

## **Democratic capital: The nexus of political and economic change**

By TORSTEN PERSSON AND GUIDO TABELLINI\*

*We study the dynamics of economic and political change, theoretically and empirically. Democratic capital measured by a nation's historical experience with democracy and the incidence of democracy in its neighborhood appears to reduce exit rates from democracy and raise exit rates from autocracy. Higher democratic capital stimulates growth by increasing the stability of democracies. Heterogeneous effects of democracy induce sorting of countries into political regimes, which helps explain systematic differences between democracies and autocracies. Our results suggest the possibility of a virtuous circle, where accumulation of physical and democratic capital reinforce each other, promoting economic development and consolidation of democracy.*

*JEL: D70, H11, N10, O11*

*Keywords: political regimes, hazard rates, economic growth*

In the past two hundred years, the world has undergone dramatic economic change, as a large number of countries have seen economic growth take off. But these takeoffs have occurred at different points in time and not always at a sustained pace, while many countries have not yet seen any takeoff at all. This change and its unequal incidence has produced huge differences in today's living standards across countries.

During the same time, the world has also undergone dramatic political change, as a large number of countries have seen the introduction of democracy. But these democratizations have occurred at different points in time and not always in a consolidated way, while many countries have not yet seen any democratization at all. This change and its unequal incidence has produced huge difference in today's democratic experience across countries.

Figure 1 plots living standards – GDP per capita – against democratic experience – years of democracy since 1800 – in the year 2000 (see Section IV for details on the underlying data). Evidently, the two measures are positively correlated. A striking aspect of this plot is its triangularity: while all levels of democratic experience are consistent with achieving very high income, no really poor country has a long history of democracy. Long democratic experience appears to be a sufficient, but not a necessary, condition for high income.

What forces may have produced these patterns in the data? By pure logic, a positive relation

\* Torsten Persson: Institute for International Economic Studies, Stockholm University, 106 91 Stockholm, Sweden, LSE, CEPR, NBER, CESifo and CIFAR, torsten.persson@iies.su.se. Guido Tabellini: IGIER, Università Bocconi, Via Guglielmo Röntgen, 20136 Milan, Italy, CEPR, CESifo and CIFAR, guido.tabellini@unibocconi.it. We thank Daron Acemoglu, Manuel Arellano, Ernesto Dal Bo, Jim Fearon, Michele Pellizzari, Eliana La Ferrara, Jan Teorell, the editors, two referees, and participants in seminars at Bocconi, LSE, Stanford, CIFAR, Gothenburg, Northwestern, IIES, Oslo, Munich, Brussels, CEMFI, UCL, NBER, Oxford, Cambridge, the PIER political economy conference, and the World Meeting of the Public Choice Society for helpful comments. We are grateful to Elena Besedina, Giovanna D'Adda and, especially, David von Below for research assistance, and to Christina Lönnblad for editorial assistance. Financial support from the Swedish Research Council, the Tore Browaldh Foundation, Bocconi University, the Italian Ministry of University and Research, and CIFAR is gratefully acknowledged.

has three possible explanations: income fosters democracy, democracy fosters income, or some other factor(s) produces a spurious correlation. Economists, political scientists and historians have considered all three explanations. Despite a large number of studies, however, the dynamic interactions between economic and political change remain poorly understood. We review the literature in Section I.

In this paper, we bring three new ideas to the study of these interactions. The first one concerns the economic effects of political regimes. If democracy influences economic performance, this also affects returns to investment. Via expected returns, the prospects of future democracy then become a determinant of current economic performance, well before any actual regime change takes place. Empirically, we argue that one must look beyond the current political regime, to expectations about its stability. However simple, this insight is overlooked in existing empirical studies.

The second idea concerns the consolidation of democracy. The latter requires that citizens learn to cherish and respect democracy. A common perception of democracy as a valuable form of government will not pop up overnight, or in a vacuum, but through a slow accumulation of a stock of civic and social assets that we call “democratic capital”. Empirically, we argue that democratic capital accumulation occurs through a country’s learning from its own historical experience or its neighboring countries.

Combining these two ideas leads to the possibility of a virtuous circle of democratic experience and economic development. Suppose a country (randomly) enters into democracy. If it persists, democratic experience keeps accumulating, which makes a backlash into autocracy less likely. If democracy is indeed more productive, a greater likelihood of democratic survival raises expected returns to investment, which feed on to economic growth. If higher income makes transition into autocracy less likely, this adds another positive feedback loop.

