Democracy Does Cause Growth

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We provide evidence that democracy has a positive effect on GDP per capita. Our dynamic panel strategy controls for country fixed effects and the rich dynamics of GDP, which otherwise confound the effect of democracy. To reduce measurement error, we introduce a new indicator of democracy that consolidates previous measures. Our baseline results show that democratizations increase GDP per capita by about 20 percent in the long run. We find similar effects using a propensity score reweighting strategy as well as an instrumental-variables strategy using regional waves of democratization. The effects are similar across

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different levels of development and appear to be driven by greater investments in capital, schooling, and health.

I. Introduction

With the spectacular economic growth under nondemocracy in China, the eclipse of the Arab Spring, and the recent rise of populist politics in Europe and the United States, the view that democratic institutions are at best irrelevant and at worst a hindrance for economic growth has become increasingly popular in both academia and policy discourse. For example, the prominent *New York Times* columnist Tom Friedman (2009) argues that "one-party nondemocracy certainly has its drawbacks. But when it is led by a reasonably enlightened group of people, as China is today, it can also have great advantages. That one party can just impose the politically difficult but critically important policies needed to move a society forward in the 21st century." Robert Barro (1997, 1) states this view even more boldly: "More political rights do not have an effect on growth."

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

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

The estimation of the causal effect of democracy (or a democratization) on GDP faces several challenges. First, existing democracy indices

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

are subject to considerable measurement error, leading to spurious changes in democracy scores that do not correspond to real changes in democratic institutions.

Second, democracies differ from nondemocracies in unobserved characteristics, such as institutional, historical, and cultural aspects, that also have an impact on their GDP. As a result, cross-country regressions, as those in Barro (1996, 1999), could be biased and are unlikely to reveal the causal effect of democracy on growth. Recent studies tackle this problem by using difference-in-differences or panel data estimates with country fixed effects.

Third, as shown in figure 1, as well as in Acemoglu et al. (2005) and Brückner and Ciccone (2011), democratizations are, on average, preceded by a temporary dip in GDP. This figure depicts GDP dynamics in countries that democratized at year 0 relative to other countries that remained nondemocratic at the time. The pattern in this figure implies that failure to properly model GDP dynamics, or the propensity to democratize based on past GDP, will lead to biased estimates of democracy on GDP. Though largely overlooked in previous work, the dip in GDP that

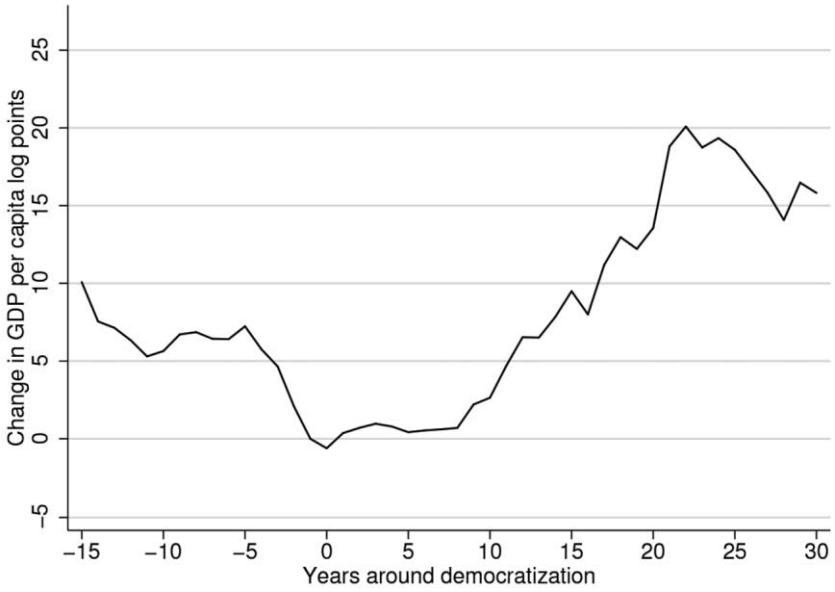

FIG. 1.—GDP per capita before and after a democratization. This figure plots GDP per capita in log points around a democratic transition relative to countries remaining nondemocratic in the same year. We normalize log GDP per capita to 0 in the year preceding the democratization. Time (in years) relative to the year of democratization runs on the horizontal axis.

precedes a democratization constitutes a clear violation of the parallel-trends assumption that underlies the difference-in-differences or panel data estimates used in the literature. Modeling GDP dynamics would also enable an investigation of whether the impact of democratization on GDP is short-lived or gradual.

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

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

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

Our second strategy adopts a semiparametric treatment effects framework in which democratization—the treatment—influences the distribution of potential GDP in all subsequent years. This strategy requires us to model the process of selection into democracy as a function of observables, in particular, lags of GDP (e.g., Jordà 2005; Angrist and Kuersteiner 2011; Kline 2011), but it does not rely on a parametric model for the dynamics of GDP, which affords us greater flexibility in estimating the time path of the impact of democracy on GDP. Related to our first approach, the economic assumption in this case is that, conditional on the lags of GDP, countries that democratize are not on a differential GDP trend relative to other nondemocracies. We show that this approach successfully controls for the influence of the dip in GDP preceding democratizations shown in figure 1 and estimates that after a democratization, GDP increases gradually until it reaches a level 20–25 percent higher than what it would reach otherwise.

These two strategies model the selection of countries into different regimes and control for the dip in GDP in figure 1 as a function of their re-

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

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

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

There is a substantial literature in political science that investigates, but does not reach a firm conclusion on, the empirical linkages between democracy and economic outcomes, summarized in part in Przeworski and Limongi (1993) and in Doucouliagos and Ulubaşoğlu's (2008) meta-analysis.

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

Persson and Tabellini (2008), too, use propensity score techniques to estimate the impact of democracy. However, they focus only on changes in the average growth rate of countries after a democratization and do not develop the semiparametric approach we use here or model the selection into democracy as a function of lags of GDP. Also related is recent independent work by Meyersson (2015), who estimates the effect of successful coups on economic growth by comparing them to unsuccessful coups.

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

[3] A smaller literature focuses on the effects of democracy on other economic outcomes. For example, Grosjean and Senik (2011), Rode and Gwartney (2012), and Giuliano, Mishra, and Spilimbergo (2013) look at the effect of democracy on economic reforms. Ansell (2010) looks at its impact on educational spending. Gerring, Thacker, and Alfaro (2012), Blaydes and Kayser (2011), Besley and Kudamatsu (2006), and Kudamatsu (2012) investigate its impact on health, infant mortality, and nutrition outcomes. Reynal-Querol (2005) and Sunde and Cervellati (2014) study 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. (2015). There is also a growing and promising literature that investigates the impact of democracy by using within-country differences in the extent of democratic and electoral institutions (see, among others, Martinez-Bravo et al. 2012; Naidu 2012; Fujiwara 2015).

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

The rest of the paper is organized as follows. The next section describes the construction of our democracy index and provides data sources and descriptive statistics for our sample. Section III presents our dynamic panel model results. This model is estimated with the standard within estimator and various generalized method of moments (GMM) estimators. This section also presents a variety of robustness checks. Section IV introduces the treatment effects framework and presents results from our semiparametric strategy. Section V presents our results obtained by instrumenting democracy with regional democratization waves. Section VI presents evidence on potential channels through which democracy affects growth. Section VII investigates heterogeneous effects of democracy depending on the level of economic development and education. Section VIII concludes the paper. We present several additional exercises in our online appendix.

II. Data and Descriptive Statistics

We construct an annual panel that comprises 175 countries from 1960 to 2010, although not all variables are available for the entire sample. In order to address the issue of measurement error in democracy indices, we create a consolidated and dichotomous measure of democracy. Following Papaioannou and Siourounis (2008), our index combines information from several data sets, including Freedom House and Polity IV, and considers a country democratic only when several sources classify it as such. In the online appendix, we explain in detail the construction of our measure; here, we provide an overview. We code our dichotomous measure of democracy in country c at time t, D_{ct}, as follows. First, we consider a country democratic during a given year if Freedom House codes it

as "free" or "partially free" and Polity IV assigns it a positive score. When one of these two sources is unavailable, we verify whether the country is also coded as democratic by Cheibub, Gandhi, and Vreeland (2010) or Boix, Miller, and Rosato (2012). (These two data sets extend the popular Przeworski et al. [2000] dichotomous measure of democracy.) Many of the democratic transitions detected in this manner are studied in detail by Papaioannou and Siourounis (2008), who use historical sources to date the exact year of the transition. When possible, we also draw on their data to verify the date of a democratization event.

Our measure of democracy covers 184 countries from 1960 to 2010 and is available for all the years during which a country was independent.[4] In 1960, 31.5 percent of the countries in our sample were democracies. By 2010, this percentage had increased to 64.1 percent, which shows the unprecedented spread of democracy we study in this paper. Our measure identifies 122 democratizations and 71 reversals from democracy to nondemocracy. The countries and years in which these events took place are listed in online appendix tables A1 and A2. Not surprisingly, our democracy measure is highly correlated with the Freedom House and Polity indices, as well as the Cheibub et al. (2010) and Boix et al. (2012) measures.

