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ONLINE APPENDIX FOR "DEMOCRACY DOES CAUSE GROWTH."

A1 Detailed Construction of our Democracy Measure

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

A1.1 Comparison to Previous Measures of Democracy

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

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

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

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

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

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

A1.2 Components of Democracy

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

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

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

A2 Alternative GDP measures

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

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

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

A3 Alternative Estimates of the Long-Run Impact of Democracy

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

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

fact that the country would have democratized with some low probability in any case even if it failed to democratize in a given year.

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

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

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

To do so, we rearrange equation (1) as

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

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

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

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

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

A5 Monte Carlo Simulations

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

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

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

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

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

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

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

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

Three messages emerge from these results. First, the within-group estimator and the GMM estimator slightly underestimate the persistence of the GDP process (by less than 1%), which suggest that

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

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

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

A6 Additional Tests and Checks for the Dynamic Panel Model Estimates

A6.1 Robustness to Outliers

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

The results in Table A8 show that our within estimates are not driven by outliers. Remarkably, the

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

A6.2 Additional GMM Estimates

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

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

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

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

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

A6.3 Separating the Effect of Democratizations and Reversals

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

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

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

To do so, we let

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

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

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

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

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

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

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

A7 Additional Checks for our Semi-Parametric Estimates

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

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

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

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

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

with Φ the cumulative normal distribution.

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

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

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

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

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

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

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

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

A8 Two Illustrative Examples of Democratization

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

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

Similarly, in South Korea democracy was by no means a foregone conclusion, as reflected in the

estimated propensity score of 0.02 (see again Table A1). The dictatorship's succession announcement on June 10, 1987 triggered large student protests. Nevertheless, large and even more daring pro-democracy protests had been decisively repressed earlier in the decade, notably in the Gwangju uprising of 1980. Repression was eschewed by the government this time, in part because of world image concerns in anticipation of the 1988 Olympics, and the regime acquiesced to holding elections (see Cumings, 1997).

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

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

A9 Additional Checks and Material for the IV Estimates

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

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

The top panel of Table A12 presents our results. In particular, it presents estimates obtained by regressing own country democracy on its own lag, a lagged jackknifed average of democracy in its region × initial regime cell (lagged regional democracy for simplicity), average democracy in other countries weighed by the inverse of their distance, and average democracy on neighboring countries. All

these models include a full set of country and year fixed effects. Our findings suggest that innovations to democracy are highly correlated with lagged regional democracy, but not so much with distance-based averages of democracy or neighbors' democracy. When we include all these variables together, lagged regional democracy explains the bulk of the variation in the innovation, while distance-based measures of democracy have small and insignificant effects. Panel B shows that the same holds for unrest, which we view as a proxy for political discontent. Finally, in Panel C we do not find evidence of strong regional correlation or distance-based correlation in GDP shocks.

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

A9.2 Robustness to Outliers (IV Estimates)

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

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

procedure. Finally, column 8 presents results using a Huber $M-$estimator at each stage. We find similar long-run effects of democracy on growth, except in column 7. The evidence suggests that outliers have little effect on our IV estimates.

A9.3 Alternative Construction of Regional Instruments

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

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

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

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

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

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

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

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

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

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

A10 Appendix: Additional Heterogeneous Effects

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

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Table A1: Transitions to democracy in our sample.

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

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

Table A2: Reversals to nondemocracy in our sample.

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

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

TABLE A3: EMPIRICAL PROCESS FOR GDP AND DEMOCRACY USED IN THE ALTERNATIVE COUNTERFACTUAL FOR OUR LONG-RUN ESTIMATES.

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

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

TABLE A4: EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA. ESTIMATES OBTAINED BY IMPOSING THE PERSISTENCE OF THE GDP PROCESS TO LIE BETWEEN 0.95 AND 1.

