic components of democracy (the gap closes over time as other countries democratize or countries revert to nondemocracy). These patterns suggest that transitions to democracy in our sample typically entail a similar set of institutional changes, and that our estimates capture the effect of this common institutional bundle on growth.

[37]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.

A.2 Additional Tests and Checks for our Dynamic Panel Model Estimates

A.2.1 Specification Tests

As a first check on our preferred dynamic panel specification, we estimate models with democracy as dependent variable on different lags of GDP per capita as explanatory variables. These models test whether, once we control for four lags of GDP per capita as well as country and year fixed effects, democracy is (conditionally) uncorrelated with past GDP dynamics. The top panel of Table A.3 presents our results. In column 1 we only include four lags of GDP. As anticipated by Figure 1, these four lags are jointly significant. In particular, this specification predicts that democratizations are particularly likely to occur following temporary declines in GDP, while permanent increases in GDP (captured by the sum of the GDP lags' coefficients) do not increase the likelihood of democracy.[38]

Column 2 to 5 add deeper lags of GDP up to a total of 20 lags. As shown by the p-values reported at the bottom rows, the first four lags of GDP are strong joint predictors of contemporary democracy. Deeper lags are only marginally significant in one specification, suggesting that a specification with only four lags of GDP is enough to capture the dip in GDP before a democratization.

As an additional check on our specification, we test if the estimated error term $\widehat{\varepsilon}_{ct}$ is uncorrelated with lags of democracy. This is a direct implication of Assumption 1, so this corresponds to a test of overidentifying restrictions. The bottom panel of Table A.3 presents our results. In column 1, we find that lagged democracy does not predict future GDP residuals. Columns 2 to 4 show the same pattern for lags 2-4 of democracy. Finally, column 5 shows that all these lags of democracy do not jointly predict future GDP residuals. This suggests, but of course does not prove, that democratizations are not driven by expectations about future GDP growth or anticipation of a more favorable growth profile.

A.2.2 Robustness to Outliers

We investigate the robustness of our baseline within estimates to outliers in Table A.5. 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 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 A.5 show that our within estimates are not driven by outliers.

[38] This is in line with the findings in Acemoglu, et al. (2005) and Brückner and Ciccone (2011) that temporal negative income shocks tend to increase the likelihood of democracy. This is also consistent with the probit model estimates presented in Section A.3 of this Online Appendix.

Remarkably, the long-run effect of democracy remains broadly unchanged from our preferred specification in Column 1.

A.2.3 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 A.6.

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. 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, 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. 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 these additional Ahn and Schmidt moment conditions to the moment conditions exploited in columns 2, 3 and 4, respectively.[39] 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.

A.2.4 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 course of opposite sign). Here, we relax this restriction and

[39] 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.

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

To do so, we define

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

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:

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

Equation (1) now corresponds to the special case of this equation where $\beta = \beta^d = -\beta^r$ — so that democratizations and reversals have opposite effects on GDP of equal magnitudes. However, this model still imposes some structure as democratizations and reversals are assumed to have additive and separable effects over time.

Table A.7 presents estimates of this model. Column 1 presents within country estimates of equation (A.1), controlling for four lags of GDP. Column 2 add up to eight lags of GDP to allow for the possibility that reversals may follow after more permanent declines in GDP. Columns 3 and 4 present results from the Arellano and Bond GMM estimator, 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 the estimates in this case are less precise, 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 A.3 of this Online Appendix, also suggest that it is democracy, and not transitions to a new political regime, that impacts GDP.

A.3 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 set of countries affected by the transition in question (in this case transitioning to nondemocracy), and present results using the three estimation approaches outlined in the text. Figure A.3 presents our semi-parametric estimates for the effect of reversals on GDP. 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 the event.

Figure A.4 presents our semi-parametric estimates for democratizations, but focusing on our doubly-robust estimator for the average treatment effect (ATE). As already noted in the main text, because the computation of the ATE requires a stronger form of the overlap assumption and necessitate precise estimates of potential outcomes for the treated, these estimates are less precise and exhibit poor finite sample behavior. This motivates our focus on average effects on the treated (democratizers) in the text, and also forces us to compute the ATE only for the first 20 years following a democratization (where we have greater precision). Finally, 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. With all of these caveats, the estimated ATE exhibits a similar pattern to the one reported for the average effect on democratizers, 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

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

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 left panel (columns 1-5) of Table A.8.

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 1.84 percentage points. In contrast, a permanent increase in GDP does not raise 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 baseline 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 have impact on 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. Thus, our baseline estimates for the propensity to democratize remain consistent and converge at usual rates even if GDP were close to nonstationary.