The major difference between our measure of democracy and that of Papaioannou and Siourounis (2008) is that theirs considers only permanent transitions to democracy. By considering only democratizations that are not reversed, their index encodes information on the future state of democratic institutions, which exacerbates the endogeneity concerns when it is included as a right-hand-side variable in growth regressions. Instead, we code both permanent and transitory transitions to democracy

[4] Our measure of democracy captures a bundle of institutions that characterize electoral democracies. These institutions include free and competitive elections, checks on executive power, and an inclusive political process that permits various groups in society to be represented politically. To a lesser extent, our measure of democracy also incorporates the expansion of civil rights, which are taken into account in Freedom House's assessment of whether a country is free or not. Figure A2 shows that these institutional components covary strongly. After a transition to democracy, we observe sharp improvements in the likelihood that the country holds free and competitive elections, enacts institutional constraints on the executive, and opens participation into the political system. The pattern in fig. A2 suggests that the effects we estimate correspond to the joint effects of this bundle of democratic institutions, which improve in tandem following a democratization. Although our measure of democracy comprises the main characteristics of an electoral democracy, it leaves out other important de facto and de jure elements that are part of the broader set of inclusive institutions emphasized by Acemoglu and Robinson (2012). Consider, for instance, the case of North Korea. A democratization, according to our measure of democracy, would not transform it into South Korea. But in terms of political institutions, a democratization would get North Korea closer to the average electoral democracy in our sample, which includes countries such as Bangladesh, Indonesia, Kyrgyzstan, or Nepal. Though coded as democratic in 2010, these countries still struggle with clientelism, corruption, and lack of state capacity.

and nondemocracy. For example, our measure of democracy indicates that Argentina had a short spell of democracy from 1973 to 1976, when it held general elections for the first time in 10 years. This spell was interrupted by a military coup in 1976, which put a series of military dictators in power until 1983—a period we code as nondemocratic. Argentina returned to democracy again in 1983, when the collapse of the military junta gave way to general elections. While we code all such transitions, Papaioannou and Siourounis code only the permanent transition to democracy in 1983.

As our main outcome variable, we use the log of GDP per capita measured in year 2000 dollars, which we obtained from the World Bank Development Indicators. This measure is available for an unbalanced panel of 175 countries from 1960 to 2010 that constitutes our main sample. Additional variables used include investment, trade (exports plus imports), enrollment in secondary and primary schools, and infant mortality from the World Bank Development Indicators; financial flows (net foreign assets over GDP) from Lane and Milesi-Ferretti (2007); total factor productivity (TFP) from the Penn World Tables constructed by Feenstra, Inklaar, and Timmer (2015); tax revenues from Hendrix (2010); and an index of economic reforms coded by Giuliano et al. (2013). Finally, using Banks and Wilson's (2013) Cross-National Time-Series Data Archive, we construct a dichotomous measure of social unrest that indicates the occurrence of riots and revolts. In some of our exercises, we group countries into seven geographic regions, following the World Bank classification. These regions are Africa, East Asia and the Pacific, Eastern Europe and Central Asia, Western Europe and other developed countries, Latin America and the Caribbean, the Middle East and the North of Africa, and South Asia.

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

III. Dynamic Panel Estimates

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

A. Baseline Results

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

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

	NONDEMOCRACIES			DEMOCRACIES		
	Observations	Mean	Standard Deviation	Observations	Mean	Standard Deviation
GDP per capita	3,376	$2,074	$3,838	3,558	$8,149	$9,334
Investment share of GDP	3,012	.22	.10	3,300	.23	.07
Trade share of GDP	2,959	.71	.50	3,427	.77	.41
Primary-school enrollment rate	2,305	90.13%	27.47%	2,771	101.60%	15.72%
Secondary-school enrollment rate	1,970	44.76%	31.09%	2,500	75.78%	29.62%
Tax revenue share of GDP	2,524	.15	.08	2,442	.20	.09
Child mortality per 1,000 births	3,185	76.75	47.58	3,492	33.07	32.87
Unrest rate	2,911	29.44%	45.58%	3,419	22.57%	41.81%
Market reforms index (0–100)	2,533	26.89	23.09	2,711	52.73	24.61

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

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

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

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

ASSUMPTION 1 (Sequential exogeneity). $\mathbb{E}[\varepsilon_{ct}|y_{ct-1}, \dots, y_{ct_0}, D_{ct}, \dots, D_{ct_0}, \alpha_c, \delta_t] = 0$ for all $y_{ct-1}, \dots, y_{ct_0}$, $D_{ct}, \dots, D_{ct_0}$, α_c, and δ_t and for all c and $t \geq t_0$.

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

Economically, this assumption imposes that countries that transition to or away from democracy are not on a different GDP trend relative to others with similar levels of GDP in the past few years (captured by the lags of GDP) and similar levels of long-run development (captured by country fixed effects). This is a strong assumption, but it is not implausible. Besides controlling for the fact that democratizations are more frequent after economic crises, the lags of GDP per capita summarize the impact of a range of economic factors that affect both growth and democracy, such as commodity prices, agricultural productivity, and technology. Indeed, many of these economic factors should have an impact on future GDP, primarily through their influence on current GDP. As our results in Section VI show, various policy and other institutional outcomes, such as taxes and a range of economic reforms, also change following democratization. But we do not view these changes as confounding our estimates

[5] It is also useful for comparison with our second strategy to note that eq. (1) can be interpreted as specifying the treatment effects of a transition to democracy (or a reversal). Anticipating notation that we introduce in the next section, let $\Delta y_{ct}^{s}(d) = y_{ct}^{s}(d) - y_{ct-1}$ denote the potential change in (log) GDP per capita from time $t-1$ to time $t+s$ for a country with a change in political regime to $d \in \{0, 1\}$ at time t. Then the "treatment effect" implied by eq. (1) is $\beta^0 = \mathbb{E}[\Delta y_{ct}^{0}(1) - \Delta y_{ct}^{0}(0)|D_{ct} = 1, D_{ct-1} = 0] = \beta$. Moreover, for a permanent transition to democracy, as we define below, and for all $s \geq 1$, β^s is determined recursively as $\beta^s = \beta + \Sigma_{j=1}^{p} \gamma_j \beta^{s-j}$ (with the convention that $\beta^s = 0$ for all $s < 0$).

of the effects of democracy, since they constitute some of the channels via which democracy has an impact on economic outcomes. Finally, our confidence in the plausibility of assumption 1 is bolstered by the fact that controlling for a variety of economic factors and potential sources of differential trends in table 4 has very little impact on our estimates, and our IV strategy in Section V, which filters out country-specific changes in democracy, yields broadly similar estimates as well. This triangulation of evidence suggests that controlling for lags of GDP and country fixed effects is successfully accounting for the selection of countries into democracy.

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

Under assumption 1 and stationarity, equation (1) can be estimated with the standard within estimator.[6] Columns 1–4 of table 2 report the within estimates, controlling for different numbers of lags. Throughout, the reported coefficient on democracy is multiplied by 100 to ease its interpretation, and we report standard errors robust against heteroskedasticity.

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

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

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

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

with T_c being the number of times a country appears in the estimation sample. The within estimator has an asymptotic bias of order $1/T$ when D_{ct} and y_{ct-j} are sequentially exogenous and GDP is stationary. Thus, for long panels, such as the one we use, the within estimator provides a natural starting point.

the cumulative long-run effect of a permanent transition to democracy on GDP is

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

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

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

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

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

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

The within estimates of the dynamic panel model in columns 1–4 have an asymptotic bias of order $1/T$, which is known as the Nickell bias. This

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

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

TABLE 2
EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA

	Within Estimates				Arellano and Bond Estimates				HHK Estimates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Democracy	.973	.651	.787	.887	.959	.797	.875	.659	.781	.582	1.178	1.682
	(.294)	(.248)	(.226)	(.245)	(.477)	(.417)	(.374)	(.378)	(.455)	(.387)	(.370)	(.352)
Log GDP, first lag	.973	1.266	1.238	1.233	.946	1.216	1.204	1.204	.938	1.158	1.150	1.155
	(.006)	(.038)	(.038)	(.039)	(.009)	(.041)	(.041)	(.038)	(.011)	(.038)	(.040)	(.036)
Log GDP, second lag		−.300	−.207	−.214		−.270	−.193	−.205		−.217	−.127	−.122
		(.037)	(.046)	(.043)		(.038)	(.045)	(.042)		(.035)	(.050)	(.041)
Log GDP, third lag			−.026	−.021			−.028	−.020			−.030	−.040
			(.028)	(.028)			(.028)	(.027)			(.026)	(.024)
Log GDP, fourth lag			−.043	−.039			−.036	−.038			−.039	−.028
			(.017)	(.034)			(.020)	(.033)			(.015)	(.026)
p-value, lags 5–8				.565				.478				.094
Long-run effect of democracy	35.587	19.599	21.240	22.008	17.608	14.882	16.448	11.810	12.644	9.929	25.032	35.104
	(13.998)	(8.595)	(7.215)	(7.740)	(10.609)	(9.152)	(8.436)	(7.829)	(8.282)	(7.258)	(10.581)	(11.140)

