Democracy Does Cause Growth*

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Abstract

We provide evidence that democracy has a significant and robust positive effect on GDP. 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 linear model for GDP dynamics estimated using either a standard within estimator or various different Generalized Method of Moments estimators, and show that democratizations increase GDP per capita by about 20% in the long run. These results are confirmed when we use a semi-parametric propensity score matching estimator to control for GDP dynamics. We also obtain similar results using regional waves of democratizations and reversals to instrument for country 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.[1] For example, prominent *The New York Times* columnist Tom Friedman argues:[2]

> "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... The first lesson is that democracy is not the key to economic growth" (Barro 1997, pp. 1 and 11).

A more recent summary of the academic literature by Gerring et al. (2005) also reaches a similar conclusion: "the *net* effect of democracy on growth performance cross-nationally over the last five decades is negative or null."

In this paper we present evidence from a panel of countries between 1960 and 2010 challenging this view. Our results show a robust and sizable effect of democracy on economic growth. Our central estimates suggest that a country that switches from nondemocracy to democracy achieves about 20 percent higher GDP per capita in the long run (or roughly in the next 30 years). Our results indicate no differential effect of democracy on economic growth by the initial level of economic development, though there is some evidence that democracy is more conducive to higher GDP in countries that start out with higher levels of education.[3]

There are several challenges in estimating the impact of democracy on economic growth. First, existing democracy indices are typically subject to considerable measurement error, leading to spurious changes in the democracy score of a country even though its democratic institutions do not truly change. Second, because democratic and nondemocratic countries differ in many institutional, policy, historical and cultural aspects, cross-country comparisons are subject to a

[1] This broad popular view notwithstanding, a number of papers have estimated positive effects of democracy on growth and there are some theoretical reasons for expecting such positive effects, as we discuss below.

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

[3] 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 described 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).

myriad of biases and are unlikely to reveal the causal effect of democracy on growth. Third, as shown in Figure 1, democratizations are preceded by temporary movements in GDP (at least in the raw data). A reliable estimate of the impact of democracy on future GDP needs to model and estimate the dynamics of the GDP process. Fourth, even with year and country fixed effects, changes in democracy may be correlated with other changes or respond to current or future economic conditions (Acemoglu et al., 2005, Brückner and Ciccone 2011), raising obvious omitted variable bias concerns.

In this paper, we make progress in addressing all four of these challenges. First, we build on the important work by Papaioannou and Siourounis (2008) to develop a dichotomous index of democracy purged of spurious changes in democracy scores available in the standard datasets, and rely on this measure for most of our analysis (in the Appendix we show robustness to many other measures).

Second, we include country fixed effects in all specifications in order to remove the impact on economic growth of fixed country characteristics potentially correlated with democracy. We also include year fixed effects to remove any common global changes in democracy that may be correlated with GDP.

Third, we allow for and estimate serially correlated dynamics in (log) GDP using a number of different strategies. Our first strategy is to control for lags of GDP in linear regressions. Our second strategy is to adapt to our panel context the semi-parametric time-series estimators proposed in Angrist and Kuersteiner (2012) and Angrist, Jordà, and Kuersteiner (2013), which use propensity-score-based matching methods to correct for the effects of GDP dynamics.

Fourth, in addition to controlling for a full set of country and year fixed effects, we use an instrumental-variables (IV) strategy to overcome omitted variable bias. Partially building on previous work on the effect of "democratic capital" on growth by Persson and Tabellini (2009), we develop an instrument for democracy based on regional waves of democratizations and reversals. Our identification assumption is 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 (at least conditional on lagged levels of country and regional GDP, and various covariates that could be correlated with country-level GDP at the year, region and initial regime level).

The importance of modeling the dynamics of GDP and a glimpse of the effect of democratization on future GDP can be seen in Figure 1. The figure plots (log) GDP per capita in countries that democratize relative to continuing nondemocracies, with year 0 corresponding to the year of democratization. It shows that, on average, democracy is preceded by a sharp and persistent fall in GDP. This pre-democratization GDP dip (the downward trend before year 0) makes it clear that

the failure to model the dynamics of GDP can lead to sizable bias in the estimates of the impact of democracy on GDP growth. This figure also shows the higher level of future GDP following a democratization, which is at the root of the positive estimates of the impact of democracy on growth we report in the rest of the paper.

Our baseline strategy to deal with the confounding effects of GDP dynamics is to include a sufficient number of lags of GDP in annual panel data regression with country and year fixed effects. Ordinary Least Squares (OLS) or Generalized Method of Moments (GMM) estimates lead to fairly stable estimates of the dynamics of GDP. Similar results are obtained when we use semi-parametric propensity score matching to control for the dynamics of GDP. Crucially for our inference, once these dynamics are modeled, there is no differential GDP dip for democratizing countries. Figure 2 provides a preview by plotting the behavior of GDP around democratizations (relative to continuing nondemocracies) when we control for GDP dynamics using the semi-parametric propensity score matching (which is particularly conducive to a visual analysis). The lack of a significant downward trend before year 0 in this figure is indicative of a more general pattern: our various strategies for controlling for GDP dynamics ensure that democratizations are (conditionally) uncorrelated with past GDP. The figure also shows that there is a significant and clearly-visible increase in GDP per capita following a democratization relative to continuing non-democracies. Our baseline and semi-parametric results suggest that modeling the dynamics of GDP is critical for obtaining the correct counterfactual for the impact of democracy on economic growth—though the exact estimates are not very sensitive to the details of the specification or estimation strategy.

Our instrumental-variables (IV) strategy uses our regression-based correction for GDP dynamics and exploits exogenous variation in regional waves of democratization to identify the effect of democracy (essentially comparing countries that are otherwise similar but are affected by differential waves of regional democratization). This IV strategy also leads to sizable and (depending on the specification) somewhat larger estimates of the impact of democracy on GDP than the non-IV 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, increasing schooling, encouraging economic reforms, improving public services and reducing social unrest.

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 the view that democracy becomes a hindrance to economic growth below a certain threshold of development. But we do find some heterogeneous effects by the level of education, suggesting that democracy has more positive effects for economies

with a greater fraction of the population with (secondary) schooling.

The rest of the paper is organized as follows. The next section discusses the prior theoretical and empirical literature on the relationship between democracy and growth. Section 3 describes the construction of our democracy index, provides sources for our main variables and presents descriptive statistics for our sample. Section 4 presents our baseline results, which use a linear model for controlling for GDP dynamics. This model is estimated using the standard within estimator and various GMM estimators. This section also presents a variety of robustness checks. Section 5 presents results using the semi-parametric propensity score matching estimator. Section 6 presents our IV results using regional democratization and reversal waves, which yield similar results to our two other strategies. Section 7 presents evidence on potential channels through which democracy might be affecting economic growth. Section 8 investigates whether democracy has heterogeneous effects depending on the level of economic development and education. Section 9 concludes, while the Appendix provides several additional robustness checks and results.

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. A large literature has 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.[4] Relatedly, Acemoglu (2008) argues 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 barriers that these other political systems tend to erect to protect politically powerful incumbents.

[4]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.

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). Barro (1996) reported results from cross-country regressions showing a small negative effect on economic growth, with some evidence of a nonlinearity where democracy increases growth at low levels of democracy but reduces it at higher levels (see also Helliwell, 1994). In subsequent work, Tavares and Wacziarg (2001) also focus on cross-country regressions and report a weak negative effect (adjusting for a variety of other channels), while Persson and Tabellini (2008) find a positive effect using propensity score matching. However, this type of work relying on repeated cross-sections and failing to tackle the four empirical challenges we pointed out in the Introduction is unlikely to be informative about the causal effect of democracy on growth.

Papers focusing on panel data regressions include Rodrik and Wacziarg (2005) and Persson and Tabellini (2008), who find a positive effect of recent democratization on growth, Bates, Fayad and Hoeffler (2012), who find positive effects for Africa, and Burkhart and Lewis-Beck (1994) and Tabellini and Giavazzi (2005), who find no significant effects on growth. These and other papers in this literature all 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.[5]

Our work builds on the important paper by Papaioannou and Siourounis (2008). They 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. In addition, Papaioannou and Siourounis do not tackle the empirical challenges related to the endogeneity of democracy and the modeling of the dynamics of GDP (though they do report a robustness check related to this).

Our work also builds on and complements Persson and Tabellini (2009), who use a related IV strategy based on neighbors' democracy to estimate the effect of "democratic capital", defined as the sum of recency-weighted past democracy, on economic growth (see also Ansell, 2010, and Aidt and Jensen, 2012). In addition to differences related to how the instruments are constructed and the fact that Persson and Tabellini do not model GDP dynamics, a central difference is in the right-hand side variable. Following an earlier literature in political science (e.g., Gerring et al. 2005),

[5] 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) investigates 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), Fujiwara (2012), and Naidu (2012).

Persson and Tabellini focus on the effect of democratic capital. One formidable challenge here is the difficulty of identifying the impact of democratic capital separately from country fixed effects (which is just a reflection of the difficulty of distinguishing duration dependence and unobserved heterogeneity).

Another closely related literature investigates the effect of economic growth on democracy. This literature, which was pioneered by Lipset (1959), was also partly revived by Barro (1996, 1999). We do not focus on this relationship here, except to note that Acemoglu et al. (2008, 2009) show no evidence of a statistical or causal effect from economic growth to democracy.[6]

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 countries in all periods. 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 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 that 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. We also use these measures to code the few instances that are missing in both Freedom House and Polity IV. 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, though we have GDP data for only 175 of those, which make up our baseline sample.

The major difference between our index of democracy and that of Papaioannou and Siourounis is that they focus on *permanent* changes in democracy status. One drawback of this 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

[6]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.

as a right-hand side variable in GDP regressions. Instead, we code all transitions to democracy and reversals (transitions to nondemocracy).[7] This procedure gives us 122 instances of democratization and 71 reversals, which are shown in Appendix Tables A1 and A2. Out of 8,733 country/year observations, 3,777 are coded as democratic while 4,956 are nondemocratic.

Our main outcome variable, log GDP per capita in 2000 constant dollars, is from the World Development Indicators. When we examine mechanisms, we use investment, trade (exports plus imports), secondary and primary enrollment, and infant mortality data, all from the World Development Indicators, as well as TFP data from the Penn World Tables and tax revenues from Hendrix (2010). We also 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). We use the data on economic reforms coded by Giuliano, Mishra and Spilimbergo (2013), which includes indices of product market, agriculture, trade, financial system, current account and capital account reforms, to construct an aggregate (average) index of economic reform normalized between 0 and 100.

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, that democracies are richer and have more educated populations.

4 Results

In this section, we provide our baseline results using linear regression models.

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, while the α_c's denote a full set of country fixed effects and the δ_t's denote a full set of year effects. The error term ε_{ct} includes all other 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.

Until we consider IV models in Section 6, we will maintain the following assumption:

[7] 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.

Assumption 1 $E[D_{cs}\varepsilon_{ct}] = 0$ for all $s \leq t$.

This assumption implies that democracy is orthogonal to the contemporaneous and lagged error terms conditional on the lagged dependent variables and the country and year fixed effects already included in equation (1).

Under Assumption 1, the simplest strategy is to estimate equation (1) using the standard within estimator.[8] 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 interpretation, and standard errors are robust and clustered by country.

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 the lag of 0.973 (standard error = 0.006), but this coefficient is still significantly less than 1. The adjusted t-statistic from Levin, Lin and Chu (2002) for a unit root in a panel setting is also reported at the bottom.[9] In column 1, the statistic is -4.971, comfortably rejecting a unit root in the empirical process for log GDP per capita. More importantly for our focus, 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 following democratization, GDP per capita is higher by about 1%. However, the serially-correlated nature of GDP implies that this effect will accumulate over time. For example, in the second year, GDP per capita will be higher by about 2%, and so on. To obtain the long-run impact (for a permanent change in democracy), we need to compute the sum of these effects over time, which is given by

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

where $\widehat{\ }$ denotes the parameter estimates, and this formula is written for the general case with several lags on the right-hand side. Applying this formula to the estimates from column 1, we find the long-run effect as 35.59, meaning democratization increases GDP per capita by 35.6% in

[8] 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 the number of times a country appears in the estimation sample.

[9] 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. The 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.

the long run (and the p-value underneath this number indicates that this estimate is statistically different from zero at the 1.1% confidence level).

Column 2 adds a second lag of GDP per capita to this specification, and shows that both lags are highly statistically significant and point to richer dynamics (with the first lag being positive and greater than 1, while the second one is negative). But the sum of the two lags, shown also in the GDP persistence row at the bottom, is close to that found in column 1, and Levin, Lin and Chu's test again comfortably rejects a unit root. The effect of democracy is slightly lower but still highly significant, 0.651 (standard error =0.248). Correspondingly, the long-run impact is now smaller and more plausible, indicating a 19.60% increase in GDP per capita in the long run.

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 their estimates in column 2. In particular, the coefficient of democracy is 0.787 (standard error=0.226) and the long-run impact is a 21.24% (p-value=0.003) increase in GDP per capita following a democratization.[10]

Column 4 includes four more lags of GDP (for a total of eight lags) and shows that these additional lags are not significant. 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.[11] We do not report the coefficients of these lags to save space, but present the p-value of a test for their joint significance. This test suggests that including four annual lags of GDP per capita appears sufficient to capture the rich dynamics of GDP in the linear regressions (and there is again no evidence of a unit root in this extended specification).