We also present several estimates of the propensity to revert to non-democracy in columns 6-10 of Table A.8. 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, they may have poor finite sample properties if GDP dynamics have a very high degree of persistence (near a unit root).

Figure A.5 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.

A.4 Additional Checks and Material for our IV Estimates

A.4.1 Role of Regional Diffusion Patterns in Democracy and Political Discontent

In this subsection we explore if indeed democracy spreads more strongly within region× initial regime cells — as exploited in our IV strategy — or if it spreads more easily or rapidly to neighbors or countries depending on their distance, as many economic shocks potentially do.

The top panel of Table A.9 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× initial regime cell (lagged regional democracy for simplicity), average democracy in other countries weighted 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 not significant 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 on 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 instrument. Moreover, we do not find such strong correlation in GDP within region × initial regime cells, suggesting (but not proving) 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.

A.4.2 Robustness to Outliers (IV Estimates)

We now explore the robustness of our IV estimates to outliers in Table A.10. We focus on our preferred IV specification presented in column 2, Panel A of Table 5. 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 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 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), 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.

A.4.3 Alternative Construction of Regional Instruments

In this section, we show that our 2SLS results are robust to different constructions of the democratization waves' instruments.

Our baseline instrument is constructed by defining D_{cinit} as 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_{cinit}. Columns 1-4 of Table A.11 present the results.

In the first column, we code $D_{cinit} = 1$ if a country is democratic from 1960-1964. In this coding, non-independent countries are coded as nondemocracies $D_{cinit} = 0$. 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 $D_{cinit} = 1$ 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, increasing 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 more complex definitions 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 (5), using this alternative region$\times$ initial regime classification (in this case we have 34 region/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 (5). In particular, we construct a set of instruments of the form

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

so that the number of instruments equals the number of region× initial regime cells. The motivation for this construction is that regional democracy waves may have a differential effect on each region× initial regime cell.

Columns 5-8 of Table A.11 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 regional democratization and reversal waves instrument.

A.5 Unit Roots and GDP Persistence

We have so far assumed that GDP is stationary and have used the Levin, Lin and Chu test for the presence of unit root in panel data settings to verify this conclusion. In this part of the Online Appendix, we provide further evidence that the assumption of stationarity is not playing an important role in our results. First, we present models where we restrict the persistence of the GDP process to be high and close to 1, and examine the robustness of our findings in such setting. Second, we explicitly depart from stationarity and present estimates for a model of changes of GDP on the level of democracy, similar to some models estimated in previous contributions to the literature (e.g., Papaioannou and Siourounis, 2008)

A.5.1 Estimates Imposing Different Levels of Persistence of the GDP Process

We investigate the robustness of our baseline results to imposing different levels of persistence of the GDP process, specially near 1. 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 in the case that we are under-estimating the persistence of the GDP process (e.g., our within estimates may understate persistence because of the traditional Nickel bias).

To do so, we rearrange equation (1) as

$$y_{ct} - \rho y_{ct-1} = \beta D_{ct} + \sum_{j=1}^{p-1} \eta_j (y_{ct-j} - y_{ct-j-1}) + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{A.2}$$

where $\rho = \sum_{j=1}^{p} \gamma_j$ is the level of persistence of the GDP process. In our baseline specifications in Table 2, we estimate persistence levels of around 0.95-0.96. We now estimate equation (A.2) imposing different values of ρ in the range 0.95 to 1, where $\rho = 1$ corresponds to the extreme case in which the GDP process is not stationary.

Table A.12 presents our within estimates (Panel A) and 2SLS estimates (Panel B) obtained with 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. Thus, this model has the additional advantage that all these terms are clearly far from exhibiting a near-unit root behavior. 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.

A.5.2 The Effect of Democracy on GDP under Nonstationarity

We next show the robustness of our results to explicitly allowing GDP to follow a unit root process. In particular, we estimate a version of equation (1) in changes:

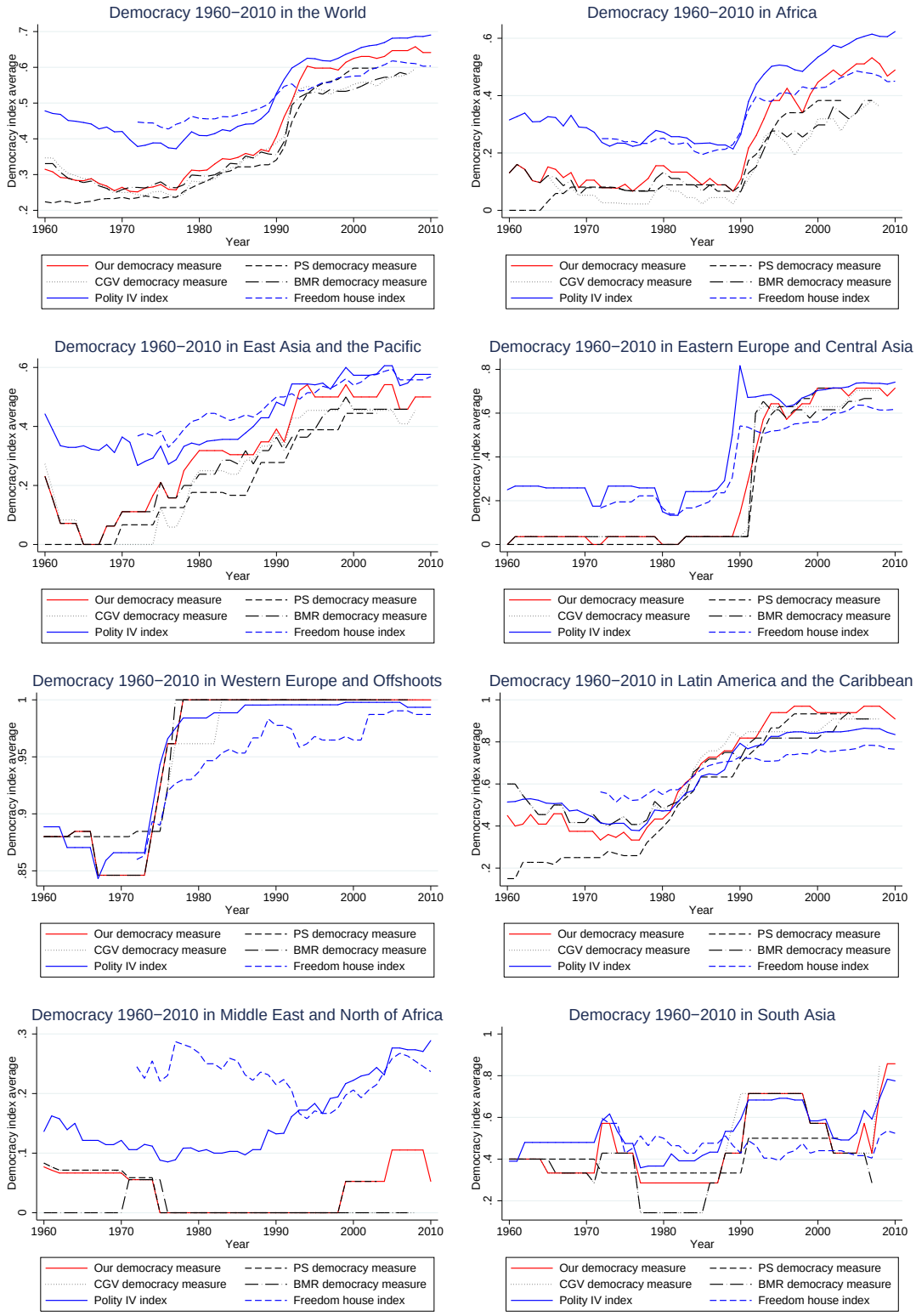
$$\Delta y_{ct} = \beta D_{ct} + \sum_{j=1}^{p} \gamma_j \Delta y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}.$$

Table A.13 presents the estimates for this model. We also compute the effects on the level of GDP 25 years after a democratization for comparison (since with a unit root process there are no meaningful long-run effects). Though these quantitative magnitudes are now larger than our preferred estimates in the text, they remain plausible.