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

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

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

Assumed persistence of GDP:	Within estimator				GMM estimator			
	$\rho = 0.963$ (1)	$\rho = 0.97$ (2)	$\rho = 0.98$ (3)	$\rho = 0.99$ (4)	$\rho = 0.963$ (5)	$\rho = 0.97$ (6)	$\rho = 0.98$ (7)	$\rho = 0.99$ (8)
	Panel A: Assuming non-stationary initial conditions.							
Average persistence of GDP	0.9558	0.9637	0.9750	0.9864	0.9586	0.9662	0.9771	0.9881
(standard deviation)	(0.0016)	(0.0014)	(0.0012)	(0.0009)	(0.0019)	(0.0017)	(0.0014)	(0.0010)
Relative bias (Nickell bias):	-0.75%	-0.65%	-0.51%	-0.36%	-0.46%	-0.39%	-0.29%	-0.19%
Average coefficient of democracy	0.7925	0.7924	0.7923	0.7922	0.7889	0.7888	0.7886	0.7884
(standard deviation)	(0.2257)	(0.2258)	(0.2258)	(0.2258)	(0.2355)	(0.2355)	(0.2355)	(0.2355)
Average long-run effect of democracy	18.004	21.951	31.900	59.066	19.141	23.489	34.714	67.082
(standard deviation)	(5.266)	(6.460)	(9.533)	(18.438)	(5.908)	(7.301)	(10.991)	(22.476)
	Panel B: Assuming stationary initial conditions.							
Average persistence of GDP	0.9171	0.9238	0.9334	0.9485	0.9129	0.9175	0.9247	0.9539
(standard deviation)	(0.0049)	(0.0048)	(0.0045)	(0.0042)	(0.0096)	(0.0096)	(0.0095)	(0.0075)
Relative bias (Nickell bias):	-4.7%	-4.8%	-4.8%	-4.2%	-5.2%	-5.4%	-5.6%	-3.6%
Average coefficient of democracy	0.7757	0.7741	0.7712	0.7706	0.7499	0.7455	0.7394	0.7558
(standard deviation)	(0.2283)	(0.2281)	(0.2277)	(0.2133)	(0.2386)	(0.2383)	(0.2376)	(0.2376)
Average long-run effect of democracy	9.340	10.209	11.641	15.069	8.744	9.189	10.013	16.971
(standard deviation)	(2.862)	(3.123)	(3.586)	(4.404)	(3.032)	(3.226)	(3.587)	(6.438)

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

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

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

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

TABLE A7: EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA. ESTIMATES OBTAINED WITH ALTERNATIVE MEASURES OF GDP.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

TABLE A12: SPATIAL PATTERNS OF DIFFUSION FOR DEMOCRACY, UNREST, AND GDP PER CAPITA.