The problem with the estimates in columns 1-4 is the so-called Nickell bias which results when panel data models with fixed effects and lagged dependent variables are estimated by the standard within estimator and the time dimension, T, is finite. This bias is of the order $1/T$ and thus disappears as T grows large (Nickell 1981, Alvarez and Arellano 2003). Since T is fairly large in our panel (on average, each country is observed 38.8 times), the standard within estimator should have at most only a small bias.[12] This motivates our use of the models in columns 1-4 as the

[10]These robust, clustered standard errors are in fact quite close to non-robust, non-clustered standard errors (e.g., equal to 0.228 for column 3), which supports the conclusion that our estimates successfully model GDP dynamics.

More conservatively, we also computed standard errors robust to serial correlation within a region $\times$ initial regime $\times$ year cell, which are similar but slightly larger than those reported here. For example, for column 3, the standard error for the democracy coefficient in this case is 0.263.

[11]Though columns 1-4 show that the exact lag length included on the right-hand side does not change the qualitative results, the Appendix shows that not including any lags — a common strategy in the literature — does lead to misleading results.

[12]Returning to footnote 8, 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} (as long as $\gamma_j \neq 0$ for some j). Clearly as T_c tends to infinity, this bias disappears. In the text, we simplify the discussion by referring to T, the average number of times a country appears in the panel.

Monte Carlo studies by Judson and Owen (1999) suggest that the Nickell bias is of the order of 1% for $T = 30$.

baseline.

The rest of Table 2 reports various GMM estimators that are consistent for finite T. Under Assumption 1 and the additional assumption that ε_{ct} is serially uncorrelated, we have the following moment conditions

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

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.[13] Consistent with our expectations that the within estimator has at most a small bias, the GMM estimates are very similar to our baseline results from columns 1-4. The only notable difference is that GMM models have consistently slightly smaller persistence, leading to somewhat smaller long-run effects. For example, column 7, corresponding to our preferred specification in column 3, estimates a long-run impact of 16.45% increase in GDP per capita following a democratization.

In addition, the underlying identification assumption for this GMM procedure can be investigated directly. In particular, we can test the hypothesis that there is no serial correlation in the residuals of equation (1)—or equivalently that there is no AR2 correlation in the differenced version of this equation—which is used as a key exclusion restriction for the GMM estimator. The numbers reported in Table 2 indicate that this assumption is rejected when we include only one or two lags, which is not surprising, since a specification with one or two lags only fails to adequately control for the dynamics in GDP per capita (recall the significance of the third and the fourth lags in columns 3 and 4). However, when four lags or more are included as in columns 7 and 8, there is no evidence of further serial correlation in the residuals.

An alternative to Arellano and Bond's GMM estimator is proposed by Hahn, Hausman and Kuersteiner (2002) and relies on forward orthogonal differences. Hahn et al. note that Arellano and Bond's GMM estimator is a minimum distance estimator combining $T-1$ 2SLS estimates.[14]

[13] 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. This is related to the fact that because the number of moments is of the order of T^2, there is a potential "many instrument problem". This problem affects the efficiently weighted estimator, but not the one we use, which remains consistent under "large N, large T" asymptotics.

[14] More specifically, it 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

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

Arellano and Bond's GMM estimator is an efficiently weighted combination of these $T-1$ 2SLS estimates.

They propose replacing each 2SLS estimate with a Nagar-type estimator, which is robust to the use of many instruments, and combine these estimates by weighting them by the number of observations in year t times the inverse of their variances.[15] 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, though slightly larger. For example, in column 11, which corresponds to our preferred specification, the long-run effect, which is 24.51%, is about 50% larger than the GMM estimate in column 7 but only slightly larger than our baseline within estimate in column 3.

In the rest of the paper we focus on the specification with four lags of GDP as our benchmark for several reasons. First, Table A3 in the Appendix shows that, once four lags of GDP are included on the right-hand side, the correlation between democratizations and transitory movements in GDP disappears. Second, as Table 2 shows, there is also no further serial correlation in the residuals. Third, Table A4 in the Appendix shows that the predicted residual $\widehat{\varepsilon}_{ct}$ is uncorrelated with lags of democracy. Therefore, there is no evidence of correlation between residual GDP (once dynamics are taken into account) and past democracy, weighing against an interpretation in which democratizations take place in anticipation of future changes in GDP.

4.2 Robustness

Table 3 probes the robustness of our results to the inclusion of various covariates. Notice that any time-invariant covariate is already absorbed by the country fixed effects. Thus our focus will be on time-varying variables, including various regional trends.

Table 3 comprises three panels: the top one using the within estimator, the middle one Arellano and Bond's GMM, and the bottom one the HHK estimator. We only report the coefficient estimates on the democracy index to save space. 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

[15] Returning to footnote 14, the equation for time t in forward orthogonal differences is estimated using a k-class estimator with $\{y_{cs}, D_{cs}\}_{s=1}^{t-1}$ as instruments. The general k-class estimator is given by

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

where X are the endogenous right-hand side variables, Z the instruments, and Y the dependent variable, and M_Z denotes orthogonal projections on Z. Nagar's (1959) estimator is obtained by setting k equal to $1 + \frac{L}{N}$, with L being the degree of overidentifying restrictions and N the number of countries. This estimator is robust to finite-sample bias in the case of many instruments (in contrast to the traditional 2SLS estimator which sets $k = 1$). In particular, as shown by Hahn, Hausman and Kuersteiner (2002). Because the 2SLS estimates described in footnote 14 are biased when both T and N are large, Arellano and Bond's GMM estimator also has an asymptotic bias of the order $1/N$. In contrast, the Nagar estimate of each of the $T-1$ cross-sectional equations in footnote 14 is unbiased when T is large. The HHK estimator, obtained by efficiently combining these Nagar estimates, is thus also asymptotically unbiased. We compute standard errors using 100 bootstrap repetitions.

set of year effects.[16] This specification is useful for two reasons. First, it controls for potentially time-varying effects of baseline differences across countries.[17] 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 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 to control for the effects of political and economic changes following the fall of the Berlin wall in the Soviet Union and Eastern Europe (and thus ensuring that our results are not driven by the experiences of these Soviets block countries). This has little impact on the results. The long-run effect of democracy increases slightly to 24.86% because the coefficient of democracy is larger in this specification.

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 on growth or for the possibility that external shocks are driving both growth and democracy. These controls have a limited impact on our estimates in all panels.

Finally, 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 in GDP between countries undergoing democratizations or reversals in democracy relative to other countries in the same region. This estimate thus fully controls for any omitted variable varying at the region $\times$ initial regime level. This specification is motivated by our IV strategy in Section 6, where we use regional democracy waves as instruments. In this light, the specification in column 6 of this table exploits the variation in the data that is orthogonal to the one our IV focuses on. Reassuringly, this specification leads to very similar estimates to our baseline results (and also to our baseline 2SLS results contained in Table 5 below).[18]

[16]To compute the GDP per capita rank in 1960 we use Angus Maddison's estimates, since many more countries are missing GDP per capita data in 1960 in the World Bank data.

[17]We can also go further in this direction and control for interactions between (log) GDP per capita in 1960, non-agricultural share of labor in 1968, urbanization in 1960 and share with secondary education in 1960 with a full set of year effects. Though the sample becomes smaller given data availability, our within and 2SLS estimates remain similar. In particular, the within estimate for democracy becomes 0.598 (standard error=0.283). The 2SLS estimate for democracy becomes 1.307 (standard error=0.612). However, since the interactions with baseline characteristics are correlated with the instruments in the Arellano-Bond GMM and HHK specifications, the results in Panels B and C become highly imprecise.

[18]We have also explored (but do not report) several specifications partly 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$\times$initial regime cells (democracy or nondemocracy at the beginning of the

We also report several additional robustness checks in the Appendix. First, in Table A5 in the Appendix, we explore the sensitivity of our baseline results to outliers. In particular, we re-estimate our preferred specification without countries with a standarized residual above 1.96 or below -1.96. In addition, we estimate our preferred specification without countries 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.

Second, in Table A6 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 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.

Third, in Table A7 we pursue an alternative strategy to deal with both the potential Nickell bias and 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 (meaning that the sum of the coefficients on lags of GDP per capita, $\sum_{j=1}^{p} \gamma_j$, is restricted to be equal to the specified number). We then estimate the impact of democracy on GDP per capita under this restriction. These estimates show that our results are remarkably robust to imposing different levels of persistence. 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. The last column imposes that $\sum_{j=1}^{p} \gamma_j = 1$, which leads to a standard consistent estimator in the presence of a unit root. It shows a positive and statistically significant impact of democracy on growth, demonstrating that even if there were a unit root in the GDP per capita process, our conclusions would remain valid (though now democracy would permanently change the growth rate and thus would have a much larger impact as shown in the table).

Fourth, in Table A8 we explore if our results are robust to other measures of democracy that have been used in the literature. In particular, 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 using Cheibub, Gandhi, and Vreeland's

sample) to take into account regional shocks among countries with similar political characteristics. These controls had practically no impact on our key estimates.

democracy-dictatorship measure. Consistent with the presence of considerable measurement error in these alternative democracy indices, the IV estimates they produce are much larger than the OLS estimates compared to the results with our baseline measure (or with Pappaioannou and Siourounis's).

In Table A9, we explore separately the effect of democratizations and reversals (transitions from democracy to nondemocracy) and different components of democracy. Both democratizations and reversals in democracy yield consistent results—democratizations increase GDP and reversals reduce it.

In Table A9 we also explore the role of different components of democracy coded by Freedom House and Polity. We find that civil liberties are somewhat more important for GDP than political rights. However, our estimates using the Polity dataset are too imprecise to draw any strong conclusions. These results are subject to the caveat that these components may be measured with greater error than the overall democracy indices, making unbundling the components of democracy particularly challenging.

5 Semi-Parametric Control Strategies

In the previous section, we controlled for GDP dynamics using linear regression models (and various different estimation strategies). An alternative is to adopt a semi-parametric correction for GDP dynamics. In this section, we follow Angrist et al. (2013) and adapt their propensity score matching estimator for time series models to our panel context. This framework enables us to estimate the effects of democratizations and reversals in democracy separately, while flexibly controlling for GDP dynamics and their correlations with changes in democracy. We explain the construction of the estimator for the case of a democratization (the construction for a reversal is entirely analogous).

5.1 Semi-Parametric Estimates

Let $\Delta y_{ct}^{j}(d)$ denote "potential" change in (log) GDP per capita at time $t+j$ of a country with $\Delta D_{ct} = d$, i.e., what the change in log GDP per capita would have been for country c if it did or did not democratize ($d = 1$ and 0, respectively). With this notation, the effect of a democratization j periods after it occurs on the change in GDP per capita is

$$\beta^{j} = \mathbb{E}[\Delta y_{ct}^{j}(1) - \Delta y_{ct}^{j}(0)].$$

The assumption underlying the semi-parametric estimator used in this section is the following *conditional independence assumption* (CIA):

Assumption 1′ $\Delta y_{ct}^{j}(d) \perp \Delta D_{ct} | D_{ct-1}, y_{ct-1}, y_{ct-2}, y_{ct-3}, y_{ct-4}, t$ for all c, t, j.

The focus on changes of GDP and democracy to eliminate persistent differences between countries (i.e., removes country fixed effects). In this light, the CIA simply states that once permanent differences between countries, time effects, lagged democracy and GDP dynamics are taken into account, further changes in democracy are as good as randomly assigned. We impose Assumption $1'$ throughout this section.

Let P_{ct} be the probability of a democratization in country c at time t conditional on D_{ct-1}, y_{ct-1}, y_{ct-2}, y_{ct-3}, y_{ct-4}. We refer to this probability as the *propensity score* following Angrist et al. (2013). Let Δy_{ct+j} be the actual change in log GDP per capita for country c at time $t+j$. In the data, the effect of a democratization j periods after it occurs can be estimated as a weighted average of growth rates, given by

$$\widehat{\beta}^j = \mathbb{E}\left[\Delta y_{ct+j}\widehat{w}_{ct}\right],$$

where $\mathbb{E}$ here denotes the sample average and the $\widehat{w}_{ct}$'s are weights given by

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

These weights correspond to Hirano, Imbens and Rider's (2003) efficient weighting scheme. They essentially reweight the data so that observations with a very high or very low propensity score get a much larger weight. Intuitively, observations with a very high or very low propensity score are those with changes in democracy that are not predicted by year effects and lags of GDP per capita, thus corresponding to changes in democracy "orthogonal" to past levels of GDP per capita.

To estimate the weights, we first specify a model for the propensity score. We estimate $\widehat{P}_{ct}$ from a Probit model for whether $\Delta D_{ct}=1$ or $\Delta D_{ct}=0$ conditional on $D_{ct-1}=0$, and using $y_{ct-1}, y_{ct-2}, y_{ct-3}, y_{ct-4}$ and δ_t as explanatory variables. This parametrization of the propensity score is what makes the approach semi-parametric. Notice that the weights are only defined for observations for which $D_{ct-1}=0$, which are the relevant sample to study the effect of a democratization (otherwise the propensity score is zero by definition). In Table A1 in the Appendix, we list all democratization in the sample, together with their propensity scores, while Table A2 does the same for reversals.

Using this procedure, we compute estimates of $\widehat{\beta}^j$ for $j=-15,-14,\ldots,30$, with year 0 corresponding to the year of democratization. The average treatment effect on GDP is computed as the cumulative sum of these effects on the growth rate starting from a base year, in this case the year before democratization (and this base year's log GDP is normalized to zero). The top panel of Figure 3 plots the full estimated effects of a democratization on GDP over time. Time runs in the horizontal axis, and is normalized so that the democratization occurs at time $t=0$. The solid line plots the increase in GDP per capita caused by democracy, and the dotted lines plot a 95% confidence interval obtained by bootstraping. We see that following a democratization GDP

increases gradually, becoming about 15% higher between 25 and 30 years after a democratization. The estimates for negative values of j can be computed and used to test if there are any significant pre-trends before the democratization. These are also plotted in Figure 3, as the data points before year zero. Reassuringly, they show no significant pre-trends before democratization. The absence of pre-trends also suggests that this estimator is removing any GDP dynamics potentially correlated with democratization (of the type visible in Figure 1 in the Introduction).