A.6 Appendix: Additional Heterogeneous Effects

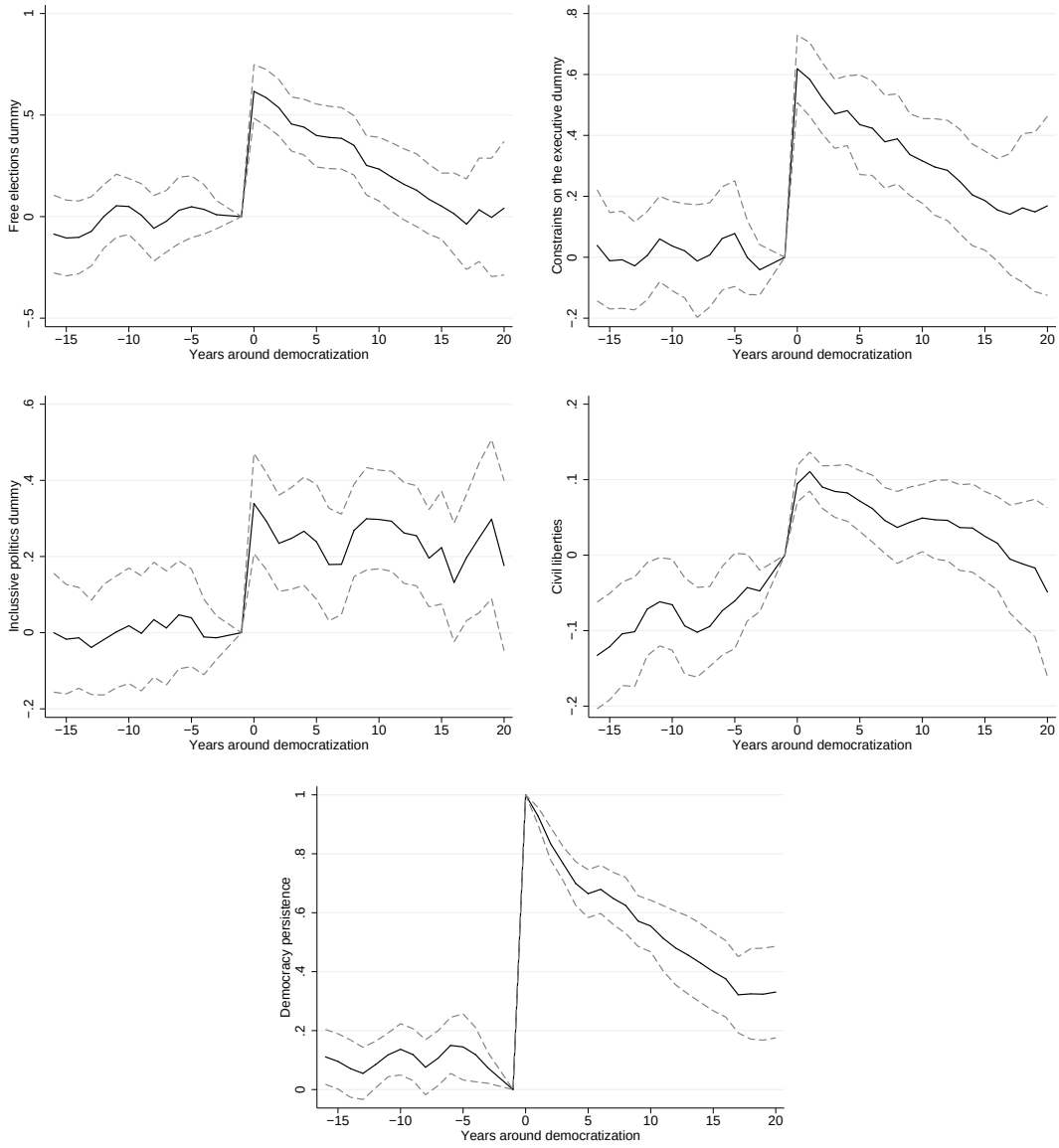
Table A.14 presents within estimates interacting democracy with other measures of education. Columns 1-4 focus on the share of the population with primary education from the Barro-Lee dataset, while columns 5-8 present results using the share with tertiary education. We do not find evidence of a consistent interaction between democracy and these alternative measures of education.

FIGURE A.1: DEMOCRACY MEASURES ACROSS REGIONS OVER TIME.