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

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

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

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

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

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

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

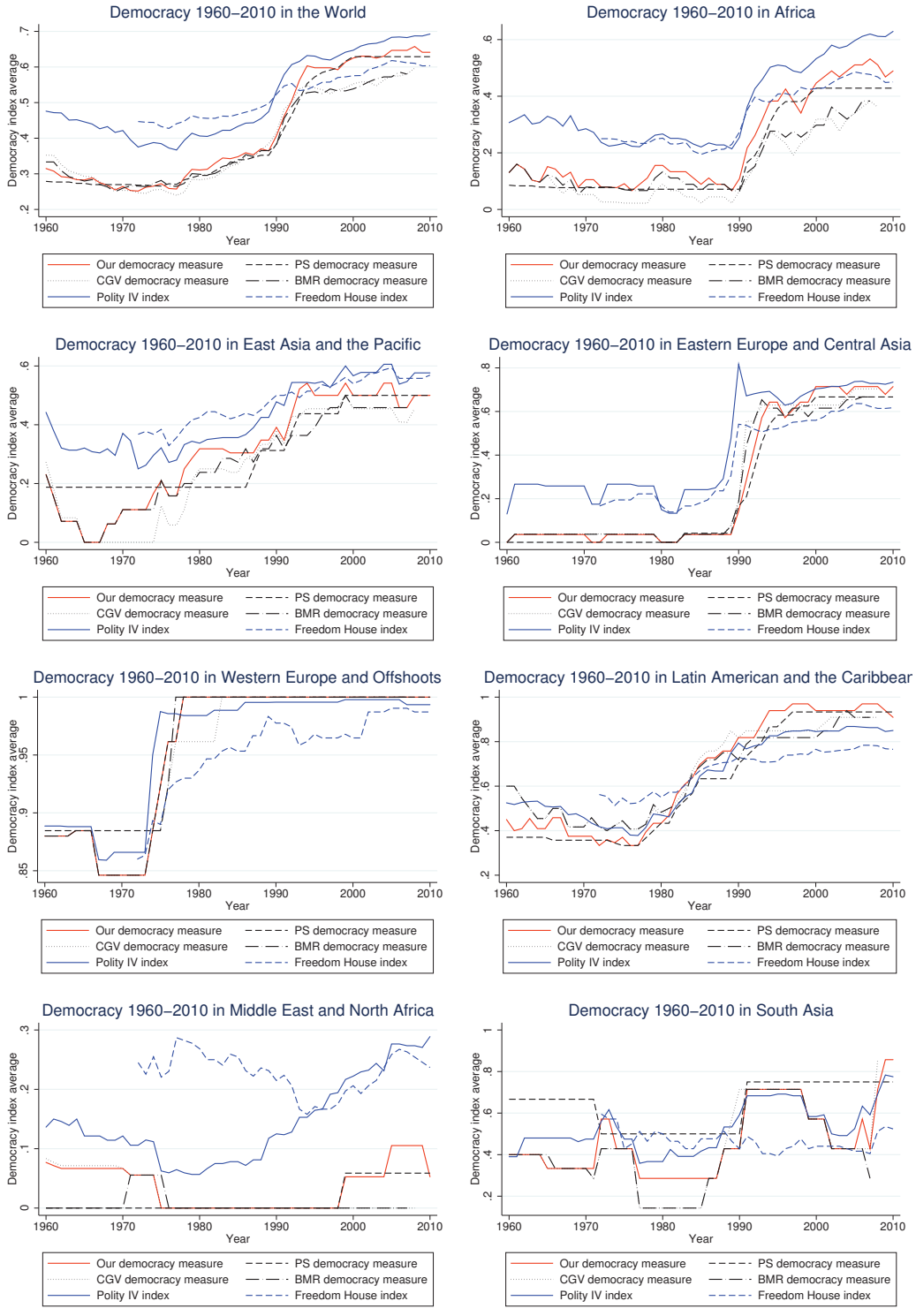
Notes: This table presents 2SLS estimates of the effect of democracy in GDP per capita using alternative constructions of the regional democracy instrument. The coefficient on democracy is multiplied by 100. In all models we instrument democracy using four lags of the alternative instruments. In columns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