The estimates are also presented in Table 4. Panel A shows that during the five years before the democratization (column 1), GDP per capita was on average 1.74% higher than in the baseline year, but this difference is not only small, but also statistically insignificant.[19] During the first five years following democratization (column 2), GDP per capita is 1.43% higher. However, 25 to 30 years after the democratization (column 3), GDP per capita is 15.06% higher, and this increase is significantly different from zero (p-value of 0.06).

If the propensity score is correctly specified and we have a large sample, the estimated weights, $\widehat{w}_{ct}$ should have mean zero and should be orthogonal to $y_{ct-1}, y_{ct-2}, y_{ct-3}, y_{ct-4}$, and δ_t. Thus, as a specification and finite sample correction, we also construct estimates in which we replace the weights with residualized weights, after partialling out the covariates, to compute the average treatment effects. The bottom panel of Figure 3 plots the estimated effects on GDP per capita relative to the base year. This adjusted estimator does a better job of controlling for the dynamics as can be gauged from the flat pattern of GDP before the democratization. The effect of democracy on GDP per capita now with these adjusted estimator is slightly larger and more precisely estimated: a democratization now increases GDP by about 18% in 25-30 years. These results are presented in Panel B of Table 4. Column 1 shows precisely estimated zero pre-trends. Column 2 shows that GDP increases by 2.14% in the first five years following democratization (p-value=0.058), and by 17.58% between 25-30 years following democratization (p-value= 0.013).

Notably, these effects are very similar to the long-run estimates in our baseline linear models, suggesting that the specific parametrization of the GDP process imposed in equation (1) is a good approximation to the actual dynamics.[20]

We follow an analogous procedure to compute estimates for the average effect of a reversal from democracy to nondemocracy. The top panel of Figure 4 plots the baseline results and the bottom panel plots the results using the residualized weights. The results without the adjustment for weights do not appear to be reliable since they do not eliminate pre-trends in reversals as is visible in the figure. This failure possibly reflects poor finite sample properties of our estimator

[19] Test p-values are constructed using 100 bootstrap repetitions clustering at the country level.

[20] The long-run impact estimated here is unconditional in the sense that democracy could be reversed in subsequent years, whereas the long-run effects in the linear models are computed assuming a permanent democratization (recall equation (2)). This accounts for the difference between the long-run estimates in this section and the baseline estimates.

given the few reversals in our sample (relative to democratizations). The right panel shows that adjusting the weights goes a long way in eliminating pre-trends in GDP. All the same, the estimates in this case, summarized in Panels C and D of Table 4, are still less precise than the estimates for a democratization, and we put less value on them.

Overall, our semi-parametric estimates confirm the sizable and statistically significant effect of democratizations on economic growth, but also show that there is not enough information in the sample to tightly pinpoint the impact of reversals in democracy on economic growth.

5.2 Two Illustrative Examples

The end of the Portuguese Estado Novo in 1974 and the South Korean transition to democracy in 1988 nicely illustrate the sort of information our semi-parametric estimator exploits (because they are both identified as low propensity score democratic transitions and provide us with 20 or more years of post-democracy data). In Portugal, the 1974 coup replaced Salazar's right-wing dictatorship with a left-wing dictatorship which, after a series of further coups, eventually gave way to democracy. Portugal held its first elections in 1976 (which is when we code it as a democracy). As emphasized by the low propensity score of this democratization episode in Table A1 (0.018), democracy was not an ex ante likely outcome in Portugal. There was no economic crisis precipitating the downfall of Salazar's dictatorship, and Fearon and Laitin (2005) describe the situation as "There is hardly any doubt that the anocracy and instability of the period from 1974-75 put Portugal on the verge of insurgency." Scholars generally agree that the organization and internal culture of the military helped prevent a civil war and brought about the successful transition to democracy (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 A1). 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 (e.g., Cumings, 1997).

The long-run growth effects of the resulting democratic transitions are palpable 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 compared to an average growth rate of 0.5% during the same period among the six countries with the closest GDP per capita to Portugal in 1975 (Barbados, Gabon, Oman, Trinidad and Tobago, Uruguay and Venezuela). 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 income 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 in 1989 following democratization. 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 below.

6 IV Estimates: Democratization Waves

The econometric strategies adopted so far control for GDP dynamics but impose Assumption 1 or $1'$, which imply that democracy, conditional on the correctly specified GDP dynamics, country and year fixed effects and controls, is exogenous. This is clearly a restrictive assumption. First, there could be concerns related to reverse causality from GDP to democracy. Though the timing assumptions and the results in Acemoglu et al. (2008, 2009) are reassuring here, it may still be the case that countries democratize when there is potential "good news" about future GDP. Second, there are the usual omitted variable concerns. For example, bad policy decisions of nondemocratic rulers may induce both a fall in GDP and demands for democratization, or political unrest paving the way for democracy might also disrupt economic growth. Though many of these concerns, conditional on correctly specified GDP dynamics, should lead to a downward bias in our estimates, there could be other plausible candidates that might cause an upward bias. In addition, as discussed in the Introduction, democracy indices are notoriously ridden by measurement error, even after our construction of the dichotomous index, and this will naturally cause a downward attenuation bias in the estimate of democracy on economic growth.

In this section, we develop an instrumental-variables (IV) strategy, which can (at least partially) correct for these reverse causality, omitted variable bias, and measurement error concerns.

6.1 First Stage and Exclusion Restrictions

The motivation for our IV strategy is that, as highlighted by the recent Arab Spring experience, democratizations often occur in regional waves. For example, 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.[21]

Though there is no consensus on why there are such waves, they cannot be explained by regional economic trends. Rather, they appear to be related to the demand for democracy (or dissatisfaction with democracy) spreading from one country to another, reminiscent of models in which there is cross-country learning (e.g., Ellis and Fender, 2010, Kuran, 1989, and Lohmann, 1994, for theoretical models, and Aidt and Jensen, 2012, Buera, Monge-Naranjo and Primiceri, 2011, and Persson and Tabellini, 2009, for empirical evidence). This perspective suggests that regional waves could be an attractive source of exogenous variation in democracy (see also Persson and Tabellini, 2009). We introduce the formal exclusion restriction next after defining our parametrization of this instrument.

We start with a look at democratization waves and reversals in Figure 5. The top panel depicts the evolution of average democracy among countries that were initially nondemocracies within one of seven regions (Africa, East Asia, Eastern Europe and Central Asia, Latin American and the Caribbean, Middle East and North Africa, South Asia and in other categories including Western Europe and other long-standing OECD countries) after the first democratization in the region.[22] For comparison, we show average democracy among initial nondemocracies in the other regions (which start, by construction, having higher levels of average democracy). Following the first democratization in a region, average democracy there rises faster than average democracy 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 democratization and reversal waves and estimate our first-stage relationship, we define jack-knifed average democracy in a region$\times$ initial regime cell, Z_{ct}, which leaves out the own country (country c) observation as

$$Z_{ct} = \frac{1}{N_{rinit} - 1} \sum_{c' \in r, D_{c'init} = D_{cinit}, c' \neq c} D_{c't}. \tag{3}$$

Here, r designates the seven regions mentioned above, $D_{cinit} \in \{0, 1\}$ is a dummy variable indicating if the country was initially democratic ($D_{cinit} = 1$) or nondemocratic ($D_{cinit} = 0$) during the first years it appears in our sample, and N_{rinit} denotes the number of countries in that region$\times$ initial regime cell at time t.[23] This construction, which also conditions on the initial regime, is motivated by the fact that democratization can only happen in nondemocracies, and conversely, reversals can only happen among democracies. We use lags of Z_{ct} as our instruments.

[21] 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 Treisman (2013). Our first-stage results document the presence of robust waves.

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

[23] That is, after 1960 for countries that were not colonies in 1960, or after independence for the rest.

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

$$\begin{aligned} y_{ct} &= \beta D_{ct} + \textstyle\sum_{j=1}^{p} \gamma_j y_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct} \\ D_{ct} &= \textstyle\sum_{j=1}^{q} \pi_q Z_{ct-q} + \sum_{j=1}^{p} \phi_j y_{ct-j} + \theta_c + \eta_t + \upsilon_{ct}. \end{aligned} \tag{4}$$

This is identical to our linear models above, except that democracy will be treated as endogenous, instrumented by the lags of Z_{ct}.

Therefore, our exclusion restriction is that regional waves of democratizations and reversals have no impact on a country's GDP except through their influence on that country's democracy, conditional on lags of GDP and year and country fixed effects.

Assumption 2 $\mathbb{E}[Z_{cs}\varepsilon_{ct}] = 0$ for all $s \leq t-1$.

The economic justification for this assumption is that, conditional on covariates, switches to democracy in neighboring countries should have no direct effect on GDP per capita in the region. Though this exclusion restriction is plausible, there could be reasons why it might be violated (e.g., political instability may affect regional trade patterns or capital flows). Nevertheless, we show below that controlling for such variables has little effect.

Note the importance of correctly specifying GDP dynamics. Even if Assumption 2 held, misspecified dynamics will lead to a violation of the exclusion restriction. If lags are not controlled for, past GDP per capita of a country could be correlated with both current GDP and regional waves of democracy.

The strong first-stage relationships underlying our 2SLS estimates are shown in Panel B of Table 5. Jack-knifed regional democratization or reversal waves (by initial regime type) have a strong predictive power on democracy, as shown by the first-stage F-statistics, which are always above 16. In terms of time patterns, the largest impact is from the one-year lag, though the effects continue for at least three years. Importantly, different columns of Table 5 show that the first-stage relationship is very stable across specifications with different regional covariates, bolstering our confidence in our exclusion restriction (if the regional correlation in democracy reflected the effect of some correlated shocks on the democracy of countries in the region, then the inclusion of proxies for these regional covariates would be expected to have a much more significant effect on the first stages).

6.2 2SLS Estimates

Panel A of Table 5 presents our 2SLS estimates of the model in equation (4), with the first stages given in Panel B as mentioned in the previous subsection. Panel C then presents a version of the HHK estimator described in Section 4 using lags of regional democratization or reversal waves as

(external) instruments, and will be discussed later.[24] This estimator is consistent and corrects for the Nickell bias as long as Assumption 2 holds and ε_{ct} is serially uncorrelated.[25]

Column 1 of Panel A gives the benchmark within estimate from column 3 of Table 2 for comparison. Column 2 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 the within estimate in column 1. The long-run effect of democracy is also a little larger, 26.31%.

Column 3 uses four lags of Z_{ct} as instruments. This leads to a slightly larger 2SLS coefficient of 1.149 (standard error=0.554) and a long-run effect of 31.52%. The increase in the estimated impact of democracy on GDP in columns 2 and 3 may reflect the fact that some of the potential downward biases mentioned above were in fact important or that there was attenuation due to measurement error. The inclusion of several lags of Z_{ct} as instrument allows us to perform a Hansen-type overidentification test which, under the Assumption that Z_{ct-1} is exogenous, allows us to test if Z_{ct-2}, Z_{ct-3} and Z_{ct-4} are also exogenous. While this does not directly test Assumption 2, it does provide indirect support for it, since no past lags of the instrument are correlated with the outcome variable once the first lag is included.

Columns 4 and 5 add the same country-specific controls as in Table 3, interactions between 1960 GDP per capita quintile and a full set of year effects and interactions between a dummy for Soviet and Soviet satellite countries and dummies for the years 1989, 1990, 1991, and post-1992, respectively. These have relatively modest effects on our 2SLS estimates.

More importantly, there are several reasons why regional waves could be correlated with the structural error term in the GDP equation. For instance, our exclusion restriction would be violated if there were regional shocks that simultaneously affect GDP in a country and lagged democracy in the region. Or as already mentioned above, regional democracy could affect trade or financial flow patterns. The rest of the table investigates how including various regional or country level controls affects our 2SLS estimates.

In column 6 we add four lags of the average GDP in the region×initial regime cell, partly as a control for the possibility that regional economic conditions could be the trigger for regional democratization (rather than the other way around as assumed by our exclusion restriction). The 2SLS estimates are now larger but still highly significant (and the first stage remains stable).

[24] In particular, using the notation from footnote 14, 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.

[25] We do not not present results from Arellano and Bond's GMM estimator in this case since asymptotic properties of this GMM estimator with many external instruments are unknown.

Column 7 controls for four lags of the average unrest in the region×initial regime cell. This controls for the possibility that unrest (not only democracy) may spread across countries (consistent with Aidt and Jensen, 2012), and have a direct effect on growth. Column 8 controls for four lags of the average share of trade in the region×initial regime cell, partly as a control for the possibility of trade flows to the region being affected by the democratization wave. These controls leave our results in all three panels fairly close to the baseline estimates.

Finally, column 9 controls for region-specific trends, which is also useful for probing further whether our IV results are driven by region-specific trends correlated with the waves of democratization. Reassuringly, we obtain similar estimates for the impact of democracy, consistent with our exclusion restriction that our instrument is not capturing differential secular trends across regions.

Observe also that, as already noted in the previous subsection, the first stages in columns 6-9 are quite stable, suggesting that these first stages are not being driven by correlated economic shocks to countries in the same region×initial regime cell. Rather, this pattern is consistent with our interpretation that democratization in one country increases the demand for democracy in the region.

The HHK estimates in Panel C are broadly similar to our 2SLS estimates. In column 3, we obtain a smaller (and perhaps more plausible) long-run effect of 22.37%. The coefficient estimate and the long-run effect are significant only at 7% and 13%, respectively, in this column (and as in Table 2, this estimator does not perform well when the lag structure is cut short as in column 2), but the HHK estimates are more precisely estimated and significant at 5% or less in all other columns, except in column 5, where we include time interactions with a dummy for Soviet and Soviet satellite countries.