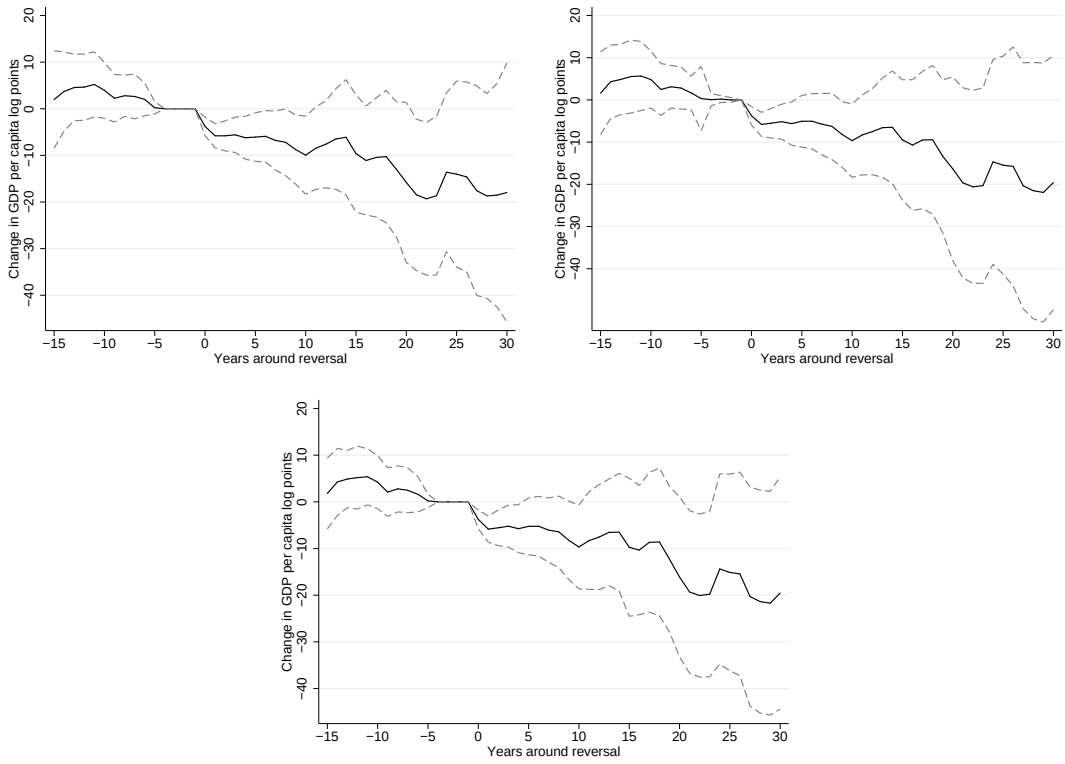
Notes: The figures present the evolution over time of the average of our democracy measure for each of the seven regions used in the paper, as well as for the whole World. For comparison we also plot the average Polity IV score and Freedom House index (both normalized between 0 and 1), Papaioannou and Siourounis (2008), Cheibub, Gandhi, and Vreeland (2010) and Boix, Miller and Rosato (2012) democracy measures.

FIGURE A.2: COMPONENTS OF DEMOCRACY FOLLOWING A DEMOCRATIZATION EPISODE.



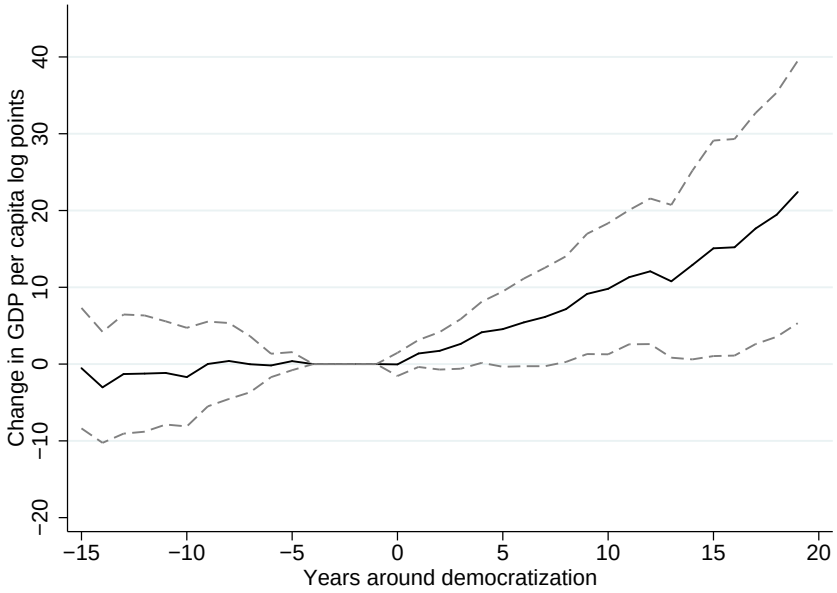
Notes: The figures plots the behavior of different component of democracy around a democratization (relative to continuing nondemocracies). Time (in years) relative to the year of democratization runs on the horizontal axis. See the text for a detailed explanation of how we measure these components separately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