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

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

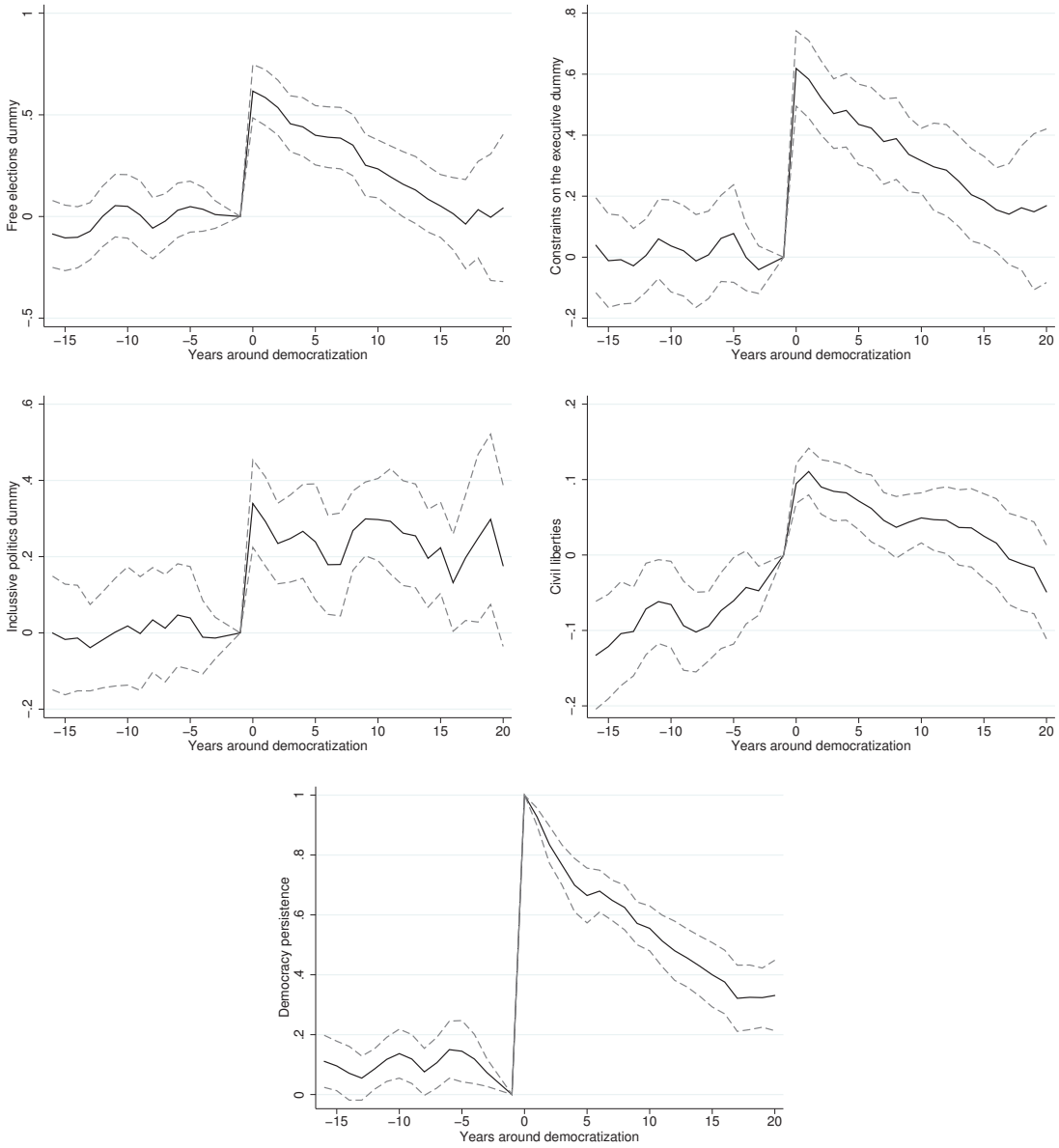
Notes: This table presents within estimates of the effect of democracy on log GDP per capita and its interaction with other country characteristics. The column labels specify the variable interacted with democracy in each model. The reported coefficients on democracy and the interaction are multiplied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

FIGURE A1: DIFFERENT MEASURES OF DEMOCRACY AVERAGED ACROSS REGIONS AND WORLDWIDE.