We also conducted a number of further robustness checks for our results, focusing on the specification in column 3, which are presented in Table A10 in the Appendix. In particular, 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 A11. 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 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 exploiting the plausibly exogenous sources of variation in democracy

resulting from regional democratization (and reversal) waves leads to estimates of the impact of democracy on GDP in the ballpark of our baseline 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

$$z_{ct} = \beta D_{ct} + \sum_{j=1}^{p} \gamma_j y_{ct-j} + \sum_{j=1}^{p} \eta_j z_{ct-j} + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{5}$$

where z_{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.[26]

We estimate (5) using the within estimator (corresponding to column 3 of Table 2), our preferred specification for the 2SLS estimator (corresponding to column 3 of Table 5, Panel A, with $p = 4$), and our preferred specification for the HHK estimator with the same external instruments for democracy (corresponding to column 3 of Table 5, Panel C).[27] These results are presented in Table 6. In addition, Figure 6 presents the semi-parametric estimates (using the same estimator as in Section 5. The variables we investigate 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.

The results for most of these variables are not as clear-cut as our baseline findings for GDP per capita. In several cases, there are noteworthy differences between the four estimators (the three in Table 6 and the semi-parametric one in Figure 6). The only variables for which we have consistent results with all four estimators are the index for economic reforms and child mortality. We also obtain broadly consistent positive estimates for the effect of democracy on tax to GDP ratio and primary enrollment rates, though the HHK estimates are not precise and the semi-parametric estimates show a smaller and imprecise response for the tax to GDP ratio and a tapered effect for primary enrollment. In addition, for investment to GDP ratio, secondary enrollment and social unrest, we have two of the estimators showing precisely-estimated results and two of them showing less well estimated effects.

[26] Excluding the lags of GDP per capita on the right-hand side leads to broadly similar results. We prefer to control for GDP, since some of the variables we focus on may respond to democracy precisely because GDP responds to democracy, in which case they are unlikely to be the main mechanisms through which democracy affects GDP.

[27] For the HHK estimator, we first partialled out the lags of GDP.

Overall, we take these results as suggesting that democracy might be working through a number of channels, in particular, by encouraging economic reforms, increasing human capital (especially primary schooling), and raising state capacity and some aspects of public services (especially related to health) as well as, to some degree, increasing investment and 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:[28]

> "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:[29]

> "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 to be such that the impact of democracy for low economic development or for low schooling countries is 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). The first four columns are for log GDP per capita and the second four for share of the population with secondary schooling. In each case, we evaluate the main effect of democracy at the

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

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

bottom 25th percentile of the interaction variable (so that it indicates whether democracy has a negative effect for countries at a low level of economic development or with low levels of schooling). For the interaction variable we use the baseline values in 1960, 1970 or 1980, or the current value of the variable.[30]

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. Nevertheless, these interactions are not large enough to make democracy have any significant negative effect for low human capital countries, but they 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).

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

[30]For log GDP per capita, "current" 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.

future GDP per capita. This result remains true in GMM estimates that account for any bias due to lagged dependent variables, as well as with semi-parametric estimators based on a propensity score for democratic transitions estimated using past lags of log GDP. Our preferred specifications imply that long-run GDP increases by about 20% following a democratic transition.

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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APPENDIX

A1 Construction of the Democracy Index

This Appendix explains in detail the construction of the democracy measure used in the paper. We construct a consolidated democracy measure using Freedom House and Polity IV as our main sources.[31] We also use secondary sources to resolve ambiguous cases or those without data coverage from 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 two dichotomous measures that extend Przeworski et al.'s (2000) work. These are Cheibub, Gandhi, and Vreeland (2010)—henceforth CGV—and Boix-Miller-Rosato's (2012)—henceforth BMR.[32] Finally, we use Papaioannou and Siourounis's (2006) — henceforth PS — measure to date the exact timing of some of the democratic episodes that we identify based on historical sources. In total, there are 183 countries for which one of these sources is available. Our dichotomous democracy index is available for these 183 countries and covers their post-independence period since 1960. In our empirical exercises we restrict attention to the sample of 175 countries for which we also have GDP data.

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 gives the core variation in our democracy measure.[33]

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 of these cases.[34]

[31] We use Freedom House and Polity as our main sources for two reasons. First, these datasets have been used extensively in the literature. Second, they are based on experts assessments which combine important *de jure* and *de facto* elements of democracy. In contrast, our secondary sources are procedural and are based solely on *de jure* components. We view the correct notion of democracy as comprising a bundle of both *de jure* and *de facto* elements.

[32] 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 must have taken 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.

[33] 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 (2006) 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.

[34] 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, and 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.[35]

4. Soviet and Ex-Yugoslav countries are taken as independent throughout, and we use the USSR and Yugoslavia scores for them in years before these countries' dissolution, so they are all nondemocracies before 1990.

5. Finally, when both Freedom House and Polity are missing, we rely on our secondary sources. We have 174 observations for 16 countries that are only covered by CGV and BMR for which we code our measure manually.[36] In all of these cases, both sources provide a consistent view of these countries.

We perform two additional refinements. First, we use PS's democratization dates when there is overlap with their sample (which is the case for 68 transitions to democracy), and then we modify our coding to reflect PS dates of democratization based on historical sources.[37]

Second, we check for spurious transitions created by countries entering and leaving the Freedom House, Polity, or our secondary datasets. We only detect such transitions for Cyprus, Malaysia,

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 that was in place in 2008.

[35]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.

[36]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. It enters the Freedom House sample in 1972 and is still coded as democratic. Germany is set as always democratic based on West 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. In 1963 it enters the Polity sample where it is also coded as nondemocratic. The Maldives are set as nondemocratic from its independence in 1965 until it enters the Freedom House sample in 1972, where it continues being nondemocratic. Malta is set as democratic from its independence in 1964 until it enters the Freedom House sample in 1972, where it is also 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 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.

[37]For five countries, our algorithm did not produce transitions close to PS official dates of democratization. These countries are Guatemala, El Salvador, Iran, Tanzania and South Africa. For Guatemala, we code a democratization in 1986 based on all of our sources, 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, while PS do. In all of these cases we keep our original coding. Our algorithm produces a permanent 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.

Gambia and Guyana, which we handled on a case-by-case basis. The particular coding of these countries does not affect our results.[38]

It is important to note that, in contrast to Papaioannou and Siourounis, we do not impose any "stability criteria" and also code "temporary" transitions. Though some temporary switches in and out of democracy may be driven by measurement error, focusing only on "permanent democratizations" (or permanent reversals) would create an obvious bias because the democracy index would be a function of future events, thus potentially correlated with future GDP.

Overall, our 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. There are a total of 122 democratizations and 71 reversals, suggesting large within country variation in our democracy measure.

Figure A1 presents time-series plots, for the whole world and also separately for each of our regions, our measure and the alternative indices. Freedom House and Polity are shown in blue and are normalized to lie between 0 and 1. The figures are presented for the whole world and then by geographical region. As can be seen, all measures show very similar patterns in all regions and are highly correlated (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).

In Tables A1 and A2, we list all democratizations and reversals in our sample. We also present the estimated propensity scores from our semi-parametric analysis in Section 5. The estimated propensity score is missing for countries for which lags of GDP are not in the sample. Countries that do not appear in these tables are always nondemocracies or democracies in our sample.

A2 Specification Tests

As a first check on our baseline AR4 specifications, 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 affects, democracy is (conditionally) uncorrelated with past GDP dynamics. Table A3 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

[38]In particular, 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. None of the results reported in the paper change if we exclude these countries.

occur following temporary declines in GDP.[39]

Column 2 adds lags 5-8 of GDP. Column 3 adds lags 9-12 of GDP. Column 4 adds up to the 16th lag and finally, column 5 adds up to the 20th lag of GDP. 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, making us conclude that a specification with only four lags of GDP successfully models GDP dynamics before a democratization or a reversal.

As an additional check on our specification, we test if the estimated error term $\widehat{\varepsilon}_{ct}$ is uncorrelated with lags of democracy. Table A4 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.

A3 Robustness to Outliers

We investigate the robustness of our baseline within estimates to outliers in Table A5. Column 1 shows estimates for our baseline model for comparison. In column 2 we remove points with a standardized residual (in our baseline) above 1.96 or below -1.96. In column 3 we remove points with a Cook's distance (in our baseline) above the rule-of-thumb value of 4 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.[40]

Overall, the results in Table A5 show that our within estimates are not driven by outliers. Though the point estimates for the coefficient on democracy are generally smaller, the estimated GDP persistence is greater, leaving the long-run effect of democracy broadly unchanged from our baseline.

A4 Additional GMM Estimates

Arellano and Bond's GMM estimator exploits a full set of moment conditions derived from Assumption 1 and the additional assumption of no serial correlation in the error term. We now explore the robustness of our results to using different sets of moments in Table A6.

Column 1 presents the baseline within estimator and column 2 repeats the baseline GMM estimator from Table 2. Column 3 replaces the moments formed using lags of democracy with the

[39] This is consistent 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.

[40] This estimator is similar to the robust regression estimator used in column 4, but avoids the biweight weighting which creates convergence problems in some cases.

single moment $E[(\varepsilon_{ct} - \varepsilon_{ct-1})D_{ct-1}] = 0$. This brings the number of moments down by half. 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. The advantage of this strategy is that fewer moments are needed to estimate the GDP per capita process, allowing us to use up to the fifth lag of GDP when forming the moment conditions.[41] Both the estimated persistence and the coefficient of democracy are greater in this case, implying a larger long-run effect of 37.56%. But this effect is imprecisely estimated because the persistence term is close to 1, so the delta method used to construct standard errors performs poorly.

As an additional check, we add Ahn and Schmidt's (1995) moment conditions. These conditions are nonlinear—and this is why they are ignored in many cases—but they follow from the same assumptions required by Arellano and Bond's baseline GMM estimator. The additional moments take the form (in a balanced panel)

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

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.[42] Comparing the estimates in columns 2-4 with their counterparts in columns 6-8 shows that these additional nonlinear moment conditions improve the fit to GDP dynamics. In particular, GDP persistence in these columns is 0.96, which is closer to our baseline within estimates, but the coefficient of democracy is greater, leading to a larger estimate of the long-run effect on democracy.

A5 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. To do so, we rearrange equation (1) as

$$y_{ct} - \rho y_{ct-1} = \beta D_{ct} + \sum_{j=1}^{p-1} \eta_j (y_{ct-j} - y_{ct-j-1}) + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{6}$$

[41] The results using this transformation are equivalent to Arellano and Bond's baseline GMM estimator when all moment conditions are exploited, but when only a subset of moment conditions is exploited, they may differ.

[42] We incorporate these additional conditions using an iterative procedure: We start with the estimates obtained using the linear conditions. At each step, we add the nonlinear conditions computed with the previous estimated coefficients. We iterate the procedure 15 times, which in practice leads to convergence.

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 (6) for 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 A7 presents our within estimates and 2SLS estimates for these values of ρ. These estimates have three advantages. First, recall that the the estimated persistence level using the within estimates in Table 2, 0.96, may have a small downward bias of the order $1/T$ (see Nickel, 1982), which can then induce a bias in the estimates of the effect of democracy. Though we have argued that this bias is small based on other GMM estimates and the fact that we have a panel with a long time dimension, imposing different values of ρ enables us to have a different line of attack that is immune to the Nickel bias. These results in Table A7 show that even if the level of persistence of the GDP per capita process were above 0.95, we would obtain very similar results. For all levels of persistence, democracy has a positive and significant effect on GDP per capita, that actually becomes larger as ρ increases. The second advantage of this procedure is that the transformed variable $y_{ct} - \rho y_{ct-1}$ is heavily stationary for all the imposed values of ρ. In particular, we can rule out the presence of unit roots in the transformed variable $y_{ct} - \rho y_{ct-1}$ using a variety of unit root tests for panels. Therefore, these estimates do not suffer from complications related to the potential existence of unit roots in the GDP process. Finally, the last column, which imposes $\rho = 1$, is a consistent estimator under a unit root, and shows that the effects are similar (though larger because now democracy affects the growth rate permanently). This result thus demonstrates that even in the presence of a unit root (which does not seem likely given our results), there is a sizable impact of democracy on growth.

A6 Other Measures of Democracy

In this section we study whether our results hold using different measures of democracy. These measures include dichotomous versions of Freedom House and Polity, PS, CGV and BMR democracy measures already described above.[43]

Table A8 presents our results using these alternative measures. Panel A presents within estimates of our baseline model using all these alternative measures of democracy. In Panel B we present 2SLS estimates using the specification in column 3, Panel A, Table 5. Finally, in Panel C we present within estimates that do not control for GDP dynamics. Column labels specify the measure of democracy used in each specification.

[43]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 for 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 the former Soviet Union).

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. This could reflect the possibility that these measures are noisier, or that procedural measures like CGV and BMR are missing important *de facto* elements 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), suggesting that these alternative measures are more heavily affected by measurement error than our baseline measure. This provides support for our approach of constructing a consolidated measure to mitigate attenuation bias. The 2SLS results using the Freedom House dichotomous measure stands out for being considerably larger than the others. However, the first stage is somewhat weak in this particular case as shown by the low F-statistic of 3.87.

As discussed in the Introduction, a reliable estimate of the effect of democracy needs to control for GDP dynamics. This is confirmed in Panel C, which shows that not controlling for serial correlation in GDP at all has a significant impact on the results. In this case, the estimates of the effect of democracy on economic growth vary considerably depending on the measure used. If anything, most measures produce a misleading negative effect of democracy on GDP by falsely attributing the lower GDP before a democratization and its subsequent impact on GDP to democracy. These results suggest that the impact of 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 robust and consistent results and lack of such results in several papers in the literature.