FIGURE A.3: SEMI-PARAMETRIC ESTIMATES FOR REVERSALS



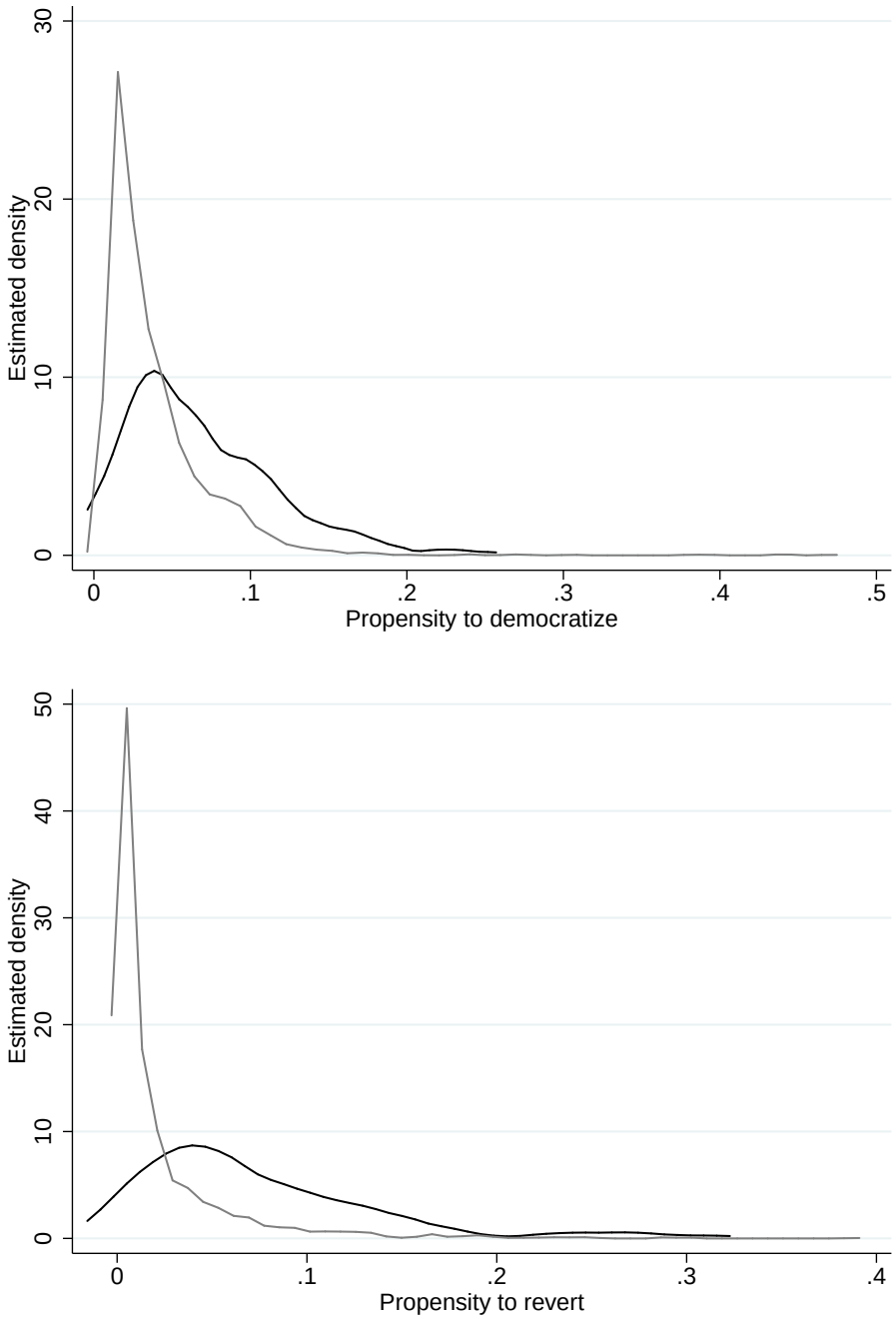
Notes: These figures plot semi-parametric estimates of the effect of a reversal to nondemocracy on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that reverted (in log points), together with a 95% confidence interval in dashed lines. Time (in years) relative to the year of reversal runs on the horizontal axis. Panel A presents results obtained via regression adjustment. Panel B presents results obtained via inverse propensity score reweighting. Panel C combines both approaches into a doubly-robust estimator, as explained in the main text.

FIGURE A.4: SEMI-PARAMETRIC ESTIMATES FOR THE AVERAGE TREATMENT EFFECT OF DEMOCRATIZATIONS.



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 A.5: SMOOTHED DENSITY OF THE ESTIMATED PROPENSITY SCORES.



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.

TABLE A.1: DEMOCRATIZATIONS 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 A.8. 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 A.2: REVERSALS 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
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 A.8. 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 A.3: SPECIFICATION TESTS FOR OUR PREFERRED DYNAMIC PANEL MODEL.