Our third new idea concerns endogenous selection into political regimes. The economic effects of transitions between autocracy and democracy are likely to vary across time and place. If so, we expect countries with large gains from democracy to be over-represented among democracies and under-represented among autocracies. Such systematic sorting produces non-random sample selection, with different comparative statics, and thus different empirical predictions, in democracies and autocracies.

To confront these three ideas with data, we use theory to approach the two-way interactions between economic and political change. Section II presents an overlapping-generations model, where the probability of regime change determines expected returns to investment and hence economic growth. Regime transitions are determined in an equilibrium, where individual decisions whether to participate in defending democracy (or attacking autocracy) depend on society’s endowment of democratic capital. Higher democratic capital implies a lower probability of autocracy in the future and therefore an *indirect*, but no direct, effect on economic growth. The comparative statics reflect the benefit of democracy for economic activity. Since this varies across countries, these tend to self-select into the regime that is most efficient for them. Endogenous sorting makes the probability of regime transition and economic growth different functions of observed variables in democracies and autocracies. Our model predictions are outlined in Section III.

The existing empirical literature relies either on cross-sectional data, or on panel data beginning

in 1960. To approach the rich dynamics of economic and political change, we prefer a very long time horizon even at the cost of data availability. We exploit the Maddison and Polity IV data sets to construct an unbalanced panel with annual data for at most 155 countries and at most 180 years. These data are discussed in Section IV, which also describes how to give operational contents to democratic capital.

Our theoretical predictions hold up fairly well. Section V presents evidence on political transitions. Our basic estimates show that the probability of exit from democracy indeed goes down with two forms of democratic capital, as well as with the level of real income. The exit rate out from autocracy increases in two forms of democratic capital, but does not respond to income. These inferences depend on specific identifying assumptions, however, and fixed effect estimates indicate that the results may partly reflect unobserved heterogeneity across countries and common shocks across time.

Section VI proceeds to evidence on economic growth. We find that the estimated probability of autocracy reduces economic growth in democracy, but has little (or positive) effect on economic growth in autocracy. Moreover, democratic capital has a stronger effect on growth in our sample of democracies than in our sample of autocracies.

## I. Related literature

Vast literatures have studied how democracy affects development, and how development affects democracy, although these two issues have often been studied separately. Adam Przeworski et al. (2000) and Bruce Bueno de Mesquita et al. (2003) are among few systematic studies addressing both issues together. The comprehensive study by Przeworski et al is confined to the postwar period, yet their empirical results are largely consistent with those in this paper, although the detailed analysis and the identifying assumptions differ. In particular, they find that higher income increases the survival of democracy but has no effect on the survival of autocracy, that a history of democratic instability helps predict regime transitions, and that the international political climate has an impact on the stability of democracy. This is in line with our findings on the effect of domestic and foreign democratic capital on regime transitions. On the link from political regimes to growth, the main finding of Przeworski et al is that the probability of abandoning the current regime hurts growth, particularly under autocracy.

How economic development determine the onset or survival of, democracy is the subject of many books and articles. Among recent contributions, Charles Boix (2002) focuses on the redistributive consequences of alternative political regimes, while Robert Barro (1999), Boix and Susan Stokes (2003), Daron Acemoglu et al. (2005), Edward Glaeser, Giacomo Ponzetto and Andrei Shleifer (2007) and Acemoglu et al. (2008) discuss the effect of income and education on democracy, reaching different conclusions. These studies mainly deal with the postwar period and do not use variables similar to our notion of democratic capital (see Section V for more discussion and comparison with our results).

We are not first to stress civic engagement and cultural attitudes in shaping the functioning of political institutions, and how some kind of “social capital” can be acquired over time. Important precursors include Seymour Martin Lipset (1959), Gabriel Almond and Sidney Verba (1963) and, more recently, Robert Putnam (1993), Axel Hadenius and Jan Teorell (2005) and Ronald Inglehart and Christian Welzel (2005). But our empirical methodology is very different from

these studies. In particular, democratic capital refers to variables that influence the stability of democratic regimes without direct effects on economic outcomes. The importance of culture in economic (as opposed to political) development is discussed from a different perspective in Guido Tabellini (2008a).