A7 The Effects of Components of Democracy

In this section we estimate the effect of different components of democracy on growth. Column 1 in Table A9 shows within estimates of the effect of democratizations and reversals separately. These models suggest that democratizations have an immediate impact on GDP per capita, increasing it by 0.8% (standard error=0.235), and a long-run effect of 21.77%. A reversal, on the other hand, reduces GDP per capita by 0.7% (standard error=0.335) in the short run and by 19% in the long run. The estimates for reversals are less precise given the limited number of such transitions in our data, but still statistically significant.

The Polity dataset codes its democracy variable based on several components. The first component is whether there are constraints on the executive. We code a dummy that takes the value of 1 for country/years in which there is a substantial limitation on executive authority, or parity or subordination to accountability groups, as captured by a polity score greater than or equal to 5 in this component. The second component is the competitiveness and openness of the execu-

tive recruitment process. We code a dummy that takes the value of 1 for country/years in which the executive authority is chosen in competitive elections, as captured by a Polity score greater than or equal to 8 in this component. The last component we explore is the competitiveness of participation, or the extent to which several political backgrounds or groups can influence policy. We code a dummy that takes the value of 1 for country/years in which political participation is open to different political groups, as captured by a polity score greater than or equal to 5 in this component.

Columns 2-5 present estimates for the Polity components of democracy. Our within estimates suggest that executive constraints are not the key component of democratizations associated with future economic growth. Though the estimates are imprecise, they suggest that the two components that are more strongly associated with growth are openness and competitiveness of executive recruitment, and especially the competitiveness of political participation. The long-run effects reported at the bottom suggest that the competitiveness of participation has the largest impact on GDP per capita, increasing it by 9.68% (though this is far from being statistically significant). In column 5 we add all components simultaneously. The estimates are even less precise due to the high correlation between all components, but still show similar qualitative results.

Likewise, Freedom House also reports two components. The first one, termed political rights, is the component of democracy encompassing the electoral process, political pluralism and functioning of the government. The second one, termed civil liberties, is the component of democracy related to freedom of expression and belief, associational and organizational rights, rule of law, personal autonomy and individual rights. We code dummies for both of them that take a value of one when the respective score is below 4 in the 1 to 7 Freedom House scale. Columns 6-8 present estimates for the different components of the Freedom House index. Our results suggest that both components, especially civil liberties, matter for economic growth. An improvement in political rights increases GDP per capita by 6.65% ($p-$value 0.159) in the long run; while an improvement in civil liberties increases it by 10.25% ($p-$value 0.049). When both components are entered simultaneously, the point estimates become imprecise but their sum still has a significant long-run effect on GDP per capita, increasing it by 10.82% ($p-$value 0.056).

A8 Robustness to Outliers (2SLS Estimates)

We explore the robustness of our 2SLS estimates to outliers in Table A10. We focus on our preferred 2SLS specification presented in column 3, Panel A of Table 5. Column 1 reproduces these 2SLS 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 4 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 are similar to our main results, suggesting that our 2SLS estimates are not driven by outliers in the second stage. Only the model in column 4 produces a larger long-run effect, but this is mostly driven by the larger GDP persistence estimated in this model.

In the remaining columns, we present estimates in which we eliminate 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.[44] 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 4 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. Only the model in column 7 produces a smaller long-run effect, which is close or to our baseline within estimate. Overall, the evidence suggests that outliers have little effect on our estimates.

A9 Alternative Regional Instruments

In this section, we show that our 2SLS results are robust to different constructions of the regional 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 A11 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

[44] We compute standard errors using a Sandwich estimator formula similar to the one in Murphy and Topel (1985) and presented in Stefanski and Boos (2002). This adjusts for the fact that we are using a generated regressor in the second stage.

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× 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 (3), using this alternative region× 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 are similar, if somewhat larger, but still plausible effects of democracy.

We also explore an alternative way of capturing regional waves other than the one presented in equation (3). In particular, we construct a set of instruments of the form

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

This makes the number of instruments equal to 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.

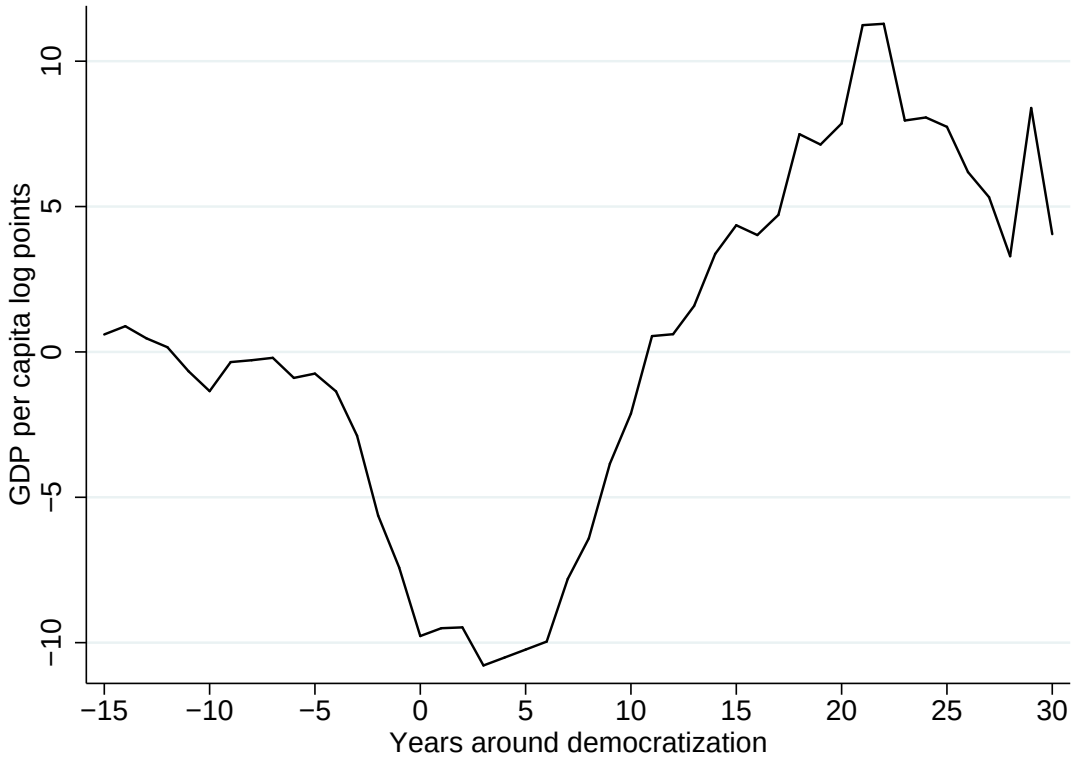
Columns 5-8 of Table A11 present results using these alternative constructions of the instrument. We use four lags of the instruments as in our baseline results. Column 5 presents 2SLS estimates obtained using our baseline definition of initial regimes. The results are similar to our baseline 2SLS estimates, though slightly smaller and less precisely estimated. 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.

A10 Appendix: Additional Heterogeneous Effects

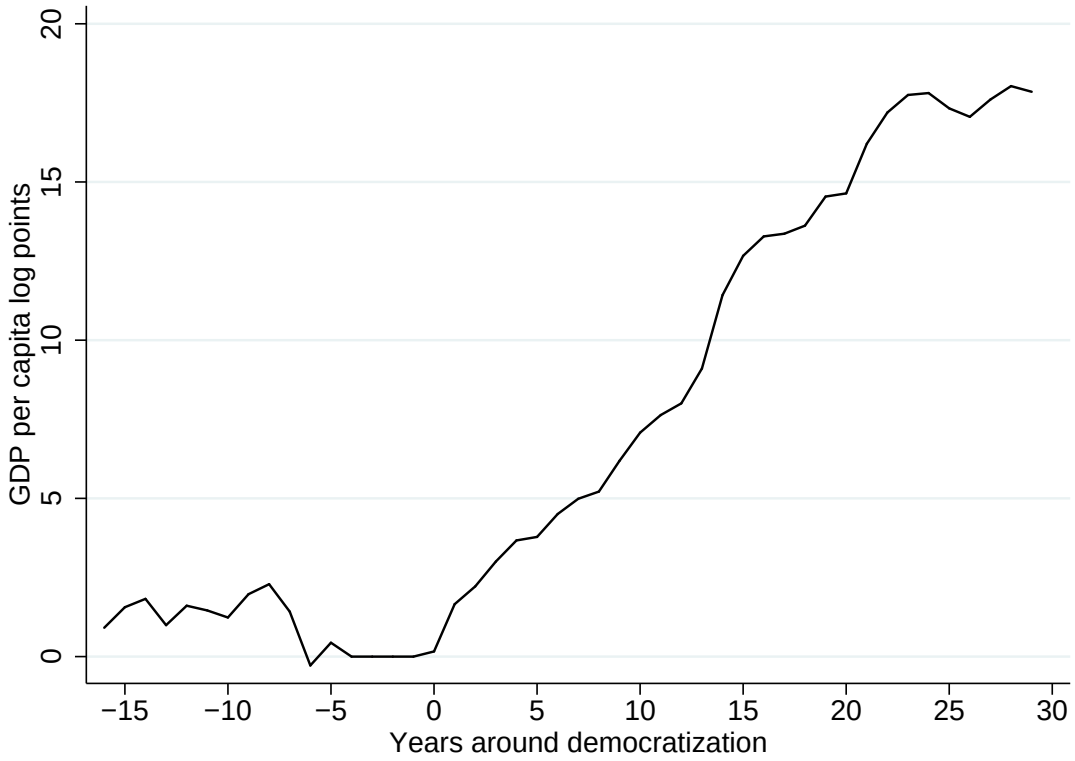
Table A12 presents estimates interacting democracy with other measures of education. Columns 1-4 focus on the share of the population with primary education from the Barro-Lee dataset, while columns 5-8 present results using the share with tertiary education. We do not find evidence of a consistent interaction between democracy and these alternative measures of education.

FIGURE 1: GDP PER CAPITA ARROUND A DEMOCRATIZATION.



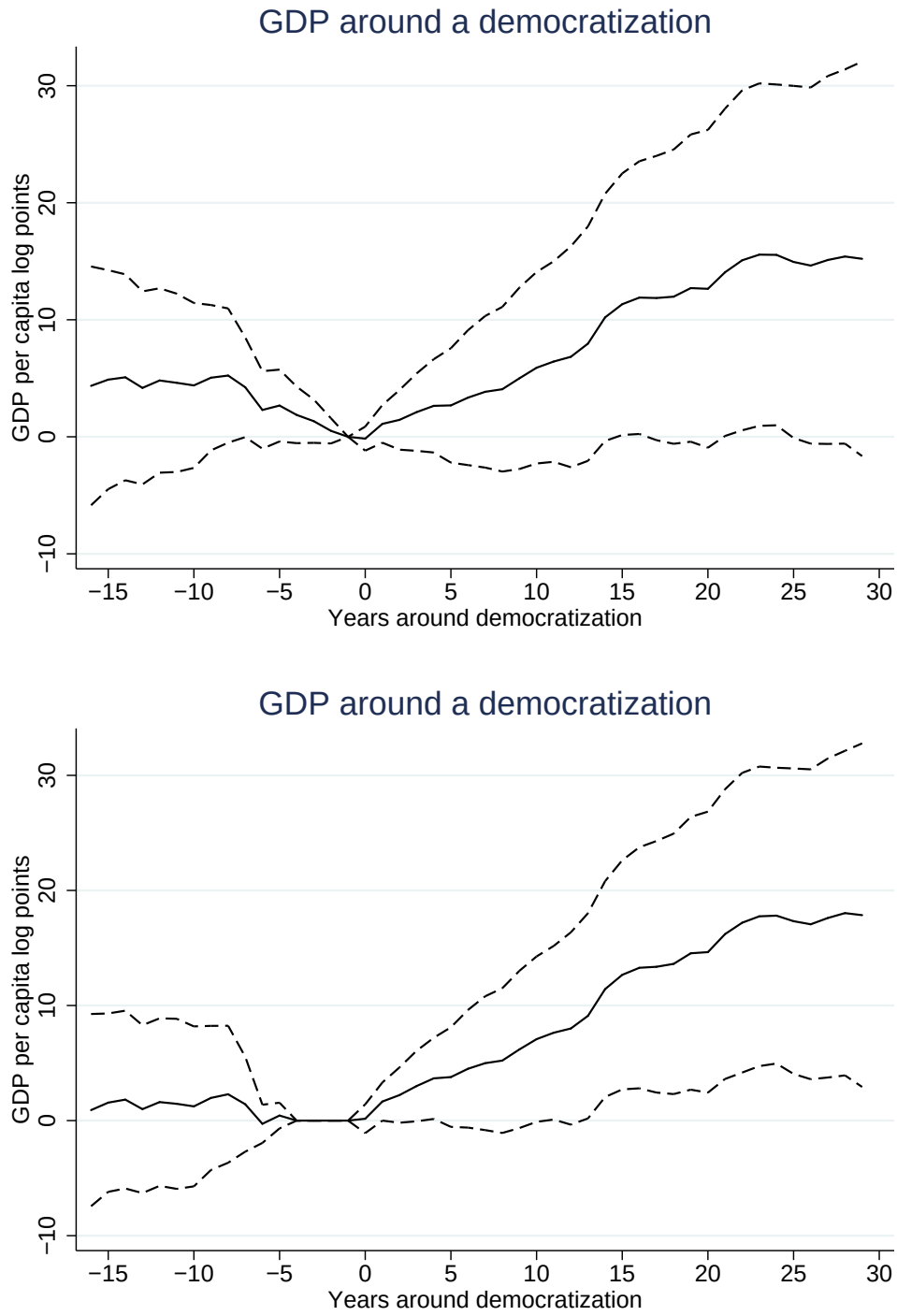
Notes: figure plots GDP per capita in log points around a democratic transition. We normalize the average log GDP per capita in a country to zero. Time (in years) relative to the year of democratization runs on the horizontal axis.