Panel A: Further lags of GDP uncorrelated with democracy					
Dependent variable:	Contemporary democracy level				
GDP lags included:	4 lags	8 lags	12 lags	16 lags	20 lags
	(1)	(2)	(3)	(4)	(5)
log GDP first lag	0.130	0.130	0.137	0.214	0.297
	(0.068)	(0.076)	(0.088)	(0.095)	(0.127)
log GDP second lag	-0.222	-0.218	-0.217	-0.260	-0.290
	(0.054)	(0.056)	(0.069)	(0.072)	(0.095)
log GDP third lag	0.007	-0.020	-0.032	-0.057	-0.081
	(0.048)	(0.055)	(0.064)	(0.072)	(0.083)
log GDP fourth lag	-0.053	-0.071	-0.069	-0.074	-0.080
	(0.062)	(0.051)	(0.066)	(0.074)	(0.086)
p-value first four lags	[0.000]	[0.001]	[0.019]	[0.012]	[0.052]
p-value additional lags		[0.201]	[0.121]	[0.052]	[0.115]
Observations	6,347	5,699	5,031	4,359	3,692
Countries in sample	175	175	173	170	165

Panel B: Democracy uncorrelated with future GDP shocks					
Dependent variable:	Estimated shock to GDP at t				
	(1)	(2)	(3)	(4)	(5)
Democracy first lag	-0.039				0.584
	(0.124)				(0.389)
Democracy second lag		-0.085			-0.874
		(0.123)			(0.522)
Democracy third lag			-0.067		-0.095
			(0.123)		(0.520)
Democracy fourth lag				-0.029	0.344
				(0.123)	(0.385)
Lags of democracy joint significance -	[0.755]	[0.487]	[0.587]	[0.814]	[0.372]
Observations	6,315	6,292	6,265	6,234	6,234

Notes: This table reports two specification tests for our model in equation (1). The top panel presents estimates of a model with democracy as dependent variable and lags of GDP as explanatory variables. The number of lags included is indicated in the top rows. All these models include a full set of country and year fixed effects. The bottom panel present estimates of lagged democracy on the estimated residual in equation (1). In each column we add different lags of democracy and report their joint p-value at the bottom of each model. For both panels, standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

TABLE A.4: EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA USING ALTERNATIVE DEMOCRACY MEASURES.

Measure of democracy:	Ours	P&S	FH	POL	CGV	BMR
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Within estimates with 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 with 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 without 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. 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 A.5: EFFECTS OF DEMOCRACY ON (LOG) GDP PER CAPITA REMOVING 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 of democracy is multiplied by 100. Column 1 presents our baseline within estimates. Column 2 removes countries with a a standardized residual estimated above 1.96 or below -1.96. In Column 3 we remove points with Cook's distance above the rule of thumb value of four over the number of observations. In Column 4 we compute a robust regression estimator that takes care of outliers by assigning them a lower weight following Li (1985). In Column 5 we present 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.

TABLE A.6: EFFECT OF DEMOCRACY ON GDP PER CAPITA USING ALTERNATIVE GMM ESTIMATES.

	Within estimator	Arellano & Bond different set of moments				Adding Ahn & Schmidt moments		
	(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 of 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. 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 A.7: EFFECT OF DEMOCRATIZATION AND REVERSALS ON 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	1.168
	(0.235)	(0.256)	(0.543)	(0.524)	(0.445)	(0.361)
Reversals	-0.705	-0.853	-1.313	-1.123	-0.465	-0.809
	(0.335)	(0.376)	(0.957)	(0.860)	(0.979)	(0.894)
Long-run effect of democracy	21.770	22.199	27.377	15.141	18.955	25.801
	(7.635)	(8.186)	(12.982)	(11.165)	(10.826)	(12.135)
Effect of democracy after 25 years	17.283	17.855	24.617	13.471	16.204	21.520
	(5.560)	(5.743)	(10.786)	(9.370)	(8.667)	(8.216)
Long-run effect of reversal	-19.116	-21.200	-24.450	-20.089	-9.301	-17.887
	(9.302)	(9.785)	(17.763)	(15.466)	(19.893)	(20.108)
Effect of reversal after 25 years	-15.177	-17.051	-21.985	-17.872	-7.951	-14.919
	(7.256)	(7.587)	(16.098)	(13.627)	(16.863)	(16.603)
Persistence of GDP process	0.963	0.960	0.946	0.944	0.950	0.955
	(0.005)	(0.007)	(0.011)	(0.012)	(0.011)	(0.012)
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 of 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. In all specifications we control for a full set of country and year fixed effects, as well as four lags of GDP. Even columns add up to eight lags of GDP as controls. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

TABLE A.8: ESTIMATED MARGINAL EFFECTS ON THE PROPENSITY TO DEMOCRATIZE OR REVERT.

	(1)	(2)	(3)	(4)	(5)
Panel A: Propensity to democratize based on past GDP					
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: Propensity to revert based on past GDP					
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 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. We allow past changes and levels of GDP to affect the probability of these events, and report their effects separately. In all specifications we control for a full set of year fixed effects, and add a varying number of GDP lags in each column. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

TABLE A.9: SPATIAL DIFFUSION PATTERNS OF DEMOCRACY, UNREST AND GDP.