Effect of democracy after 25 years	17.791	13.800	16.895	17.715	13.263	12.721	14.713	10.500	10.076	8.537	20.853	29.528
	(5.649)	(5.550)	(5.297)	(5.455)	(7.281)	(7.371)	(7.128)	(6.653)	(6.245)	(6.032)	(7.731)	(7.772)
Persistence of GDP process	.973	.967	.963	.960	.946	.946	.947	.944	.938	.941	.953	.952
	(.006)	(.005)	(.005)	(.007)	(.009)	(.009)	(.009)	(.009)	(.011)	(.010)	(.009)	(.009)
AR2 test *p*-value					.01	.08	.51	.95				
Unit root test *t*-statistics	−4.79	−3.89	−4.13	−7.00								
p-value (reject unit root)	.00	.00	.00	.00								
Observations	6,790	6,642	6,336	5,688	6,615	6,467	6,161	5,513	6,615	6,467	6,161	5,513
Countries in sample	175	175	175	175	175	175	175	175	175	175	175	175

NOTE.—This table presents estimates of the effect of democracy on log GDP per capita. The reported coefficient on democracy is multiplied by 100. Cols. 1–4 present results from the within estimator. Cols. 5–8 present results from Arellano and Bond's (1991) GMM estimator. The AR2 row reports the *p*-value for a test of serial correlation in the residuals of the GDP series. Cols. 9–12 present results from the HHK (Hahn et al. 2001) estimator. In all specifications we control for a full set of country and year fixed effects. Cols. 4, 8, and 12 include eight lags of GDP per capita as controls, but we report only the *p*-value of a test for joint significance of lags 5–8. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

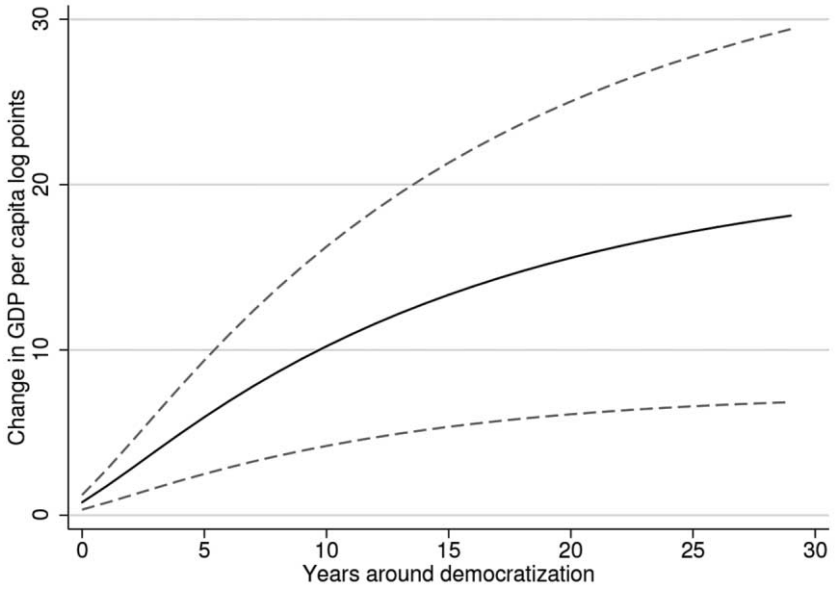

FIG. 2.—Dynamic panel model estimates of the over-time effects of democracy on the log of GDP per capita. This figure plots the estimated change in the log of GDP per capita caused by a permanent transition to democracy. The effects are obtained by forward iteration of the estimated process for GDP modeled in equation (1). A 95 percent confidence interval obtained with the delta method is presented in dotted lines. Time (in years) relative to the year of democratization runs on the horizontal axis.

bias results from the failure of strict exogeneity in dynamic panel models (Nickell 1981; Alvarez and Arellano 2003). Because T is fairly large in our panel (on average, each country is observed 38.8 times), this bias should be small in our setting, which motivates our use of the within estimator in columns 1–4 as a natural starting point.

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

$$\mathbb{E}[(\varepsilon_{ct} - \varepsilon_{ct-1})(y_{cs}, D_{cs+1})'] = 0$$

for all $s \leq t - 2$. Arellano and Bond (1991) develop a GMM estimator based on these moments. In columns 5–8, we report estimates from the same four models reported in columns 1–4, using this GMM procedure. Consistent with our expectations that the within estimator has at most a small bias, the GMM estimates are very similar to our preferred specification in column 3. The only notable difference is that GMM estimates imply a slightly smaller persistence for the GDP process, which

leads to smaller long-run impacts than in column 3. For example, in column 7, which presents the GMM estimates of our preferred specification with four lags, we find a long-run impact of democracy on GDP per capita of 16.45 percent (standard error = 8.436 percent).

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

One drawback of the Arellano and Bond GMM estimator is that the number of moment conditions is of the order of T^2. Thus, for large values of T, we have a version of the "too many instruments" problem, which leads to an asymptotic bias of order $1/N$ in our GMM estimates (see Alvarez and Arellano 2003).[9] To address this issue, we use an alternative estimator proposed by Hahn, Hausman, and Kuersteiner (2001), which is unbiased when N and T are both large, assumption 1 holds, and GDP is stationary.[10] We refer to this procedure as the "HHK estimator"

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

[10] Hahn et al. (2001) note that Arellano and Bond's GMM estimator is a minimum-distance combination of estimates of the model

$$y_{it}^{*} = \beta D_{it}^{*} + \sum_{j=1}^{p} \gamma_j y_{it-j}^{*} + \varepsilon_{it}^{*},$$

obtained via two-stage least squares (2SLS) separately for $t = 1, 2, \ldots, T-1$, with $\{y_{is}, D_{is}\}_{s=1}^{t-1}$ as instruments. Here, x_{it}^{*} is the forward orthogonal deviation of variable x_{it}, defined as

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

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

throughout the paper. The results using this estimator are reported in columns 9–12. Once we include four or more lags, they are similar to the within estimates. For example, in column 11, which corresponds to our preferred specification, the long-run effect of a permanent transition to democracy on GDP is estimated as 25.03 percent (standard error = 10.581 percent).

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

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

$$\Delta y_{it} = \beta D_{it} + \sum_{j=1}^{p} \gamma_j' \Delta y_{it-j} + \alpha_i + \delta_t + \varepsilon_{it}, \tag{3}$$

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

Our third strategy to deal with unit root or near–unit root dynamics in the GDP process is to impose different levels of persistence for this process, ranging from 0.95 to 1. To do so, we restrict the sum of the coefficients on lags of GDP, $\Sigma_{j=1}^{p} \gamma_j$ (which governs the overall amount of persistence), to be equal to 0.95, 0.96, 0.97, 0.98, 0.99, or 1. These models are obtained by replacing the left-hand side variable in equation (3) with $y_{it} - (\Sigma_{j=1}^{p} \gamma_j) y_{it-1}$, which implies that the right-hand side coefficients are given by $\gamma_j' = (\Sigma_{i=0}^{j} \gamma_i) - \rho$. We then estimate this restricted model, using the within estimator. The results, reported in appendix table A4, show that our findings are robust to assuming high levels of persistence for

[11] We should note, however, that the Levin et al. (2002) test requires two restrictive conditions to be satisfied: that the persistence of the GDP process is the same for all countries and that all cross-sectional dependence can be fully absorbed by year fixed effects. When computing the test statistics for our unbalanced panel, we use the adjustment factors that Levin et al. suggest for the average length of our panel (38.8 years).

the GDP process. Because in these models the left-hand side variable and the regressors are stationary (provided that $\Sigma_{j=1}^{p}\gamma_j \leq 1.95$) and because the persistence term is not estimated, our estimates are robust both to the potentially poor asymptotic behavior of the estimators near a unit root and to actual nonstationarity.