FIGURE 2: GDP PER CAPITA IN A DEMOCRATIZATION IN REWEIGHTED DATA.



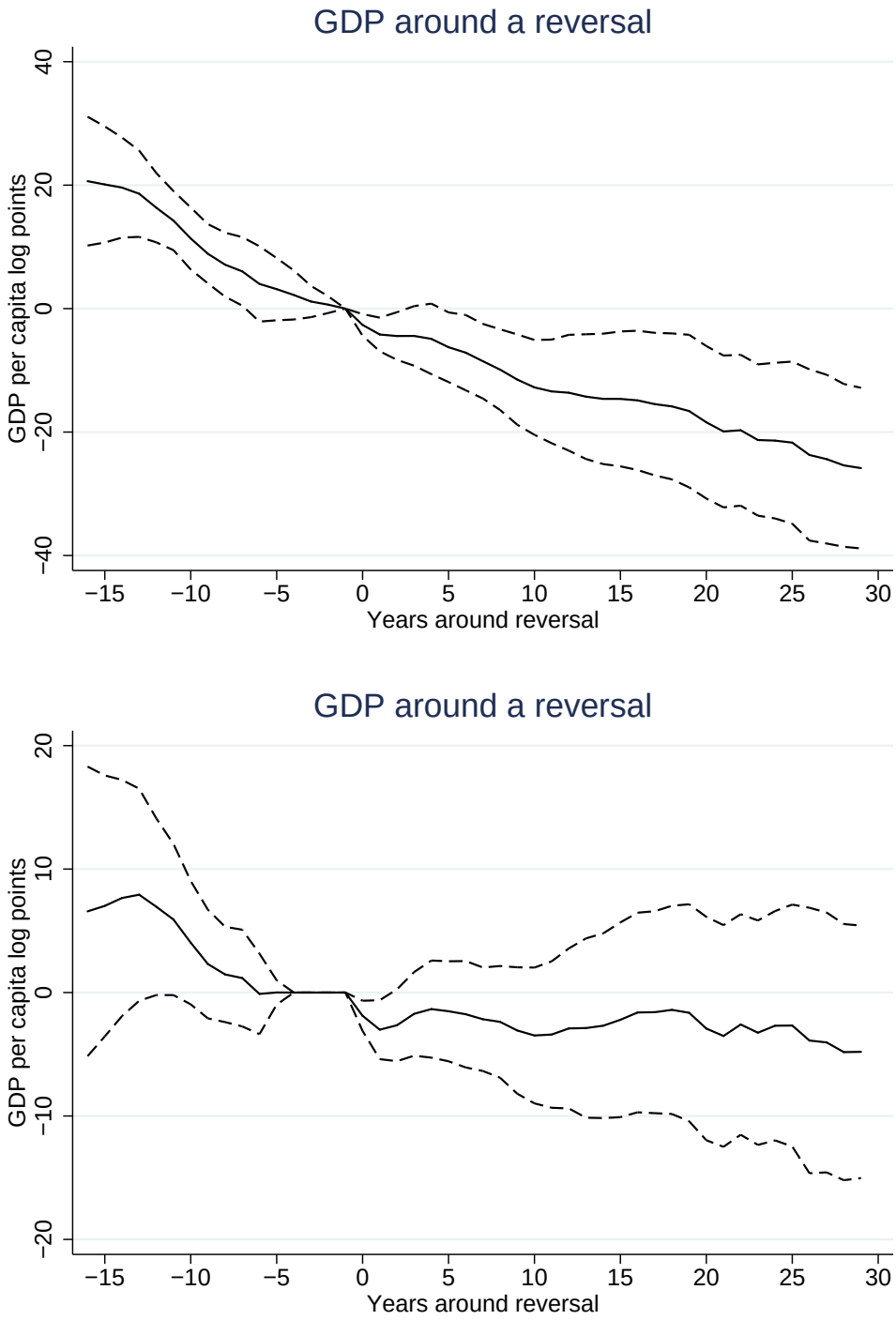
Notes: The figure plots GDP per capita in log points around a democratization. The reweighting controls semi-parametrically for the GDP dynamics before a democratization. We normalize the log GDP per capita in a country in the year before the democratization to zero. Time (in years) relative to the year of democratization runs on the horizontal axis.

FIGURE 3: SEMI-PARAMETRIC ESTIMATES OF THE EFFECT OF A DEMOCRATIZATION ON GDP PER CAPITA.



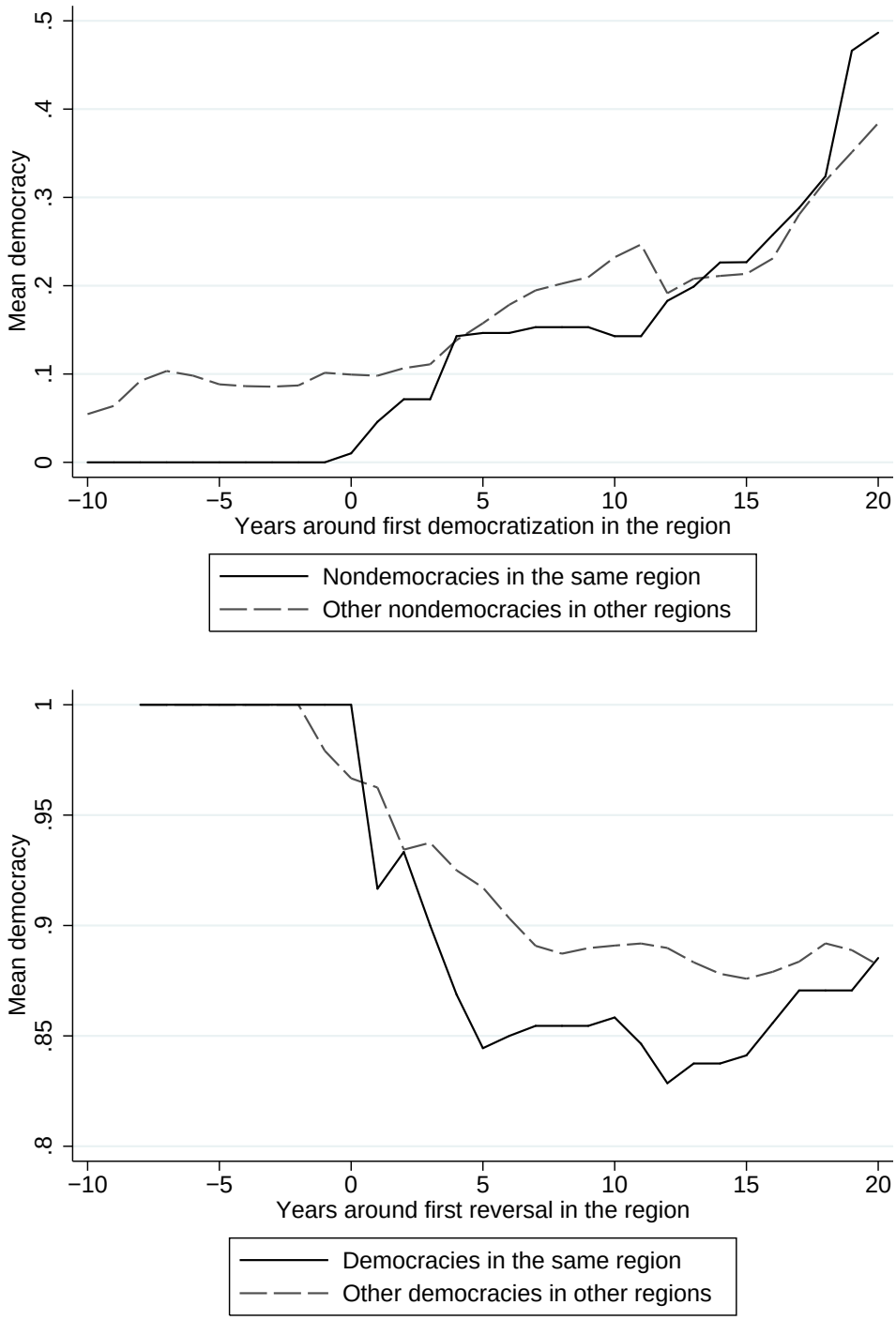
Notes: The figure plots estimates of the effect of democratizations on GDP per capita in log points. The estimates report the change in GDP per capita relative to a continuing nondemocracy, normalizing the difference in the year before the event to zero. The estimates are obtained after reweighting the data using an inverse propensity score weighting. The reweighting controls semi-parametrically for lags of GDP per capita as explained in Section 5. The bottom figure uses a finite sample correction in the estimation of the weights. Time (in years) relative to the year of democratization runs on the horizontal axis.

FIGURE 4: SEMI-PARAMETRIC ESTIMATES OF THE EFFECT OF A REVERSAL ON GDP PER CAPITA.



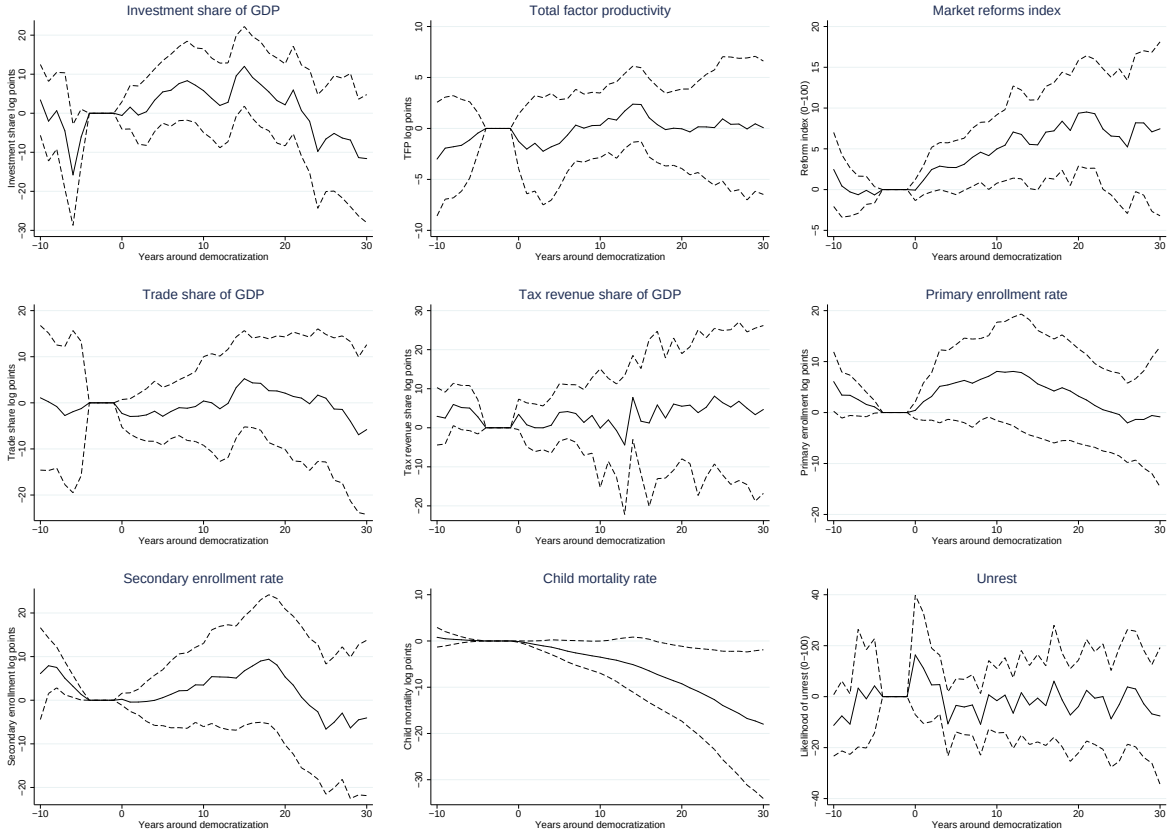
Notes: The figure plots estimates of the effect of reversals on GDP per capita in log points. The estimates report the change in GDP per capita relative to a continuing democracy, normalizing the difference in the year before the event to zero. The estimates are obtained after reweighting the data using an inverse propensity score weighting. The reweighting controls semi-parametrically for lags of GDP per capita as explained in Section 5. The bottom figure uses a finite sample correction in the estimation of the weights. Time (in years) relative to the year of reversal runs on the horizontal axis.

FIGURE 5: DEMOCRATIZATION AND REVERSAL WAVES.