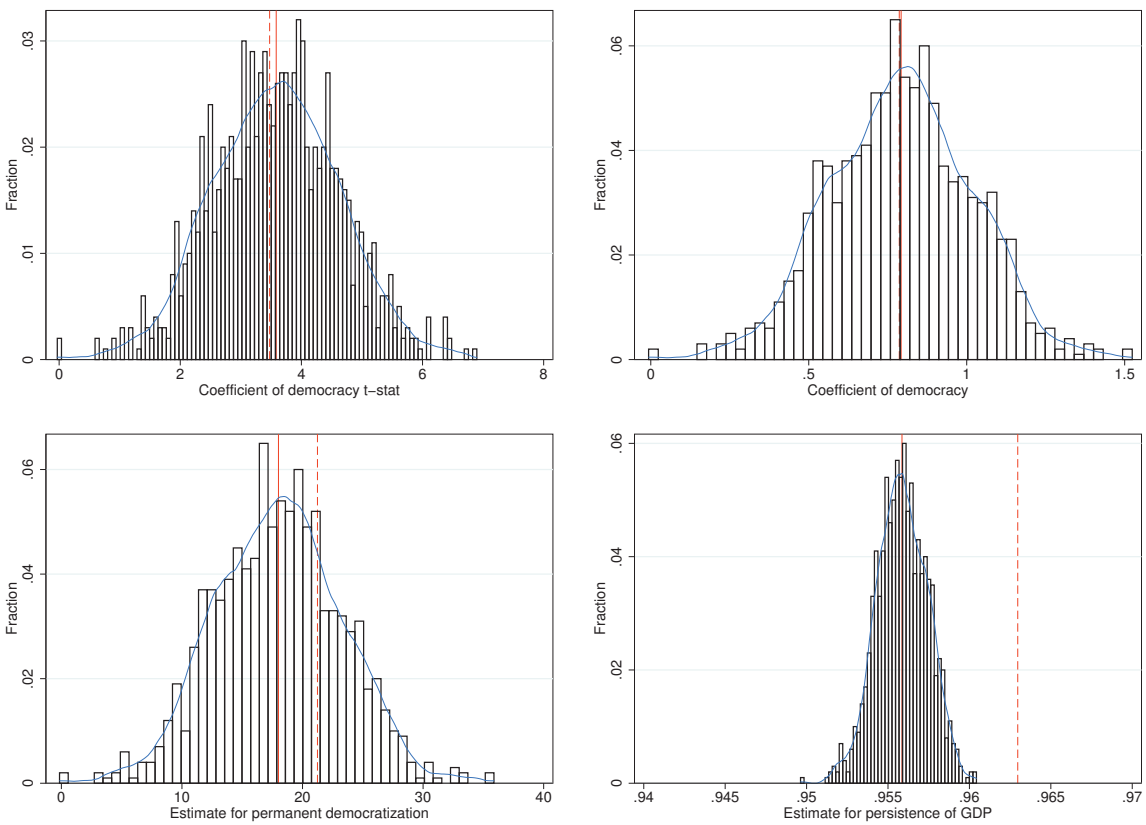
Notes: The figures present the evolution over time of the several democracy measures for each of the seven regions used in the paper, as well as for the whole world.

FIGURE A2: INSTITUTIONAL CHANGES THAT FOLLOW AN EPISODE OF DEMOCRATIZATION.



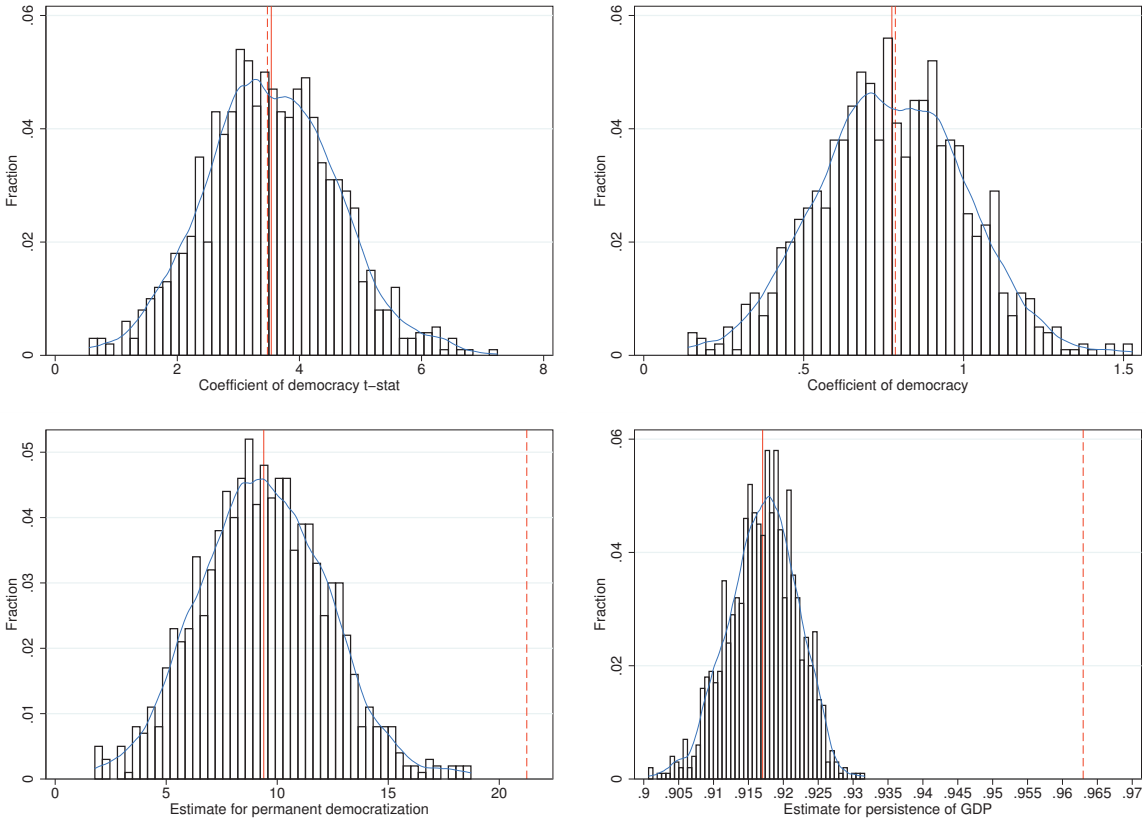
Notes: The figures plots the behavior of different components of democracy around a democratization (relative to continuing nondemocracies). Time (in years) relative to the year of democratization runs on the horizontal axis. See the text for a detailed explanation of how we measure these components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