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

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

B. Robustness

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

The most obvious source of bias in our estimates would come from differential GDP trends among countries that democratize. In column 2, we control for potential trends related to differences in the level of GDP at

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

TABLE 3
EFFECT OF DEMOCRACY ON THE GROWTH RATE OF GDP PER CAPITA

	WITHIN ESTIMATES				ARELLANO AND BOND ESTIMATES				HHK ESTIMATES			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Democracy	1.028	1.201	1.269	1.378	1.458	1.715	1.545	1.554	1.410	1.413	1.343	1.253
	(.250)	(.237)	(.243)	(.284)	(.385)	(.385)	(.368)	(.405)	(.323)	(.341)	(.348)	(.330)
GDP growth, first lag	.287	.274	.263	.250	.294	.278	.270	.254	.245	.230	.211	.211
	(.037)	(.037)	(.039)	(.037)	(.040)	(.038)	(.041)	(.036)	(.034)	(.037)	(.039)	(.036)
GDP growth, second lag		.047	.060	.042		.057	.064	.047		.075	.082	.057
		(.024)	(.025)	(.023)		(.025)	(.025)	(.024)		(.023)	(.025)	(.020)
GDP growth, third lag			.023	.024			.031	.028			.029	.036
			(.018)	(.017)			(.018)	(.018)			(.015)	(.021)
GDP growth, fourth lag			−.033	−.018			−.022	−.013			−.007	−.004
			(.022)	(.025)			(.022)	(.025)			(.015)	(.020)
p-value, lags 5–8				.00				.00				.02
Long-run effect of democracy (on growth rate)	1.443	1.768	1.845	1.960	2.065	2.579	2.349	2.273	1.867	2.032	1.961	1.790
	(.360)	(.372)	(.370)	(.441)	(.559)	(.624)	(.602)	(.644)	(.448)	(.511)	(.527)	(.488)

Level effect of democracy after 25 years	35.483	43.245	45.275	42.715	50.767	62.957	57.296	50.090	46.067	49.685	47.791	40.543
	(8.820)	(9.046)	(9.038)	(9.191)	(13.718)	(15.105)	(14.572)	(13.322)	(11.017)	(12.448)	(12.770)	(10.837)
Persistence of growth rate process	.287	.321	.312	.297	.294	.335	.342	.316	.245	.305	.315	.300
	(.037)	(.041)	(.038)	(.043)	(.040)	(.043)	(.041)	(.044)	(.034)	(.039)	(.043)	(.049)
AR2 test *p*-value					.10	.81	.98	.93				
Observations	6,642	6,490	6,178	5,523	6,467	6,315	6,003	5,348	6,467	6,315	6,003	5,348
Countries in sample	175	175	175	175	175	175	175	174	175	175	175	174

NOTE.—This table presents estimates of the effect of democracy on the growth rate of GDP per capita. The reported coefficient on democracy is multiplied by 100. Cols. 1–4 present results from the within estimator. Cols. 5–8 present results from Arellano and Bond's (1991) GMM estimator. The AR2 row reports the *p*-value for a test of serial correlation in the residuals of the growth rate series. Cols. 9–12 present results from the HHK (Hahn et al. 2001) estimator. In all specifications we control for a full set of country and year fixed effects. Cols. 4, 8, and 12 include eight lags of the growth rate of GDP per capita as controls, but we report only the *p*-value of a test for joint significance of lags 5–8. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

TABLE 4
EFFECT OF DEMOCRACY ON (LOG) GDP PER CAPITA, CONTROLLING FOR COVARIATES

		COVARIATES INCLUDED						
	(1)	GDP in 1960 Quintiles × Year Effects (2)	Soviet Dummies (3)	Lags of Unrest (4)	Lags of Trade (5)	Lags of Financial Flows (6)	Lags of Demographic Structure (7)	Region × Regime × Year Effects (8)
	A. Within Estimates							
Democracy	.787	.718	.911	.705	.595	.926	.650	.834
	(.226)	(.249)	(.251)	(.224)	(.264)	(.244)	(.230)	(.264)
Long-run effect of democracy	21.240	22.173	24.860	17.000	14.593	23.870	14.153	16.651
	(7.215)	(8.702)	(7.783)	(5.980)	(7.122)	(8.211)	(5.419)	(5.546)
Effect of democracy after 25 years	16.895	16.261	19.587	13.567	11.500	18.149	12.251	14.532
	(5.297)	(5.982)	(5.724)	(4.644)	(5.336)	(5.435)	(4.552)	(4.726)
Persistence of GDP process	.963	.968	.963	.959	.959	.961	.954	.950
	(.005)	(.005)	(.005)	(.004)	(.006)	(.006)	(.005)	(.005)
Observations	6,336	5,523	6,336	5,643	5,750	4,950	6,262	6,336
Countries in sample	175	149	175	171	172	171	172	175
	B. Arellano and Bond Estimates							
Democracy	.875	.730	1.073	.693	1.034	1.017	.756	1.217
	(.374)	(.387)	(.403)	(.396)	(.469)	(.373)	(.370)	(.420)
Long-run effect of democracy	16.448	14.865	20.006	9.871	17.926	18.607	12.152	18.209
	(8.436)	(8.998)	(8.981)	(6.479)	(9.021)	(7.842)	(6.639)	(6.746)
Effect of democracy after 25 years	14.713	12.759	17.874	9.159	15.659	15.903	11.334	16.861
	(7.128)	(7.350)	(7.564)	(5.768)	(7.593)	(6.327)	(6.004)	(6.050)

Persistence of GDP process	.947	.951	.946	.930	.942	.945	.938	.933
	(.009)	(.008)	(.009)	(.012)	(.009)	(.007)	(.010)	(.010)
AR2 test *p*-value	.51	.90	.28	.62	.72	.34	.58	.70
Observations	6,161	5,374	6,161	5,467	5,570	4,779	6,090	6,161
Countries in sample	175	149	175	171	172	171	172	175
	C. HHK Estimates							
Democracy	1.178	.722	1.059	1.203	1.110	2.030	1.262	1.482
	(.370)	(.357)	(.364)	(.376)	(.332)	(.359)	(.355)	(.449)
Long-run effect of democracy	25.032	15.731	21.648	25.557	24.575	32.631	22.161	26.358
	(10.581)	(8.476)	(9.431)	(9.842)	(9.031)	(7.727)	(6.641)	(9.178)
Effect of democracy after 25 years	20.853	12.719	18.313	20.753	19.407	28.896	19.633	22.776
	(7.731)	(6.503)	(7.162)	(7.072)	(6.359)	(6.223)	(5.647)	(7.380)
Persistence of GDP process	.953	.954	.951	.953	.955	.938	.943	.944
	(.009)	(.006)	(.009)	(.008)	(.008)	(.008)	(.006)	(.008)
Observations	6,161	5,374	6,161	5,467	5,570	4,779	6,090	6,161
Countries in sample	175	149	175	171	172	171	172	175

NOTE.—This table presents estimates of the effect of democracy on log GDP per capita. The reported coefficient of democracy is multiplied by 100. Panel A presents results from the within estimator. Panel B presents results from Arellano and Bond's GMM (1991) estimator. The AR2 row reports the *p*-value for a test of serial correlation in the residuals of the GDP series. Panel C presents results using the HHK (Hahn et al. 2001) estimator. In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. In addition, we control for the covariates specified in each column label and described in the text. Standard errors robust against heteroskedasticity and serial correlation at the country level are reported in parentheses.

the start of our sample. To do so, we interact dummies for the quintile of the GDP per capita rank of the country in 1960 with a full set of year effects (to maximize our sample, we rank counties using Angus Maddison's GDP estimates for 1960, which are available for 149 countries). This specification identifies the effect of democracy by comparing countries that had similar levels of economic development at the start of our sample. These controls have very little impact on our results. The within estimate for the coefficient of democracy is now 0.718 (standard error = 0.249), and the long-run effect is 22.17 percent. Arellano and Bond's GMM and the HHK estimates remain similar once these controls are included, although the effects of democracy are slightly smaller.[13]

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

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

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

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

In Section V, we exploit regional democratization waves as an exogenous source of variation in a country's likelihood of transitioning to de-

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

mocracy. Here, we would like to understand whether our baseline results are driven by differential movements in GDP and democracy across region × initial regime cells (which will be the level at which our instruments vary). In column 8, we answer this question by controlling for a full set of geographic region × initial regime × year effects. This ensures that the effect of democracy on GDP is identified from differences between countries that are in the same region and had the same initial political regime (democracy or nondemocracy) at the start of our sample. Reassuringly, this strategy leads to estimates that are similar to our baseline results.[14]

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

Second, in appendix table A8, we explore the sensitivity of our baseline results to outliers. We estimate our preferred specification by excluding countries with a standardized residual above 1.96 or below −1.96, and we also exclude observations with a Cook's distance above a common rule-of-thumb threshold (four divided by the number of observations). Finally, we report results using Li's (1985) and Huber's (1964) robust estimators. In all cases, the results, especially for the long-run effect of democracy, are very similar to our baseline results, establishing that our findings are not driven by outliers.

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

[14] The size of our estimates is also similar to that of our baseline 2SLS results contained in table 6, even though they exploit an orthogonal source of variation. Motivated by our IV specifications reported in Sec. V, in additional exercises that we do not report, we also found similar estimates of democracy on growth when controlling for four lags of the average GDP per capita, average unrest, and average trade (imports plus exports over GDP) among countries in the same region × initial regime cells. These controls take into account regional shocks among countries with similar political characteristics.