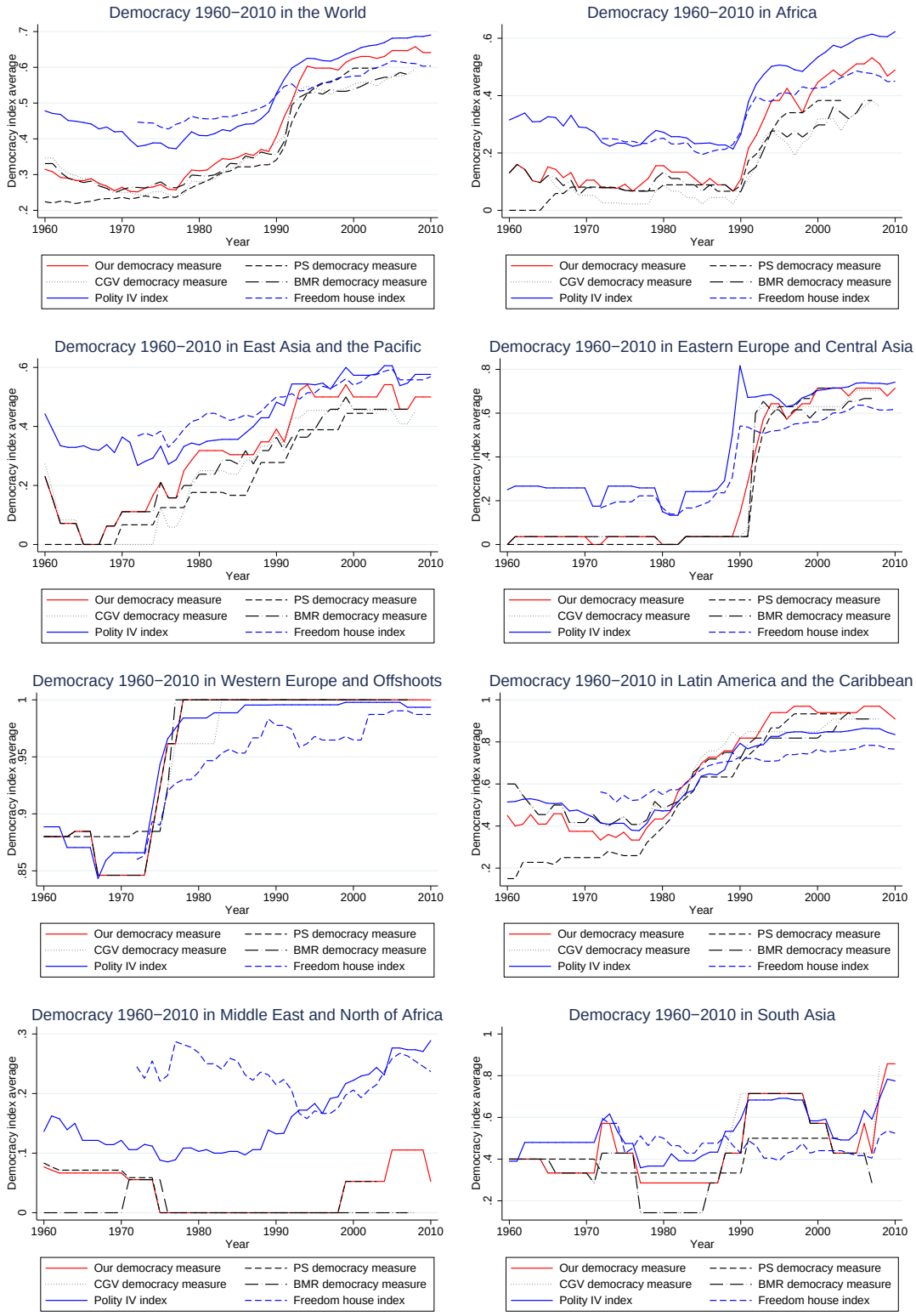
Notes: The top figure plots average democracy among initial nondemocracies in a region around the first democratization in the same region. For comparison it also plots average democracy among other initial nondemocracies in other regions. The bottom figure plots average democracy among initial democracies in a region around the first reversal in the same region. For comparison it also plots average democracy among other initial democracies in other regions.

FIGURE 6: SEMI-PARAMETRIC ESTIMATES OF THE EFFECT OF A DEMOCRATIZATION ON POTENTIAL MECHANISMS.



Notes: The figures plot estimates of the effect of democratizations on different potential mechanisms specified in the titles. The estimates report the change in the outcome relative to a continuing nondemocracy, normalizing the difference in the year before the event to zero. The estimates are obtained after reweighting the data using an inverse propensity score weighting. The reweighting controls semi-parametrically for lags of the outcome variable and lags of GDP per capita as explained in Section 5. All figures use the finite sample correction in the estimation of the weights. Time (in years) relative to the year of democratization runs on the horizontal axis.

FIGURE A1: DEMOCRACY ACROSS REGIONS



Notes: The figures present the average of our dichotomous democracy index in each of the seven geograohic regions used in the paper and defined by the World Bank. It also presents the average for the whole sample of countries. For comparison we 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's (2012) democracy measures.

TABLE 1: SUMMARY STATISTICS

	Non-Democracies			Democracies		
Variable	*Obs.*	*Mean*	*Std. Dev.*	*Obs.*	*Mean*	*Std. Dev*
GDP per capita	3376	2074.46	3838.65	3558	8149.97	9334.83
Investment share of GDP	3225	0.2182	0.1023	3340	0.2328	0.0741
TFP	1863	1.0676	0.4056	2744	0.9345	0.1646
Trade share of GDP	3175	0.7162	0.5106	3485	0.7715	0.4104
Primary Enrollment rate	2861	90.29	29.51	2823	101.60	15.86
Secondary Enrollment rate	2424	45.76	31.77	2538	75.40	29.78
Tax Revenue share of GDP	3122	0.1587	0.0948	2564	0.2075	0.0955
Child Mortality Per 1000 births	4142	77.29	49.64	3615	33.26	32.65
Unrest dummy	3739	0.2870	0.4524	3610	0.2191	0.4137
Market Reforms index (0-100)	3476	21.89	23.26	2829	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 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	0.788	0.585	1.181	1.701
	(0.294)	(0.248)	(0.226)	(0.245)	(0.477)	(0.417)	(0.374)	(0.378)	(0.466)	(0.355)	(0.355)	(0.316)
log GDP first lag	0.973	1.266	1.238	1.233	0.946	1.216	1.204	1.204	0.937	1.154	1.146	1.151
	(0.006)	(0.038)	(0.038)	(0.039)	(0.009)	(0.041)	(0.041)	(0.038)	(0.009)	(0.045)	(0.047)	(0.038)
log GDP second lag		-0.300	-0.207	-0.214		-0.270	-0.193	-0.205		-0.214	-0.126	-0.120
		(0.037)	(0.046)	(0.043)		(0.038)	(0.045)	(0.042)		(0.042)	(0.055)	(0.042)
log GDP third lag			-0.026	-0.021			-0.028	-0.020			-0.029	-0.039
			(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	-0.027
			(0.017)	(0.034)			(0.020)	(0.033)			(0.017)	(0.029)
p-value lags 5 to 8				[0.565]				[0.478]				[0.082]
GDP persistence	0.973	0.967	0.963	0.960	0.946	0.946	0.947	0.944	0.937	0.940	0.952	0.951
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
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]								
Long-run effect	35.59	19.60	21.24	22.01	17.61	14.88	16.45	11.81	12.61	9.82	24.51	34.73
p-value	[0.011]	[0.023]	[0.003]	[0.004]	[0.097]	[0.104]	[0.051]	[0.131]	[0.100]	[0.108]	[0.005]	[0.000]
AR2 test p-value					0.01	0.08	0.51	0.95				
Observations	6790	6642	6336	5688	6615	6467	6161	5513	6615	6467	6161	5513
Countries in sample	175	175	175	175	175	175	175	175	175	175	175	175

Notes: The 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. 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 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 AR2 row reports the p-value for a test of serial correlation in the residuals.

TABLE 3: EFFECT OF DEMOCRACY ON GDP PER CAPITA (ROBUSTNESS).

	Panel A: Within estimates					
Country controls:	(1)	GDP in 1960 quintiles × year effects (2)	Soviet dummies (3)	Unrest (4)	Trade (5)	Region × regime × year effects (6)
Democracy	0.787	0.718	0.911	0.705	0.595	0.834
	(0.226)	(0.249)	(0.251)	(0.224)	(0.264)	(0.264)
GDP persistence	0.963	0.968	0.963	0.959	0.959	0.950
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Unit root test adjusted $t-$stat	-4.127	-5.075	-3.643	-4.847	-4.043	-4.092
p-value (rejects unit root)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	21.24	22.17	24.86	17.00	14.59	16.65
p-value	[0.003]	[0.011]	[0.001]	[0.004]	[0.040]	[0.003]
Observations	6336	5523	6336	5643	5750	6336
Countries in sample	175	149	175	171	172	175
	Panel B: Arellano and Bond estimates					
Covariates:	(1)	GDP in 1960 quintiles × year effects (2)	Soviet dummies (3)	Unrest (4)	Trade (5)	Region × regime × year effects (6)
Democracy	0.875	0.730	1.073	0.693	1.034	1.217
	(0.374)	(0.387)	(0.403)	(0.396)	(0.469)	(0.420)
GDP persistence	0.947	0.951	0.946	0.930	0.942	0.933
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	16.45	14.86	20.01	9.87	17.93	18.21
p-value	[0.051]	[0.099]	[0.026]	[0.128]	[0.047]	[0.007]
AR2 test p-value	0.51	0.90	0.28	0.62	0.72	0.70
Observations	6161	5374	6161	5467	5570	6161
Countries in sample	175	149	175	171	172	175
	Panel C: HHK estimates					
Covariatess:	(1)	GDP in 1960 quintiles × year effects (2)	Soviet dummies (3)	Unrest (4)	Trade (5)	Region × regime × year effects (6)
Democracy	1.181	0.693	1.069	1.214	1.091	1.448
	(0.355)	(0.367)	(0.353)	(0.385)	(0.345)	(0.488)
GDP persistence	0.952	0.953	0.950	0.951	0.954	0.938
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	24.51	14.90	21.47	24.90	23.79	23.37
p-value	[0.005]	[0.079]	[0.008]	[0.009]	[0.014]	[0.005]
Observations	6161	5374	6161	5414	5578	6161
Countries in sample	175	149	175	171	172	175

Notes: The 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 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.

TABLE 4: SEMI-PARAMETRIC ESTIMATES OF THE EFFECT OF DEMOCRATIZATIONS AND REVERSALS ON GDP PER CAPITA.

	Panel A: Transition to democracy		
	Five years before	Five years after	25-30 years after
GDP per capita log points (relative to year before event)	1.74	1.43	15.06
p-value	[0.117]	[0.235]	[0.060]
	Panel B: Transition to democracy with finite sample correction		
	Five years before	Five years after	25-30 years after
GDP per capita log points (relative to year before event)	0.03	2.14	17.58
p-value	[0.905]	[0.058]	[0.013]
	Panel C: Transition to autocracy		
	Five years before	Five years after	25-30 years after
GDP per capita log points (relative to year before event)	2.23	-4.12	-24.21
p-value	[0.224]	[0.027]	[0.000]
	Panel D: Transition to autocracy with finite sample correction		
	Five years before	Five years after	25-30 years after
GDP per capita log points (relative to year before event)	-0.02	-2.12	-4.05
p-value	[0.959]	[0.111]	[0.439]

Notes: The table presents semi-parametric estimates of the short run (five years after), and long run (25-30 years after) effects of democracy on log GDP per capita, as well as estimates for pretrends in GDP (five years before). The estimator follows Angrist, Jordà and Kuersteiner (2013) and is described in the text. Panel A presents estimates of a transition from nondemocracy to democracy. Panel B presents estimates of a transition from nondemocracy to democracy using a finite sample correction in the estimation of the weights as described in the text. Panel C presents estimates of a transition from democracy to nondemocracy. Panel D presents estimates of a transition from democracy to nondemocracy using a finite sample correction in the estimation of the weights as described in the text. Below each estimate we report the $p-$value for a test of it being different from zero. The p-values are obtained via bootstrapping.

TABLE 5: EFFECT OF DEMOCRACY ON GDP PER CAPITA (INSTRUMENTAL VARIABLES).

	Within	*Panel A: 2SLS estimates*							
Covariates:				GDP in 1960 quintiles× year effects	Soviet dummies	Regional GDP	Regional Unrest	Regional Trade	Region Trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Democracy	0.787	0.966	1.149	1.125	1.292	2.570	1.272	0.955	1.697
	(0.226)	(0.558)	(0.554)	(0.689)	(0.651)	(0.762)	(0.597)	(0.576)	(0.885)
GDP persistence	0.96	0.96	0.96	0.97	0.96	0.96	0.96	0.96	0.95
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	21.24	26.31	31.52	35.23	35.72	59.36	31.88	23.22	36.79
p-value	[0.003]	[0.123]	[0.070]	[0.140]	[0.074]	[0.005]	[0.059]	[0.130]	[0.075]
Hansen p-value			0.21	0.18	0.32	0.19	0.62	0.21	0.28
Observations	6336	6312	6309	5496	6309	6309	6309	6309	6309
Countries in sample	175	174	174	148	174	174	174	174	174
Exc. Instruments F-stat.		119.1	33.2	16.8	26.7	29.6	33.1	33.2	23.7
		Panel B: First-stage regression:							
Democracy wave t-1		0.800	0.547	0.503	0.480	0.537	0.530	0.543	0.498
		(0.073)	(0.101)	(0.130)	(0.099)	(0.100)	(0.098)	(0.102)	(0.092)
Democracy wave t-2			0.133	0.109	0.133	0.133	0.128	0.123	0.129
			(0.081)	(0.094)	(0.080)	(0.079)	(0.081)	(0.081)	(0.081)
Democracy wave t-3			0.227	0.270	0.223	0.223	0.228	0.232	0.228
			(0.067)	(0.077)	(0.065)	(0.069)	(0.067)	(0.068)	(0.070)
Democracy wave t-4			-0.087	-0.119	-0.075	-0.091	-0.067	-0.084	-0.123
			(0.110)	(0.126)	(0.110)	(0.110)	(0.110)	(0.113)	(0.106)
		Panel C: HHK estimates:							
	Base	External democracy instruments							
Covariates:				GDP in 1960 quintiles× year effects	Soviet dummies	Regional GDP	Regional Unrest	Regional Trade	Region Trends
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Democracy	1.181	0.550	0.865	1.339	0.656	1.347	1.112	0.974	1.018
	(0.355)	(0.658)	(0.503)	(0.568)	(0.540)	(0.476)	(0.486)	(0.499)	(0.486)
GDP persistence	0.952	0.951	0.961	0.970	0.960	0.943	0.962	0.963	0.952
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	24.51	11.27	22.37	45.06	16.59	23.82	28.90	26.08	21.18
p-value	[0.005]	[0.425]	[0.130]	[0.070]	[0.253]	[0.011]	[0.047]	[0.080]	[0.062]
Observations	6161	6110	6110	5374	6110	6110	6110	6080	6110
Countries in sample	175	174	174	148	174	174	174	174	174

Notes: The 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 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. 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. Column 1 in Panels A and C present the baseline estimates without instrumenting democracy. Column 2 presents estimates instrumenting democracy with the first lag of regional democracy. In the remaining columns democracy is instrumented using four lags of regional democracy. 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.