	(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 A.10: IV ESTIMATES OF THE EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA CONTROLLING FOR 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
Countries in sample	174	174	174	174	174	173	174	174

Notes: This table presents different 2SLS estimates of the effect of democracy on GDP per capita instrumenting democracy with four lags of regional democracy. The coefficient of democracy is multiplied by 100. Column 1 presents our baseline 2SLS estimates. Column 2 removes countries with a standardized residual estimated 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 (4 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 A.11: EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA USING ALTERNATIVE CONSTRUCTIONS OF OUR INSTRUMENTS.

Instrument construction:	Baseline				Alternative			
Initial regime:	Base	1960-65	All years	Multiple	Base	1960-65	All years	Multiple
	(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.	33.2	12.3	45.1	8.3	965.4	130.6	513.6	303700.1
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 of 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 1960-64. 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 A.12: ESTIMATES OF THE EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA IMPOSING THE PERSISTENCE OF THE GDP PROCESS.

	Panel A: Within estimates					
Persistence $\rho = \sum \gamma_j$:	$\rho = 0.95$	$\rho = 0.96$	$\rho = 0.97$	$\rho = 0.98$	$\rho = 0.99$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)
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	-1.09e+16
	(4.943)	(5.712)	(7.255)	(10.795)	(22.342)	(2.16e+15)
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					
Persistence $\rho = \sum \gamma_j$:	$\rho = 0.95$	$\rho = 0.96$	$\rho = 0.97$	$\rho = 0.98$	$\rho = 0.99$	$\rho = 1$
	(1)	(2)	(3)	(4)	(5)	(6)
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	-3.13e+16
	(11.509)	(13.182)	(16.956)	(26.138)	(56.709)	(6.78e+15)
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)
Observations	6,309	6,309	6,309	6,309	6,309	6,309
Countries in sample	174	174	174	174	174	174
Exc. Instruments F-stat.	34.86	34.86	34.86	34.86	34.86	34.86

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 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. 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 A.13: MODELS WITH UNIT ROOT PROCESS FOR (LOG) GDP. THE DEPENDENT VARIABLE IS THE GROWTH RATE OF GDP.

	Non-IV estimates			Using waves as instruments		
	Within	Abond	HHK	Within	Abond	HHK
	(1)	(2)	(3)	(4)	(5)	(6)
Democracy	1.269	1.545	1.375	2.903	1.657	1.178
	(0.243)	(0.368)	(0.275)	(0.655)	(0.543)	(0.422)
Change in log GDP first lag	0.263	0.270	0.213	0.258	0.276	0.279
	(0.039)	(0.041)	(0.040)	(0.039)	(0.042)	(0.041)
Change in log GDP second lag	0.060	0.064	0.083	0.060	0.067	0.089
	(0.025)	(0.025)	(0.019)	(0.025)	(0.026)	(0.019)
Change in log GDP third lag	0.023	0.031	0.029	0.023	0.033	0.032
	(0.018)	(0.018)	(0.009)	(0.018)	(0.018)	(0.010)
Change in log GDP fourth lag	-0.033	-0.022	-0.007	-0.033	-0.018	-0.009
	(0.022)	(0.022)	(0.016)	(0.022)	(0.022)	(0.016)
Long-run effect of democracy	1.845	2.349	2.013	4.198	2.584	1.936
	(0.370)	(0.602)	(0.375)	(0.992)	(0.895)	(0.652)
Level effect of democracy after 25 years	45.275	57.296	49.050	103.029	62.822	46.742
	(9.038)	(14.572)	(9.147)	(24.255)	(21.674)	(15.785)
Persistence of GDP growth rates	0.312	0.342	0.317	0.308	0.359	0.391
	(0.038)	(0.041)	(0.030)	(0.038)	(0.044)	(0.031)
AR2 test p-value		0.98			0.97	
Observations	6,178	6,003	6,003	6,153	6,003	6,003
Countries in sample	175	175	175	174	175	175

Notes: This table presents estimates of the effect of democracy on changes of log GDP per capita. The coefficient of democracy is multiplied by 100. The estimation method used is indicated in the top rows, as well as whether we use external insrtuments for democracy. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita growth. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

TABLE A.14: HETEROGENEOUS EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA (ADDITIONAL ESTIMATES).

Interaction with:	Share with primary:				Share with tertiary:			
Measured at:	1960	1970	1980	Current	1960	1970	1980	Current
	(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	5300	5300	5300	5300	5300	5300	5300	5300
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 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. 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.