Several political scientists have discussed masses vs. elites in regime transitions – see Ruth Berins Collier (1999), Barbara Geddes (1999) and Nancy Bermeo (2003). James Gibson (1997) and Karl-Dieter Opp (1999) use survey data to document how decisions to participate in the uprising against socialist autocracies at the turn of the 1990s involved strategic and social considerations similar to those in our model.

How democracy (or political regimes, more generally) shapes economic development, is the subject of an equally large literature. The findings are, essentially, all over the place. John Londregan and Keith Poole (1990), Przeworski and Fernando Limongi (1993), John Helliwell (1994), Barro (1996) and Casey Mulligan, Xavier Sala-i-Martin and Ricard Gil (2004) exploit cross-country variation (or pooled time-series data for the post-war period), and find no robust effect of democracy on economic growth. On the other hand, Richard Roll and John Talbot (2003), Francesco Giavazzi and Tabellini (2005), Ben Jones and Benjamin Olken (2005), Dani Rodrik and Romain Wacziarg (2005) and Elias Papaioannou and Gregorios Siourounis (2007) exploit within-country variation, and find a positive, albeit weak, average effect of democracy on economic performance.

These papers do not consider if regime transitions were expected or unexpected, however. An exception is Londregan and Poole (1990), who attempt to estimate the effect of political instability and find no evidence that growth is affected by past coups or current coup propensity. Importantly, John Gerring et al. (2005) show that democratic history (besides the current regime) has an effect on economic performance, while Barro (1997) and Barro (1999) formulate a dynamic model of democracy, where lagged democracy helps predict future democracy. While these authors do not provide the same economic interpretation as we do, their empirical results are consistent with our reduced-form results.<sup>1</sup>

## II. A model of political and economic change

We set up a model of political regime transitions and economic growth. Our goal is not theory for its own sake, but empirical prediction, identification and specification. In our long historical sample, available time-varying data include only income per capita, the political regime and a few other variables. Thus, we leave out many possible mechanisms and construct a very simple model under the maxim: “if you can’t measure it, don’t model it”. Moreover, a simple model allows us to take heterogeneity seriously. Specifically, we can study how empirical predictions depend on endogenous sorting of countries into different political regimes by idiosyncratic country characteristics.

<sup>1</sup>At a general level, our analysis of sorting relates to other branches of economics. The classic is Gary Becker (1973), who considers assortative matching in the marriage market. More recent applications include how individuals are sorted to firms on the basis of productivity (Robert Shimer and Lones Smith, 2000), and how CEOs are sorted to firms on the basis of talent and profitability (Xavier Gabaix and Augustin Landier, 2008). Jan Eeckhout and Boyan Jovanovich (2007) bring sorting models to the macroeconomics of development. But in their study, as in the rest of the literature, sorting takes place at the level of individual agents.

In this section, we describe the economic and political equilibrium for a single country with given parameters, and the mechanics of endogenous sorting. The next section derives specific empirical predictions from the model, taking sample selection through endogenous sorting into account.

#### A. Economics and equilibrium capital accumulation

Consider a standard overlapping-generations economy with a continuum of members in each generation and constant population of mass unity. Production per old worker in period  $t$  is given by a simple  $Ak$  model:

$$(1) \quad y_t^o = A(a_t)k_t,$$

where  $k_t$  is capital per old person,  $A$  denotes productivity and  $a_t = 0, 1$  refers to democracy and autocracy respectively. We allow different levels of productivity in democracy,  $A(0) = 1 + \theta$ , and autocracy,  $A(1) = 1$ . As  $\theta$  is constant over time, productivity remains constant conditional on the political regime. Differences in productivity across regimes could reflect economic policies leading to more or less efficient outcomes. For lack of measurement, we leave the precise sources of these differences implicit. But we allow countries to have different values of  $\theta \lesseqgtr 0$ , to capture the idea that democracy may work better in some environments than in others.