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

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

IV. Treatment Effects and Semiparametric Estimates

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

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

A. *Modeling Selection on Observables*

Let us recap the notation for potential outcomes used already in note 5. Let $y_{it}^{s}(d)$ denote the potential GDP level (in logs) at time $t + s$ for coun-

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

try c transitioning to either democracy or a nondemocracy at time t, denoted by $d \in \{0, 1\}$. Specifically, for a country transitioning to democracy at t, we have $d = 1$ ($D_{ct} = 1, D_{ct-1} = 0$), and for one that remains in nondemocracy, we have $d = 0$ ($D_{ct} = D_{ct-1} = 0$). Let $\Delta y^s_{ct}(d) = y^s_{ct}(d) - y_{ct-1}$ denote the potential change in (log) GDP per capita from time $t - 1$ to time $t + s$ for a country with a change in political regime $d \in \{0, 1\}$. With analogy to the treatment effects literature, we can think of $d \in \{0, 1\}$ as corresponding to the "treatment" and $\Delta y^s_{ct}(d)$ for $s \geq 0$ as the potential outcomes affected by the treatment.

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

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

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

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

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

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

The economic content of this assumption is similar to that of assumption 1, which was the basis of our analysis in Section III. Both assumptions condition on lags of GDP to model selection into democracy and to remove the GDP dip shown in figure 1. Moreover, as already pointed out for assumption 1, they both rule out time-varying omitted factors that affect both GDP and democracy. Yet they differ in how they incorporate the dynamics of GDP and unobserved fixed country characteristics. Assumption 1 restricts GDP dynamics to be linear but allows for time-invariant unobserved country characteristics to shift GDP additively. On the other hand, assumption 2 does not require us to specify the functional

form of the dynamics of GDP or how fixed and unobserved country characteristics affect it. But it does so at the cost of restricting unobserved country heterogeneity to be common to all countries that are nondemocracies at time $t - 1$ and have experienced the same recent path for GDP per capita. Put differently, assumption 2 imposes one of the following two requirements: either omitted characteristics that affect both the likelihood of democratization and GDP growth (such as the GDP dip in fig. 1) are fully captured by the lags of GDP or any such omitted characteristics are common to all nondemocracies at time $t - 1$ (as would be the case for institutional features common to nondemocratic regimes), so that countries that democratize are not on a different trend relative to other nondemocracies with similar levels of GDP in the recent past. We show below that controlling for the lags of GDP indeed removes the GDP dip, providing partial support for the first requirement. The second requirement receives support from the evidence in Acemoglu et al. (2005) and appendix table A11, both of which suggest that the economic factors that permanently increase a country's GDP do not increase the likelihood of democratization.[16]

B. Estimation under Selection on Observables

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

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

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

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

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

Our second approach follows Angrist and Kuersteiner (2011) and Angrist, Jordà, and Kuersteiner (2013) and estimates the effect of democratizations on growth, conditioning on the propensity score for transitions to democracy. We model and estimate this propensity score via a probit regression of the probability of transitioning to democracy (conditional on $D_{ct-1} = 0$) on year fixed effects and four lags of GDP. We then estimate the causal effect of democracy on GDP, using the efficient weighting scheme of Hirano, Imbens, and Ridder (2003), with the propensity score determining the weights for different observations. This scheme gives greater weight to observations in the control group (nondemocratizers) with a high propensity score that exhibit dynamics in GDP similar to those preceding a democratization, thus generating a control group comparable to democratizers.[18]

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

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

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

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

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

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

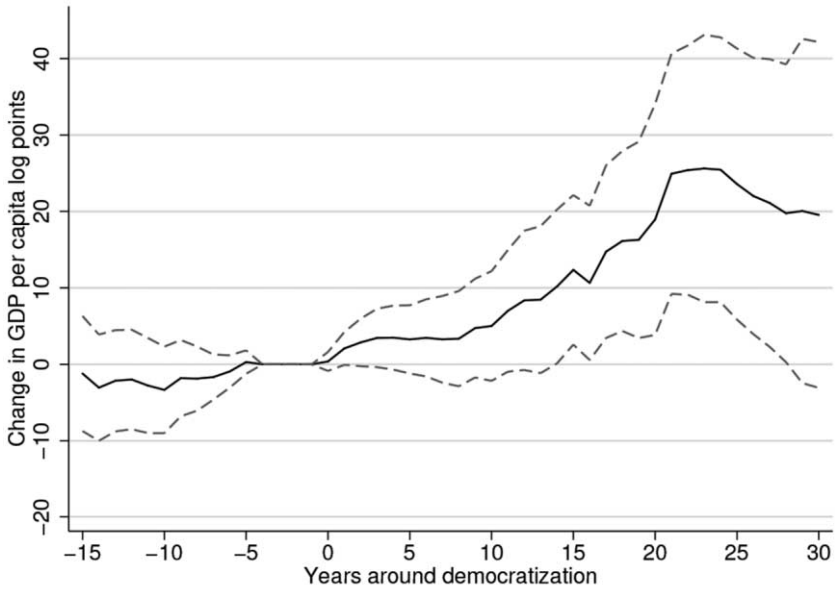

FIG. 3.—Semiparametric estimates of the over-time effects of democracy on the log of GDP, obtained with a regression model to estimate counterfactuals. This figure plots semiparametric estimates of the effect of democratization on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), with a 95 percent confidence interval in dashed lines. Time (in years) relative to the year of democratization runs on the horizontal axis. The estimates are obtained by assuming and estimating a linear model for counterfactual outcomes, which we use to control for the influence of GDP dynamics. Section IV explains our approach in full detail.

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

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

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

TABLE 5
SEMIPARAMETRIC ESTIMATES OF THE EFFECT OF DEMOCRATIZATIONS ON (LOG) GDP PER CAPITA

	Average Effects from						
	−5 to −1 Years (1)	0–4 Years (2)	5–9 Years (3)	10–14 Years (4)	15–19 Years (5)	20–24 Years (6)	25–29 Years (7)
	A. Linear Regression Adjustment						
Avg. effect on log GDP	.060	2.454	3.621	7.806	14.037	24.075	21.310
	(.156)	(1.382)	(2.792)	(4.416)	(5.384)	(8.262)	(9.643)
	B. Inverse-Propensity-Score Reweighting						
Avg. effect on log GDP	−1.586	3.724	3.214	6.818	13.542	24.111	22.184
	(1.478)	(1.789)	(3.327)	(4.848)	(5.892)	(9.035)	(11.561)
	C. Doubly Robust Estimator						
Avg. effect on log GDP	.051	2.795	2.969	6.966	12.947	23.691	21.793
	(.151)	(1.471)	(3.067)	(4.359)	(4.881)	(7.638)	(9.566)

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

Not only are the estimates from these three approaches very close to each other, but they are also similar to the impact of democracy on GDP obtained from the dynamic linear panel model presented in Section III. The congruence between the results of these approaches suggests that the specific parametrization of the GDP process is not playing an unduly important role in our conclusions.

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

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

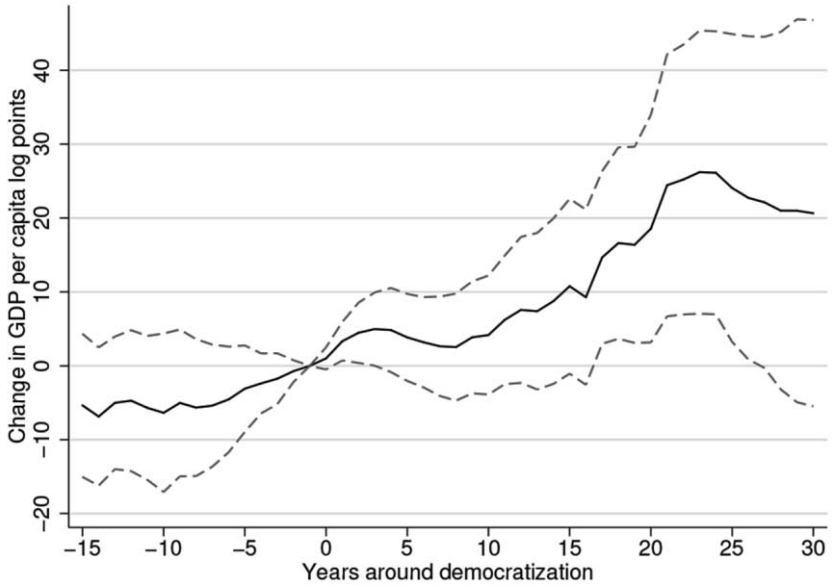

FIG. 4.—Semiparametric estimates of the over-time effects of democracy on the log of GDP, obtained with inverse-propensity-score reweighting. This figure plots semiparametric estimates of the effect of democratizations on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), with a 95 percent confidence interval in dashed lines. Time (in years) relative to the year of democratization runs on the horizontal axis. The estimates are obtained by assuming and estimating a probit model for democratizations based on GDP lags, which we use to estimate the propensity score and reweight the data. Section IV explains our approach in full detail.