FIGURE A3: RESULTS OF OUR MONTE CARLO SIMULATIONS, NON-STATIONARY INITIAL CONDITIONS.



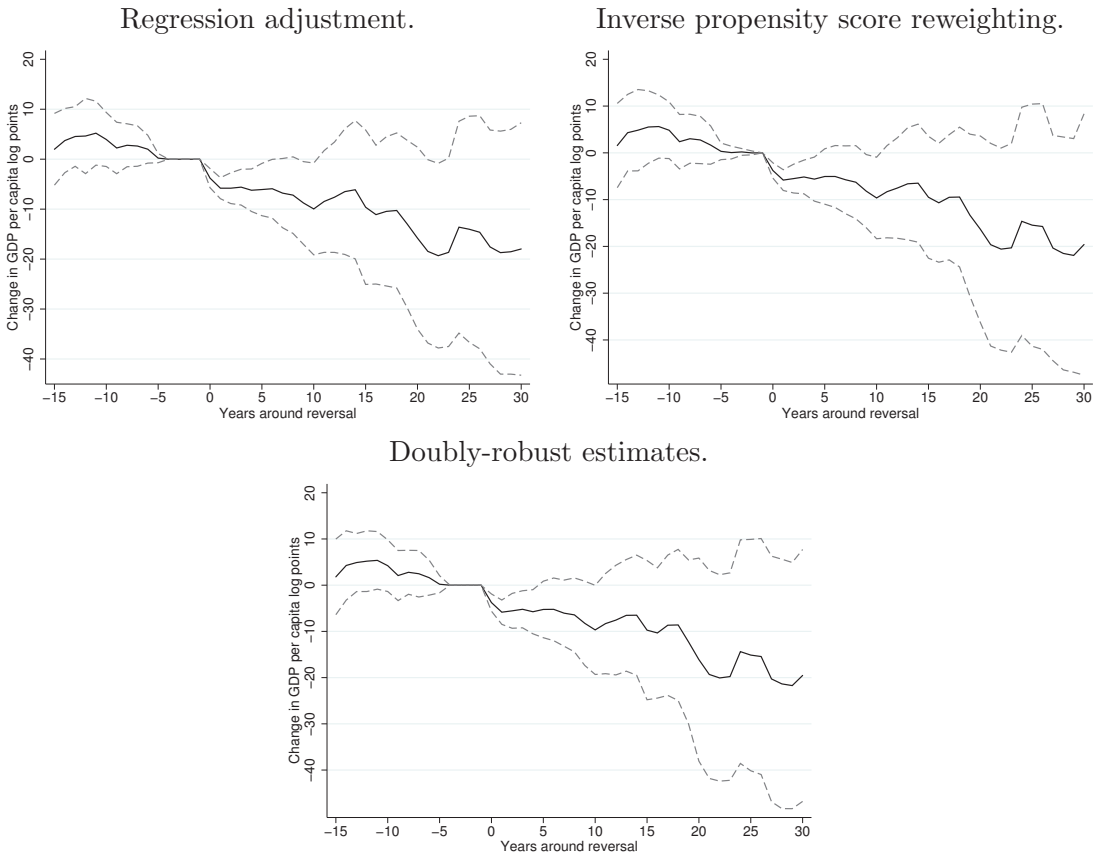
Notes: These figures plot the histograms and smoothed densities that we obtained for several estimates in our Monte Carlo simulation. The dashed red line indicates the population paremeters, and the solid red line indicates the average estimate over 1,000 simulations. In this case, the simulations assume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