Each young person has constant exogenous income  $w$ . Adding this to income per old, GDP per capita becomes

$$(2) \quad y_t = A(a_t)k_t + w.$$

Each young individual in period  $t - 1$  chooses how much to save in the form of capital  $k_t$ , to maximize her expected utility defined over consumption when young and old. Investment decisions in period  $t - 1$  are made before knowing the political regime in period  $t$  and to simplify (impose risk neutrality) we assume that utility is linear in consumption when old. Denote by  $p_t^*$  the expected probability of autocracy in period  $t$ , given the information available to the young in period  $t - 1$ . Then, expected utility becomes:

$$(3) \quad E_{t-1}(v_t) = V(w - k_t) + [p_t^* + (1 - p_t^*)(1 + \theta)]k_t,$$

where  $E_{t-1}$  denotes conditional expectations, the second term on the RHS of (3) refers to expected consumption when old, and  $V$  is a concave function with  $V_c(0) \rightarrow \infty$  and  $V_c(w) < 1 + \theta$ .

Solving this optimization problem with respect to  $k_t$ , we can write the equilibrium capital stock in period  $t$  as

$$(4) \quad k_t^* = K(p_t^*; \theta, w)$$

A Web Appendix verifies that:

LEMMA 1: *The equilibrium capital stock is increasing in last period's income of the young,*

$K_w > 0$ , and (if  $p_t^* < 1$ ) in the productivity gain to democracy,  $K_\theta > 0$ . It is increasing or decreasing in the probability of autocracy, depending on the sign of  $\theta$ ,  $K_p \leq 0$  as  $\theta \geq 0$ .

Intuitively, a higher wage in the past period raises savings, and a better economy under democracy raises the returns to investment. If democracy has higher productivity,  $\theta > 0$ , a higher probability of autocracy reduces the expected return of investment, and vice versa if  $\theta < 0$ . Since the young's income is a parameter – and not determined by contemporaneous capital – the dynamics are simple. For constant  $p_t^*$  the economy converges to a constant capital stock (and GDP) in just one period.

### B. Politics and equilibrium transitions

Irrespective of the regime in the previous period, with probability  $\chi$ ,  $0 < \chi < 1$ , in each period an opportunity arises to change the current regime (i.e., an attempted coup under democracy, or an attempted uprising against an autocrat). The outcome of this attempted regime change depends on how many old citizens participate in a struggle to defend democracy. Specifically, conditional on the attempted coup or uprising, the probability that democracy will occur in period  $t$  is equal to the proportion of the old that participates in democracy's defense.<sup>2</sup>

The individual decision whether or not to participate in the struggle to defend democracy reflects a cost-benefit analysis. The cost is borne whether or not the regime change takes place. Its precise value is unknown, but each individual receives an individual-specific noisy signal of the true cost (see Web Appendix for details). The benefit is enjoyed only if democracy prevails, and it is denoted by  $b_t$ . Since each individual is rational but atomistic, she realizes that her individual contribution to the defense of democracy is negligible. Hence, this is a "warm glow" benefit that captures the psychological satisfaction of having contributed to a successful defense of democracy (or overthrow of a dictator).

We assume that there are two components to this benefit, defined as follows:

$$(5) \quad b_t = d + \theta k_t .$$

The first one,  $d$ , reflects cultural forces related to history or geography that lead to an appreciation of democracy; we refer to it as democratic capital. For now,  $d$  is just a known parameter, but in the empirical part it will develop in pace with democratic experience at home and abroad, and Section IV further discusses this assumption. Making  $d$  endogenous would introduce more complex equilibrium dynamics, and we leave this to future research. The second component of the perceived benefit from fighting for democracy, the term  $\theta k_t$ , reflects the relative economic efficiency of democracy vs. autocracy, from the perspective of the old. Thus, each old person internalizes the economic benefit of the political regime for their fellow old citizens, and if democracy is economically more efficient than autocracy she is more willing to defend it. This component too reflects cultural forces, and captures the idea that the root support for democracy

<sup>2</sup>We assume that the young do not take part in the defense of democracy. This assumption simplifies the analysis but can be relaxed at the cost of additional algebra.

also depends on how well it works compared to autocracy. Both assumptions play a central role in the comparative statics results on regime transitions discussed in Lemma 2 and in the empirical predictions below.