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

V. IV Estimates: Democratization Waves

So far, our estimation strategies have controlled for GDP dynamics and the influence of fixed unobserved characteristics in a number of ways. In this section, we develop an IV strategy to deal with time-varying omitted

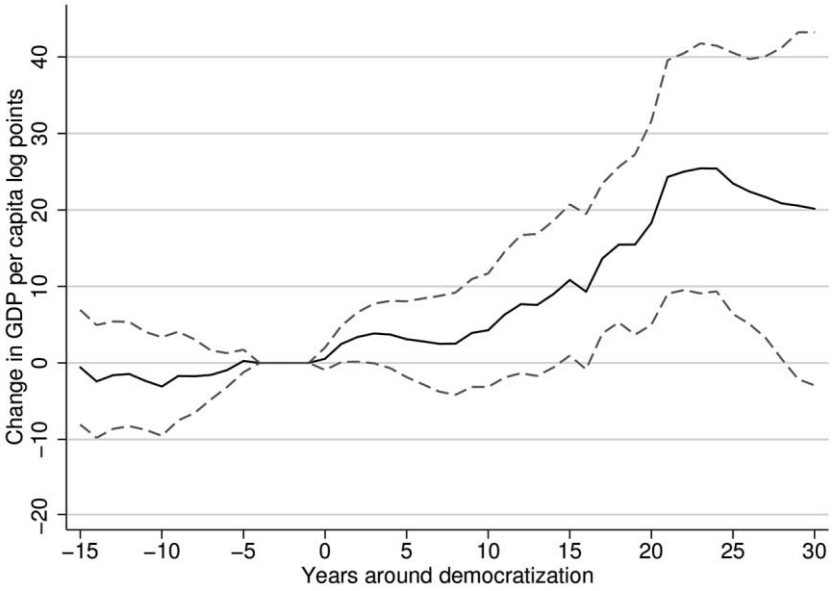

FIG. 5.—Semiparametric estimates of the over-time effects of democracy on the log of GDP; doubly robust estimates. This figure plots semiparametric estimates of the effect of democratization on GDP per capita in log points. The solid line plots the estimated average effect on GDP per capita on countries that democratized (in log points), with a 95 percent confidence interval in dashed lines. Time (in years) relative to the year of democratization runs on the horizontal axis. The estimates are obtained by assuming and estimating a probit model for democratizations based on GDP lags, which we use to estimate the propensity score and reweight the data. In addition, we partial out lags of GDP linearly, making our approach doubly robust. Section IV explains our approach in full detail.

variables that may simultaneously affect the likelihood of democracy and GDP growth. Our IV strategy further alleviates concerns related to measurement error in our measure of democracy and provides a different and complementary approach to the issue of endogenous selection into democracy (which our previous strategies confronted by conditioning on past GDP lags).

A. IV Strategy and Exclusion Restriction

As highlighted by the Arab Spring experience, democratizations and social unrest that leads to a change of regime often occur in regional waves. Countries in Latin America and the Caribbean reverted from democracy to nondemocracy in the 1970s and democratized again in the 1980s and early 1990s. The fall of the Soviet Union spurred a wave of democratizations in Eastern Europe, Central Asia, and Africa in the 1990s,

in what Huntington (1991) dubbed the "the Third Wave" (see also Markoff 1996).[21]

Although there is no consensus on the factors that create such waves, the existing evidence suggests that they are not explained by regional economic trends. For instance, as elaborated further below, Bonhomme and Manresa (2015) find that, even conditioning on GDP, transitions to democracy are correlated within regions. The most reasonable hypothesis is that this regional pattern reflects the diffusion of the demand for democracy (or, more generally, dissatisfaction with a given regime) across countries within a region, which tend to have similar histories, political cultures, practical problems, and close informational ties (e.g., see Kuran [1989], Lohmann [1994], and Ellis and Fender [2011] for theoretical models of the informational spread of political protests; see Buera, Monge-Naranjo, and Primiceri [2011] and Aidt and Jensen [2012] for empirical evidence).[22]

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

We illustrate the existence of democratization waves in the top panel of figure 6. For each of the seven regions described in Section II, we compute the share of democracies among initial nondemocracies and present its evolution over time relative to the year in which the first democratization in that region took place (we remove the first democratization in the region to avoid a mechanical increase in the share of democratic countries at time 0). For comparison, we also plot, for the remaining regions, the share of democracies among initial nondemocracies. Following the first democratization in a region, the share of countries that democratize in that region quickly converges to that of other regions, which illustrates the existence of waves of democratization. The bottom panel presents an analogous figure for the share of countries that transitioned from democracy to nondemocracy.

To formalize the existence of waves, we first define the set of countries that may influence the demand for democracy in a given country. For each country c, let D_{ct_0} denote whether the country was a democracy or

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

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

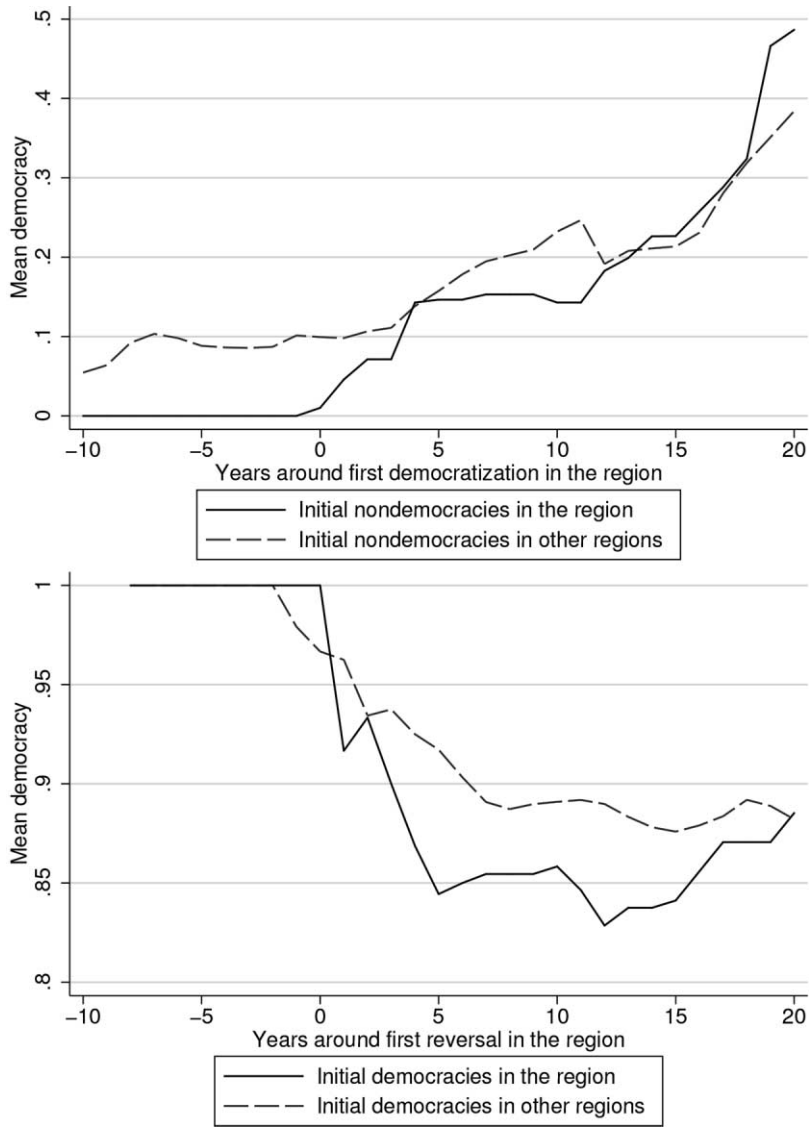

FIG. 6.—Regional democratizations and reversal waves. These figures illustrate the existence of regional democracy waves. The top figure plots average democracy among initial nondemocracies around the first democratization in the region. For comparison, it also plots average democracy among other initial nondemocracies in other regions. The bottom figure plots average democracy among initial democracies around the first reversal in the region. For comparison, it also plots average democracy among other initial democracies in other regions.

a nondemocracy at the start of our sample, and let R_c denote the geographic region in which the country lies (using the seven regions introduced in Sec. II). We posit that democracy in country c is influenced by democracy in the set of countries $I_c = \{c' : c' \neq c, R_{c'} = R_c, D_{c't_0} = D_{ct_0}\}$, which includes countries in the same region that share a similar political history, meaning that $D_{c't_0} = D_{ct_0}$. Using these sets, we define our instrument as

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

Here, Z_{ct} is the jackknifed average of democracy in a region × initial regime cell, which leaves out the own-country observation.

The corresponding 2SLS model we estimate is given by

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

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

Our key assumption in this section can be written as:

ASSUMPTION 3 (exclusion restriction). $\mathbb{E}[\varepsilon_{ct} | y_{ct-1}, \ldots, y_{ct_0}, Z_{ct-1}, \ldots, Z_{ct_0}, \alpha_c, \delta_t] = 0$ for all $y_{ct-1}, \ldots, y_{ct0}$, $Z_{ct}, \ldots, Z_{ct_0}$, α_c, and δ_t and for all c and $t \geq t_0$.