FIGURE A4: RESULTS OF OUR MONTE CARLO SIMULATIONS, STATIONARY INITIAL CONDITIONS.



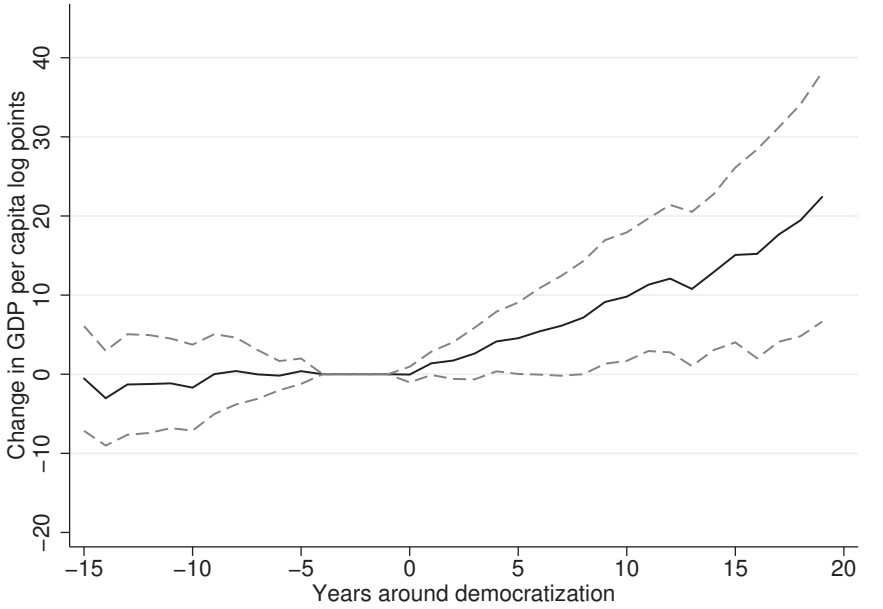
Notes: These figures plot the histograms and smoothed densities that we obtained for several estimates in our Monte Carlo simulation. The dashed red line indicates the population paremeters, and the solid red line indicates the average estimate over 1,000 simulations. In this case, the simulations assume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

FIGURE A5: SEMI-PARAMETRIC ESTIMATES OF THE OVER-TIME EFFECTS OF A REVERSAL TO NON-DEMOCRACY ON THE LOG OF GDP.