This formulation implies a strategic complementarity: if a larger share of the old are expected to fight for democracy, the probability of success is higher, which raises the expected benefit of fighting for democracy and draws even more people into the street. To obtain a unique equilibrium, we assume that the idiosyncratic signal about the cost of fighting for democracy satisfies the regularity condition for a global game. The Web Appendix proves that, under those conditions, the equilibrium probability of autocracy is a function

$$(6) \quad p_t^* = P(k_t^*, a_{t-1}; \theta, d),$$

such that:

**Lemma 2** *The equilibrium probability of autocracy is decreasing in democratic capital,  $P_d < 0$  and the productivity gains from democracy,  $P_\theta < 0$ . It is increasing or decreasing in the equilibrium capital stock,  $k_t^*$ , depending on whether productivity is higher or lower in democracy than in autocracy:  $P_k \lesseqgtr 0$  as  $\theta \gtrless 0$ .*

Intuitively, since both democratic capital,  $d$ , and the economic benefit of democracy,  $\theta$ , raise the benefit of fighting for democracy, a larger share of the old defends it. But a higher capital stock raises the benefit of fighting only if democracy produces better economic outcomes; hence, the ambiguity in the last part.

The Web Appendix also shows that political regimes are persistent, in the sense that

$$(7) \quad P(k_t^*, 1; \theta, d) = P(k_t^*, 0; \theta, d) + (1 - \chi),$$

i.e., the probability of autocracy is always strictly higher when starting under autocracy than under democracy. This is because an opportunity of changing the regime only arrives with probability  $\chi < 1$ .

### C. Reduced form equilibrium

The structural equations of the model (4) and (6) jointly determine equilibrium capital accumulation, the probability of democracy and, via

$$a_t^* = \begin{cases} 1 & \text{with prob } p_t^* \\ 0 & \text{with prob } (1 - p_t^*) \end{cases},$$

the (stochastic) evolution of the political regime, as functions of parameters  $(\theta, d, w)$  and predetermined political regime  $(a_{t-1})$ . These equations imply an “exclusion restriction”: democratic capital  $d$  does not influence capital accumulation directly, only through the probability of autocracy,  $p_t^*$ . We rely on this restriction to identify the effect of  $p_t^*$  on income in the empirical

analysis to follow.<sup>3</sup>

Equations (4) and (6) can be solved jointly to obtain the “recursive reduced form”:

$$(8) \quad \begin{aligned} p_t^* &= \tilde{P}(a_{t-1}; \theta, d, w) \\ k_t^* &= \tilde{K}(a_{t-1}; \theta, d, w). \end{aligned}$$

Under the additional assumption  $1/|K_p| > |P_k|$ , we obtain the following comparative statics results (see Web Appendix):

**Lemma 3** *The reduced-form expressions  $\tilde{P}$  and  $\tilde{K}$  have properties: (i)  $\tilde{P}_d < 0$ ,  $\tilde{K}_d \geq 0$  as  $\theta \geq 0$ , (ii)  $\tilde{K}_w > 0$ ,  $\tilde{P}_w \leq 0$  as  $\theta \geq 0$ , (iii)  $\tilde{P}_\theta < 0$  if  $\theta > \hat{\theta}_t$ , where  $\hat{\theta}_t < 0$ , and (iv)  $\tilde{P}(1; \theta, d, w) = \tilde{P}(0; \theta, d, w) + (1 - \chi) > \tilde{P}(0; \theta, d, w)$ .*

By (i), higher democratic capital reduces the probability of autocracy, since it raises the benefit of fighting for democracy. The induced effect on physical capital accumulation, however, depends on whether democracy is more ( $\theta > 0$ ) or less ( $\theta < 0$ ) productive than autocracy.

By (ii), higher income when young always increases capital accumulation, since it increases savings. The induced effect on the probability of autocracy is ambiguous, however. If democracy is more productive than autocracy ( $\theta > 0$ ), the old have stronger incentives to defend democracy in a richer economy (cf. (5)), but if democracy is less productive ( $\theta < 0$ ), the reverse is true.

By (iii), unless autocracy is much more productive than democracy (i.e., as long as  $\theta > \hat{\theta}_t$ , where  $\hat{\theta}_t < 0$ ), higher productivity of democracy reduces the probability of autocracy, as citizens are more willing to defend democracy if it is economically more efficient.

Finally, (iv) restates the political persistence result. By (iv), the three previous comparative statics results apply irrespective of the lagged regime.