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

The key advantage of assumption 3 over assumptions 1 and 2 is that it allows for time-varying, unobserved country heterogeneity but requires that such heterogeneity not be related to past regional waves of democratization. Thus, idiosyncratic factors that simultaneously influence GDP

and the likelihood of a democratic transition in a single country would violate assumptions 1 or 2 but not assumption 3. Particularly salient threats to assumptions 1 and 2 include changes in social unrest or political discontent in a nondemocratic country that make a democratic transition more likely while at the same time having a direct impact on future GDP, the rise of a middle class that simultaneously contributes to future growth prospects and articulates demands for a democratic transition. Because these types of idiosyncratic factors are not correlated with regional trends, they do not violate our assumption 3 or bias our IV estimates.

B. First-Stage and 2SLS Estimates

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

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

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

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

TABLE 6
IV Estimates of the Effect of Democracy on (Log) GDP per Capita

			Covariates Included						
	(1)	(2)	GDP in 1960 Quintiles × Year Effects (3)	Soviet Dummies (4)	Regional Trends (5)	Regional GDP and Trade (6)	Regional Unrest GDP and Trade (7)	Spatial Lag of GDP (8)	Spatial Lags of GDP and Democracy (9)
				A. 2SLS Estimates with Fixed Effects					
Democracy	.966	1.149	1.125	1.292	1.697	1.817	1.107	1.335	1.361
	(.558)	(.554)	(.689)	(.651)	(.885)	(.663)	(.656)	(.536)	(.895)
Long-run effect of democracy	26.315	31.521	35.226	35.723	36.788	41.544	25.016	37.482	38.439
	(17.075)	(17.425)	(23.846)	(19.997)	(20.657)	(17.157)	(16.002)	(17.836)	(27.883)
Effect of democracy after 25 years	20.836	24.866	25.618	27.929	32.051	35.350	21.386	29.217	29.011
	(12.862)	(12.978)	(16.538)	(14.944)	(17.703)	(14.017)	(13.342)	(12.894)	(19.692)
Persistence of GDP process	.963	.964	.968	.964	.954	.956	.956	.964	.965
	(.005)	(.005)	(.005)	(.005)	(.006)	(.006)	(.006)	(.005)	(.006)
Hansen *p*-value		.21	.18	.32	.28	.25	.09	.04	.19
Observations	6,312	6,309	5,496	6,309	6,309	6,309	6,309	6,181	6,009
Countries in sample	174	174	148	174	174	174	174	173	173
Excl.-instruments *F*-statistic	119.1	33.2	16.8	26.7	23.7	13.6	16.7	17.5	4.6
				B. First-Stage Estimates					
Democracy wave $t-1$	.800	.547	.503	.480	.498	.522	.508	.540	.586
	(.073)	(.101)	(.130)	(.099)	(.092)	(.104)	(.102)	(.103)	(.101)

Democracy wave $t-2$		.133	.109	.133	.129	.117	.115	.136	.128
		(.081)	(.094)	(.080)	(.081)	(.079)	(.078)	(.078)	(.088)
Democracy wave $t-3$		.227	.270	.223	.228	.221	.223	.224	.282
		(.067)	(.077)	(.065)	(.070)	(.069)	(.070)	(.070)	(.077)
Democracy wave $t-4$		−.087	−.119	−.075	−.123	−.083	−.064	−.072	−.107
		(.110)	(.126)	(.110)	(.106)	(.113)	(.113)	(.113)	(.116)
	C. HHK Estimates								
Democracy	.690	.944	1.435	.719	.822	1.311	.897	1.021	1.206
	(.642)	(.479)	(.599)	(.503)	(.480)	(.435)	(.371)	(.549)	(.485)
Long-run effect of democracy	14.512	24.766	46.767	18.337	16.413	24.040	17.290	29.286	31.111
	(14.703)	(14.083)	(22.556)	(13.688)	(10.700)	(9.989)	(8.556)	(18.354)	(15.167)
Effect of democracy after 25 years	11.768	18.670	31.039	13.969	13.778	21.100	14.668	21.133	23.702
	(11.445)	(9.799)	(13.113)	(9.935)	(8.523)	(8.038)	(6.734)	(11.942)	(10.243)
Persistence of GDP process	.952	.962	.969	.961	.950	.945	.948	.965	.961
	(.011)	(.008)	(.008)	(.009)	(.010)	(.010)	(.010)	(.009)	(.008)
Observations	6,161	6,161	5,374	6,161	6,161	6,161	6,161	6,132	5,960
Countries in sample	174	174	148	174	174	174	174	173	173

NOTE.—This table presents IV estimates of the effect of democracy on log GDP per capita. The reported coefficient of democracy is multiplied by 100. Panel A presents 2SLS estimates instrumenting democracy with up to four lags of regional democracy waves and the *p*-value of a Hansen overidentification test. Panel B presents the corresponding first-stage estimates and the excluded (excl.)-instruments *F* statistic. Panel C presents results using the HHK (Hahn et al. 2001) estimator instrumenting democracy with up to four lags of regional democracy waves (except for col. 1, where we use only one lag). In all specifications we control for a full set of country and year fixed effects and four lags of GDP per capita. In addition, 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.

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

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

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

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

Columns 8 and 9 develop a complementary strategy against the threat posed by regionally correlated omitted factors and explicitly model the spatial correlation of GDP, y_{ct}, and GDP shocks, ε_{ct}. First, we allow GDP to be spatially correlated as a function of the inverse of the distance be-

tween countries. Specifically, in column 8 we include a weighted average $\mathbb{W}^d y_t$ of GDP in other countries as a covariate and instrument it by using four of its lags (see Kelejian and Prucha [1998], Anselin [2001], and Lee [2007] on the estimation of spatial panel models). Here, $\mathbb{W}^d$ is the $N \times N$ matrix of inverse distances between countries with 0s on the diagonal (where N is the number of countries), and y_t is an $N \times 1$ vector of GDP at time t in all countries. The results in this case continue to be precisely estimated and are similar to our baseline findings.

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

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

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

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

Indeed, we find it plausible that the correlation of GDP shocks across countries depends on geographic distance, while democratization waves take place within regions, since, as discussed in note 22, protests and discontent with nondemocracies appear to have a marked regional element. Consistent with this reasoning, the first stages shown in panel B of table 6 indicate that the relationship between regional democratization waves and country-level transitions to democracy is essentially unaffected by the inclusion of the inverse-distance-weighted GDP and democracy in other countries. Our 2SLS estimate in this case is of a similar magnitude but somewhat less precisely estimated. This is not surprising, given the difficulty of separately estimating the spatial GDP correlation and the effect of regional democratization waves.

Panel C presents the corresponding HHK estimates described in Section III, but now we use lags of Z_{ct} as external instruments for democracy.[23]

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

This estimator is consistent for finite T as long as our exclusion restriction in assumption 3 holds. The results are broadly similar to our IV estimates.

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

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

VI. Mechanisms

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

We estimate models of the form

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

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

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

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

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

VII. Does Democracy Need Development?

As hinted in Section I, many critics of the view that democracy is good for economic performance suggest that democracy will be economically costly when certain preconditions, especially related to economic development and high human capital, are not satisfied. For example, Richard Posner (2010) has argued, "Dictatorship will often [be] optimal for very poor countries. Such countries tend not only to have simple economies but also to lack the cultural and institutional preconditions to democracy," while David Brooks (2013) stated, in the wake of the Egyptian coup of 2013, "It's not that Egypt doesn't have a recipe for a democratic transition. It seems to lack even the basic mental ingredients."

We investigate this hypothesis by considering interactions between democracy and the level of economic development (as proxied by the log of GDP per capita) and human capital (as proxied by the share of the population with secondary schooling, from the Barro-Lee data set). If this hypothesis is valid, we should expect the interaction terms to be positive

TABLE 7
EFFECTS OF DEMOCRACY ON POTENTIAL MECHANISMS

	Dependent Variable								
	Log of Investment Share in GDP (1)	Log of TFP (2)	Index of Economic Reforms (3)	Log of Trade Share in GDP (4)	Log of Tax Share in GDP (5)	Log of Primary-School Enrollment (6)	Log of Secondary-School Enrollment (7)	Log of Child Mortality (8)	Dummy for Unrest (9)
	A. Within Estimates								
Democracy	2.391	−.205	.687	.689	3.311	1.042	1.345	−.253	−7.832
	(1.114)	(.276)	(.348)	(.676)	(1.409)	(.338)	(.610)	(.063)	(2.185)
Long-run effect of democracy	9.112	−2.883	5.580	5.445	16.062	21.908	18.960	−34.264	−11.944
	(4.255)	(3.858)	(2.883)	(5.253)	(6.650)	(7.624)	(8.622)	(10.747)	(3.329)
Effect of democracy after 25 years	9.089	−2.738	5.359	5.303	15.864	18.892	18.057	−21.400	−11.944
	(4.245)	(3.648)	(2.753)	(5.126)	(6.574)	(6.321)	(8.146)	(5.124)	(3.329)
Persistence of outcome process	.738	.929	.877	.873	.794	.952	.929	.993	.344
	(.020)	(.012)	(.012)	(.011)	(.016)	(.008)	(.013)	(.001)	(.030)
Observations	5,665	3,879	4,692	5,738	4,511	3,714	2,883	6,084	5,646
Countries in sample	169	107	150	172	131	166	158	173	171