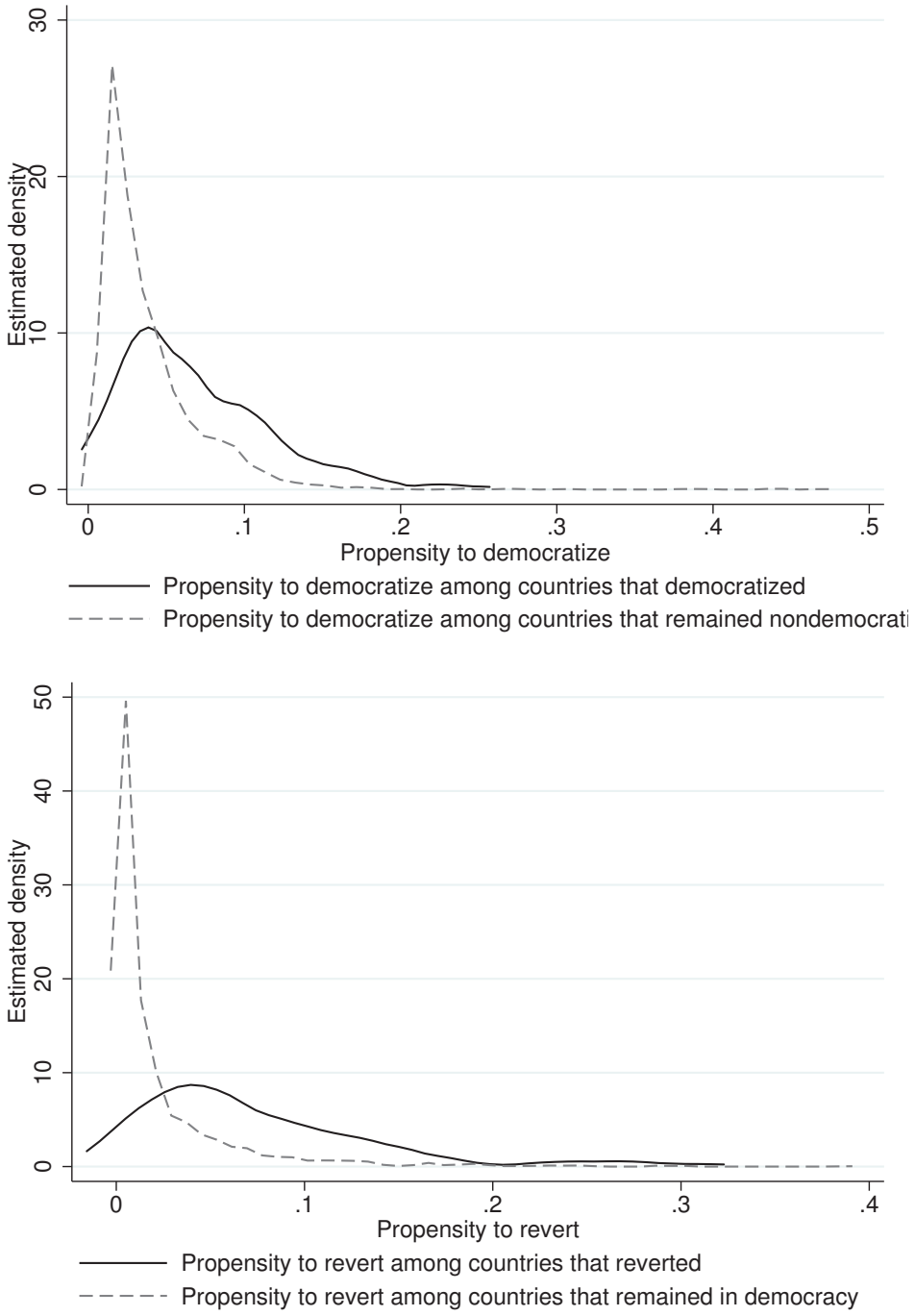
Notes: These figures plot semi-parametric estimates of the effect of a reversal to nondemocracy on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita (in log points) on countries that reverted, together with a 95% confidence interval in dashed lines. Time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

FIGURE A6: SEMI-PARAMETRIC ESTIMATES OF THE OVER-TIME EFFECTS OF A DEMOCRATIZATION ON THE LOG OF GDP. DOUBLY-ROBUST ESTIMATES FOR THE AVERAGE TREATMENT EFFECT.



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

FIGURE A7: SMOOTHED DENSITY FOR THE ESTIMATED PROPENSITY TO DEMOCRATIZE OR REVERT TO NONDEMOCRACY.



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