TABLE 6: EFFECTS OF DEMOCRACY ON POTENTIAL MECHANISMS.

Mechanism:	Investment share in GDP (1)	TFP (2)	Economic reforms (3)	Trade share in GDP (4)	Tax revenue share in GDP (5)	Primary enrollment (6)	Secondary enrollment (7)	Child mortality (8)	Riots and revolts (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)
Outcome persistence	0.74	0.93	0.88	0.87	0.79	0.95	0.93	0.99	0.34
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	9.11	-2.88	5.58	5.45	16.06	21.91	18.96	-34.26	-11.94
p-value	[0.032]	[0.455]	[0.053]	[0.300]	[0.016]	[0.004]	[0.028]	[0.001]	[0.000]
Observations	5665	3879	4692	5738	4511	3714	2883	6084	5646
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	8.44	-12.74	23.77	40.59	38.61	36.69	57.07	-95.73	-8.47
p-value	[0.430]	[0.150]	[0.000]	[0.003]	[0.007]	[0.018]	[0.009]	[0.000]	[0.323]
Outcome persistence	0.74	0.93	0.86	0.86	0.79	0.95	0.93	0.99	0.34
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
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	5640	3871	4670	5714	4489	3710	2879	6057	5619
Countries in sample	168	107	149	171	130	164	156	172	170
	Panel C: HHK estimates								
Democracy	3.500	0.232	2.947	2.583	2.929	0.756	-0.125	-0.700	-13.968
	(2.078)	(0.546)	(1.091)	(1.191)	(2.904)	(0.526)	(0.885)	(0.122)	(4.761)
Long-run effect	13.92	3.29	19.86	26.07	12.00	28.52	-3.63	-198.64	-23.95
p-value	[0.098]	[0.672]	[0.140]	[0.033]	[0.301]	[0.153]	[0.888]	[0.050]	[0.004]
Outcome persistence	0.75	0.93	0.85	0.90	0.76	0.97	0.97	1.00	0.42
p-value (test < 1)	[0.000]	[0.000]	[0.119]	[0.000]	[0.000]	[0.000]	[0.002]	[0.045]	[0.000]
Observations	5125	3557	4236	4866	4045	3579	2683	5454	5233
Countries in sample	168	107	149	171	130	164	156	172	170

Notes: The 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. 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 a 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. We report the estimated persistence of the outcome 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 7: HETEROGENEOUS EFFECTS OF DEMOCRACY ON 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	16.23	18.63	20.49	19.84	13.79	10.48	11.84	14.60
p-value	[0.146]	[0.040]	[0.017]	[0.016]	[0.107]	[0.205]	[0.145]	[0.083]
Observations	4281	4909	5525	6336	5300	5300	5300	5300
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	18.84	4.98	19.28	36.12	-3.65	-14.59	-14.14	17.37
p-value	[0.665]	[0.874]	[0.523]	[0.227]	[0.855]	[0.443]	[0.435]	[0.351]
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	4273	4901	5517	6153	5292	5292	5292	5218
Countries in sample	93	109	131	174	138	138	138	138
	Panel C: HHK estimates:							
Democracy	0.299	0.451	0.411	1.647	1.157	0.916	1.001	1.915
	(0.450)	(0.452)	(0.382)	(0.515)	(0.584)	(0.570)	(0.565)	(0.499)
Interaction	0.003	-0.002	0.001	0.004	0.094	0.101	0.056	0.010
	(0.003)	(0.003)	(0.003)	(0.004)	(0.041)	(0.036)	(0.028)	(0.012)
Long-run effect	11.99	17.87	14.83	47.70	38.92	29.01	33.29	60.85
p-value	[0.535]	[0.349]	[0.338]	[0.033]	[0.075]	[0.140]	[0.118]	[0.016]
Observations	4180	4792	5386	6110	5154	5154	5154	5154
Countries in sample	93	109	131	174	138	138	138	138

Notes: The table presents estimates of the effect of democracy interacted with other country characteristics on growth. The column labels specify the variable interacted with democracy in each model. The reported coefficients of democracy and the interaction are multiplied by 100. Main effects and the long-run effects are 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 and their interactions with the interacted variable. It also reports the F statistic for the excluded instruments and the p-value of a Hansen's overidentification test. Panel C presents results using the HHK estimator instrumenting democracy with four lags of regional democracy waves and their interactions with 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. 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 A1: 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: The 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. The estimation of the propensity score is fully explained in section 5. n.a. indicates that the propensity score is not available because of the availability of sufficient GDP data. The overall sample probability of a democratization following a period of nondemocracy is 0.0184.

TABLE A2: 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: The 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. The estimation of the propensity score is fully explained in section 5. n.a. indicates that the propensity score is not available because of the availability of sufficient GDP data. The overall sample probability of a reversal following a period of democracy is 0.0121.

TABLE A3: EFFECT OF LAGS OF GDP PER CAPITA ON DEMOCRACY.

GDP lags:	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	6347	5699	5031	4359	3692
Countries in sample	175	175	173	170	165

Notes: The table reports within estimates of the effect of lagged GDP per capita on democracy. In each column we add a different number of lags of GDP as specified in the columns labels. The table only reports the coefficients of the first four lags. Below each model we report the p-value for a test of joint significance of the first four lags, and the p-value of the additional lags. In all specifications we include a full set of country and year fixed effects. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

TABLE A4: CORRELATION BETWEEN ESTIMATED RESIDUALS IN EQUATION (1) AND LAGGED DEMOCRACY

	(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)
P-value lags of democracy	[0.755]	[0.487]	[0.587]	[0.814]	[0.372]
Observations	6315	6292	6265	6234	6234

Notes: The table reports estimates of the lagged democracy on the estimated residual in equation (1). The shock in the GDP equation is estimated after partialling out democracy and four lags of GDP. In each column we add different lags of democracy and report their joint p-value at the bottom of each model. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

TABLE A5: EFFECTS OF DEMOCRACY ON GDP PER CAPITA (CONTROLLING FOR 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)
GDP persistence	0.963	0.971	0.973	0.978	0.974
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	21.24	19.42	21.98	18.09	19.00
p-value	[0.003]	[0.006]	[0.003]	[0.010]	[0.006]
Observations	6336	6046	6027	6160	6336
Countries in sample	175	175	175	175	175

Notes: The table presents within estimates of the effect of democracy on 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 a Cook's distance above the rule of thumb value of 4 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. 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 A6: EFFECT OF DEMOCRACY ON GDP PER CAPITA (ALTERNATIVE GMM ESTIMATES)

	Within	Arellano & Bond moments				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)
GDP persistence	0.963	0.947	0.945	0.929	0.966	0.960	0.962	0.960
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.023]	[0.000]	[0.000]	[0.000]
Long-run effect	21.24	16.45	17.93	14.53	37.56	27.93	33.32	36.39
p-value	[0.003]	[0.051]	[0.125]	[0.179]	[0.225]	[0.010]	[0.052]	[0.070]
AR2 test p-value		0.51	0.45	0.53	0.32	0.46	0.38	0.39
Moments		2509	1266	941	231	2555	1312	987
Observations	6336	6161	6161	6161	6161	6161	6161	6161
Countries in sample	175	175	175	175	175	175	175	175

Notes: The table presents different GMM estimates of the effect of democracy on 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. 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 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.

TABLE A7: ESTIMATES OF THE EFFECT OF DEMOCRACY ON GDP PER CAPITA IMPOSING DIFFERENT PERISTENCE LEVELS.

	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 (after 30 years)	10.01	13.28	17.32	22.32	28.56	36.37
p-value	[0.010]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]
Observations	6336	6336	6336	6336	6336	6336
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 (after 30 years)	7.59	17.19	29.23	44.42	63.65	88.07
p-value	[0.401]	[0.065]	[0.004]	[0.000]	[0.000]	[0.000]
Observations	6309	6309	6309	6309	6309	6309
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: The 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. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

TABLE A8: EFFECT OF DEMOCRACY ON GDP PER CAPITA (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	0.813	0.649	0.152	0.315	0.521
	(0.226)	(0.291)	(0.223)	(0.251)	(0.258)	(0.270)
GDP persistence	0.963	0.963	0.951	0.966	0.963	0.964
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	21.24	22.24	13.28	4.41	8.61	14.44
p-value	[0.003]	[0.010]	[0.004]	[0.555]	[0.246]	[0.068]
Observations	6336	5736	5587	5630	5994	5783
Countries	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)
GDP persistence	0.964	0.964	0.942	0.967	0.964	0.964
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	31.52	28.60	72.04	34.51	40.41	30.40
p-value	[0.070]	[0.038]	[0.018]	[0.074]	[0.092]	[0.141]
Exc. instruments F-stat.	33.21	64.26	3.87	29.80	9.22	9.27
Observations	6309	5736	5185	5577	5962	5775
Countries in sample	174	153	174	151	174	174
	Panel C: Within estimates without GDP dynamics.					
Democracy	-10.112	-8.388	5.468	-11.017	-7.106	-4.214
	(4.316)	(6.700)	(3.151)	(3.988)	(4.715)	(4.485)
Observations	6934	6328	5840	6179	6588	6372
Countries in sample	175	153	174	154	175	174

Notes: The 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. 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 A9: EFFECT OF COMPONENTS OF DEMOCRACY ON GDP PER CAPITA.

		Polity components				FH components		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democratizations	0.803							
	(0.235)							
Reversals	-0.705							
	(0.335)							
Constraints on the executive		-0.243			-0.542			
		(0.261)			(0.406)			
Executive recruitment			0.132		0.424			
			(0.214)		(0.353)			
Competitiveness of participation				0.361	0.379			
				(0.318)	(0.333)			
Political rights						0.323		0.077
						(0.219)		(0.264)
Civil Liberties							0.497	0.449
							(0.245)	(0.297)
GDP persistence	0.963	0.963	0.963	0.963	0.962	0.952	0.951	0.952
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	21.77	-6.50	3.58	9.68	6.89	6.70	10.24	10.86
p-value	[0.004]	[0.340]	[0.533]	[0.240]	[0.456]	[0.153]	[0.048]	[0.053]
Observations	6336	5487	5487	5487	5487	5585	5585	5585
Countries in sample	175	153	153	153	153	174	174	174

Notes: The table presents within estimates of the effect of different components of democracy on GDP per capita. The coefficients of the component are multiplied by 100. Column 1 presents separately the effect of a democratization and a reversal. Columns 2-5 present estimates of the effect of the different components reported by Polity. Columns 6-8 present estimates of the effect of the different components reported by Freedom House. The components and their construction are described in the appendix. 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 (computed as the sum of all components) and the $p-$value for this being different from 0.

TABLE A10: EFFECT OF DEMOCRACY ON GDP PER CAPITA (INSTRUMENTAL VARIABLES 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)	(0.257)	(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)
GDP persistence	0.964	0.972	0.974	0.979	0.970	0.975	0.978	0.975
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	31.52	30.74	31.23	39.70	36.86	28.68	23.53	33.76
p-value	[0.070]	[0.069]	[0.104]	[0.052]	[0.059]	[0.112]	[0.059]	[0.041]
Observations	6309	6015	6000	6133	5967	5612	6309	6309
Countries in sample	174	174	174	174	174	173	174	174

Notes: The 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 a 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 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 a a 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 A11: EFFECT OF DEMOCRACY ON GDP PER CAPITA (INSTRUMENTAL VARIABLES WITH ALTERNATIVE 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)
GDP persistence	0.964	0.964	0.964	0.965	0.963	0.963	0.963	0.963
p-value (test < 1)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Long-run effect	31.52	44.57	46.12	56.72	23.03	26.93	28.03	25.65
p-value	[0.070]	[0.050]	[0.018]	[0.079]	[0.147]	[0.143]	[0.105]	[0.118]
Exc. instruments F-stat.	33.2	12.3	45.1	8.3	965.4	130.6	513.6	303700.1
Observations	6309	6270	6330	5906	6309	6270	6330	5906
Countries in sample	174	173	175	164	174	173	175	164

Notes: The 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 A12: HETEROGENEOUS EFFECT OF DEMOCRACY ON 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	17.73	16.56	16.49	13.48	16.53	15.75	16.62	20.04
p-value	[0.062]	[0.087]	[0.076]	[0.121]	[0.054]	[0.066]	[0.061]	[0.027]
Observations	5300	5300	5300	5300	5300	5300	5300	5300
Countries in sample	138	138	138	138	138	138	138	138

Notes: The table presents within estimates of the effect of democracy interacted with other country characteristics on growth. The columns label on each panel specify the variable interacted with democracy in each model. The reported coefficients of democracy and the interaction are multiplied by 100 to ease their interpretation. Main effects and lon-run effects are evaluated at the 25th percentile of the interacted variable. Interactions in columns 4 and 8 are done with the closest available observation before each year (education data is available every five years). 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.