	B. 2SLS Estimates								
Democracy	2.211	−.941	3.224	5.512	8.088	1.757	4.116	−.715	−5.569
	(2.852)	(.667)	(.863)	(2.005)	(3.021)	(.721)	(1.626)	(.164)	(5.682)
Long-run effect of democracy	8.440	−12.738	23.775	40.589	38.609	36.693	57.072	−95.728	−8.471
	(10.705)	(8.854)	(6.215)	(13.580)	(14.330)	(15.505)	(21.698)	(26.347)	(8.577)
Effect of democracy after 25 years	8.419	−12.167	23.156	39.817	38.159	31.611	54.252	−58.625	−8.471
	(10.681)	(8.380)	(6.039)	(13.375)	(14.121)	(12.863)	(20.267)	(13.123)	(8.577)
Persistence of outcome process	.738	.926	.864	.864	.791	.952	.928	.993	.343
	(.020)	(.012)	(.012)	(.012)	(.017)	(.008)	(.013)	(.001)	(.030)
Excl.-instruments *F*-statistic	21.7	27.7	43.7	21.5	31.8	12.1	10.4	26.3	28.6
Hansen *p*-value	.29	.06	.22	.09	.69	.09	.12	.02	.84
Observations	5,640	3,871	4,670	5,714	4,489	3,710	2,879	6,057	5,619
Countries in sample	168	107	149	171	130	164	156	172	170

TABLE 7 (*Continued*)

	Dependent Variable								
	Log of Investment Share in GDP (1)	Log of TFP (2)	Index of Economic Reforms (3)	Log of Trade Share in GDP (4)	Log of Tax Share in GDP (5)	Log of Primary-School Enrollment (6)	Log of Secondary-School Enrollment (7)	Log of Child Mortality (8)	Dummy for Unrest (9)
	C. HHK Estimates								
Democracy	6.603	.388	1.121	1.255	4.277	1.384	2.144	−.306	−3.638
	(1.336)	(.294)	(.371)	(.790)	(2.044)	(.366)	(.644)	(.068)	(2.931)
Long-run effect of democracy	25.495	7.518	22.655	10.182	24.622	41.349	43.070	−54.798	−5.742
	(5.313)	(6.011)	(11.199)	(6.584)	(11.858)	(14.855)	(15.445)	(15.745)	(4.630)
Effect of democracy after 25 years	25.432	6.748	15.698	9.807	23.966	29.049	36.865	−29.139	−5.742
	(5.294)	(5.366)	(5.953)	(6.307)	(11.461)	(8.614)	(11.888)	(6.131)	(4.630)
Persistence of outcome process	.741	.948	.951	.877	.826	.967	.950	.994	.366
	(.018)	(.009)	(.018)	(.014)	(.031)	(.007)	(.012)	(.001)	(.037)
Observations	5,125	3,557	4,236	4,866	4,045	3,579	2,683	5,454	5,233
Countries in sample	168	107	149	171	130	164	156	172	170

Note.—This table presents estimates of the effect of democracy on the different channels specified in the columns labels. The reported coefficient of democracy is multiplied by 100 (except for cols. 3 and 9). Panel A presents within estimates. Panel B presents 2SLS estimates instrumenting democracy with four lags of regional democracy waves, the *F*-statistic for the excluded (excl.) instruments, and the *p*-value of Hansen's overidentification test. Panel C presents results using the HHK (Hahn et al. 2001) 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.

and significant in both cases and the main effect of democracy for low-economic-development or low-schooling countries to be negative.

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

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

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

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

TABLE 8
HETEROGENEOUS EFFECTS OF DEMOCRACY ON (LOG) GDP PER CAPITA

	Interaction with Log GDP per Capita				Interaction with Share with Secondary Education			
	1960 (1)	1970 (2)	1980 (3)	Lagged (4)	1960 (5)	1970 (6)	1980 (7)	Lagged (8)
	A. Within Estimates							
Democracy	.432	.572	.687	.744	.446	.340	.385	.495
	(.275)	(.248)	(.248)	(.246)	(.254)	(.253)	(.246)	(.241)
Interaction	.001	.001	.002	.001	.046	.049	.038	.020
	(.002)	(.001)	(.002)	(.002)	(.028)	(.020)	(.014)	(.013)
Long-run effect of democracy	16.231	18.631	20.489	19.843	13.785	10.480	11.841	14.597
	(11.160)	(9.073)	(8.608)	(8.255)	(8.550)	(8.275)	(8.118)	(8.432)
Effect of democracy after 25 years	10.013	12.916	14.985	15.877	10.081	7.679	8.687	10.953
	(6.565)	(5.960)	(5.848)	(5.943)	(5.964)	(5.872)	(5.728)	(5.821)
Persistence of GDP process	.973	.969	.966	.963	.968	.968	.967	.966
	(.005)	(.005)	(.005)	(.006)	(.005)	(.005)	(.005)	(.006)
Observations	4,281	4,909	5,525	6,336	5,300	5,300	5,300	5,300
Countries in sample	93	109	131	175	138	138	138	138
	B. 2SLS Estimates							
Democracy	.500	.155	.645	1.326	−.119	−.484	−.474	.600
	(1.088)	(.961)	(.929)	(.887)	(.662)	(.665)	(.639)	(.576)
Interaction	−.002	.000	−.000	−.003	.174	.156	.116	.049
	(.005)	(.004)	(.004)	(.004)	(.060)	(.047)	(.033)	(.023)
Long-run effect of democracy	18.838	4.978	19.275	36.116	−3.649	−14.586	−14.135	17.373
	(43.554)	(31.473)	(30.208)	(29.900)	(19.968)	(19.023)	(18.114)	(18.629)
Effect of democracy after 25 years	11.592	3.486	14.078	28.377	−2.692	−10.843	−10.574	13.133
	(25.784)	(21.795)	(21.085)	(21.317)	(14.837)	(14.524)	(13.901)	(13.312)

Persistence of GDP process	.973	.969	.967	.963	.967	.967	.966	.965
	(.006)	(.006)	(.006)	(.008)	(.006)	(.006)	(.006)	(.006)
Excl.-instruments *F*-statistic	6.6	6.1	7.0	14.0	18.5	17.6	16.0	12.4
Hansen *p*-value	.81	.73	.54	.33	.44	.41	.25	.50
Observations	4,273	4,901	5,517	6,153	5,292	5,292	5,292	5,218
Countries in sample	93	109	131	174	138	138	138	138
	C. HHK Estimates							
Democracy	.222	.234	.144	1.619	1.101	.887	.790	1.713
	(.379)	(.401)	(.445)	(.477)	(.686)	(.679)	(.638)	(.584)
Interaction	.004	−.000	.001	.002	.093	.089	.058	.016
	(.003)	(.003)	(.004)	(.004)	(.046)	(.037)	(.028)	(.013)
Long-run effect of democracy	7.692	7.453	4.480	48.375	31.605	25.022	22.375	49.338
	(13.442)	(13.213)	(14.002)	(21.975)	(21.502)	(20.748)	(19.522)	(23.950)
Effect of democracy after 25 years	4.869	5.084	3.054	34.304	23.787	19.159	17.091	36.069
	(8.286)	(8.850)	(9.435)	(11.965)	(15.084)	(14.981)	(14.107)	(14.116)
Persistence of GDP process	.971	.969	.968	.967	.965	.965	.965	.965
	(.009)	(.008)	(.008)	(.009)	(.008)	(.008)	(.008)	(.009)
Observations	4,180	4,792	5,386	6,110	5,154	5,154	5,154	5,154
Countries in sample	93	109	131	174	138	138	138	138

NOTE.—This table presents estimates of the effect of democracy on log GDP per capita and its interaction with other country characteristics indicated in the column headers. The reported coefficients of democracy and the interaction are multiplied by 100. We report main effects and long-run effects evaluated at the 25th percentile of the interacted variable. Panel A presents within estimates. Panel B presents 2SLS estimates instrumenting democracy (and the interaction term) with four lags of regional democracy waves. It also reports the *F*-statistic for the excluded (excl.) instruments and the *p*-value of Hansen's overidentification test. Panel C presents results using the HHK (Hahn et al. 2001) estimator instrumenting democracy (and the interaction term) with four lags of regional democracy waves. In all specifications, we control for a full set of country and year fixed effects and four lags of GDP per capita. Standard errors robust against heteroskedasticity and serial correlation at the country level are in parentheses.

with other variables related to how modernized the population is, such as the income level.

VIII. Conclusion

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

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

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

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

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

Taken together, our results suggest that democracy is more conducive to economic growth than its detractors have argued and that there are many

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

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