#### D. Equilibrium sorting into political regimes

Lemmas 1-3 characterize the equilibrium for a given country. These results are incomplete as a basis for testable predictions, however, as they depend on the relative efficiency of democracy vs. autocracy – parameter  $\theta$ , which we do not observe. But the model implies that transition rates between political regimes depend systematically on  $\theta$ . This means that countries with different values of  $\theta$  systematically sort themselves into political regimes. In the next section, we exploit this fact to derive predictions about the comparative statics conditional on the political regime,  $a = 0, 1$ , which we do observe.

To obtain tractable analytic solutions, assume the world has a two-type distribution of  $\theta$  and a continuum of countries with mass 1. A fraction,  $\lambda$ , of these benefits from democracy, so  $\theta$  takes a positive value,  $\theta = \bar{\theta} > 0$ , while the remaining fraction,  $1 - \lambda$ , is economically better off under autocracy,  $\theta = \underline{\theta} < 0$ . The values of  $\bar{\theta}$  and  $\underline{\theta}$  are arbitrary, except that  $\underline{\theta}$  is assumed to be not too far below  $\hat{\theta} < 0$ , where  $\hat{\theta}$  is defined in the proof of Lemma 3. Let  $\bar{n}_t^a$  (resp.  $\underline{n}_t^a$ ) denote the

<sup>3</sup>The theory has other restrictions, e.g., that income when young,  $w$  does not affect the probability of regime change directly, but only through the capital stock  $k_t^*$ . As shown in a prior version of the paper, however, these additional implications are not robust to using a standard neoclassical production function with decreasing marginal returns to labor and capital.



fraction of countries with  $\theta = \bar{\theta}$  (resp  $\theta = \underline{\theta}$ ) in regime  $a = 0, 1$  in period  $t$ . By assumption, the  $\bar{\theta}$  countries sum to  $\lambda$ , while the  $\underline{\theta}$  countries sum to  $1 - \lambda$ , so the world ratio of high- $\theta$  to low- $\theta$  countries is  $\lambda/(1 - \lambda)$ .

The Web Appendix shows that the country fractions converge monotonically to a steady state, where

$$\bar{n}^0/\underline{n}^0 > \lambda/(1 - \lambda) > \bar{n}^1/\underline{n}^1.$$

Thus, we have non-random sample selection: high- $\theta$  countries are eventually over-represented among the democracies ( $a = 0$ ) and under-represented among the autocracies ( $a = 1$ ), and vice versa for low- $\theta$  countries. Since a random allocation of countries across regimes produces the same odds ratio,  $\lambda/(1 - \lambda)$ , among high- $\theta$  and low- $\theta$  countries, monotonic convergence implies:

**Lemma 4** *If countries start out randomly allocated across political regimes, we have  $\bar{n}_t^0/\underline{n}_t^0 > \lambda/(1 - \lambda) > \bar{n}_t^1/\underline{n}_t^1$  in any time period  $t > 0$ .*

### III. Empirical predictions

We now formulate testable predictions. Because countries endogenously sort themselves by (unobservable) type  $\theta$  and the comparative statics depend on  $\theta$ , we get systematic differences by (observable) political regime. Hence, in most of our empirical work we estimate parameters of interest separately for countries under democracy and autocracy. The world is assumed to have started out from a random allocation of countries to political regimes (or be close enough to a steady state), so we can invoke Lemma 4.

One concern in going from model to data is that we observe per capita income,  $y$ , rather than physical capital,  $k$ , or wage income,  $w$ . However, the model's expression for GDP per capita,  $y_t = A(a_t)k_t + w$ , allow us to re-express the predictions over observables. Our dependent variables of interest are the probability of regime changes,  $p_t^*$ , and GDP per capita,  $y_t$ .

Empirically, we treat parameters  $d$  and  $w$  as observable independent variables. Section IV discusses how to measure democratic capital,  $d$ . Wage income,  $w$ , corresponds to lagged income in the data, since it affects  $k_t$  (and hence  $y_t$  and  $p_t^*$ ) through the young's investment in period  $t - 1$  (recall (3)). From here on, we thus refer to  $w$  as lagged income. The type-parameters  $\theta$  and share-parameter  $\lambda$  are treated as genuinely unobserved. Due to non-random selection into democracies and autocracies, however, results in these subsamples give us indirect information about the unobservable parameters.

Let  $x$  be a vector of observables: democratic capital ( $d$ ) and lagged income ( $w$ ) and  $\frac{\partial p_t^a}{\partial x}$  the average effect of  $x$  on the probability of autocracy in  $t$ , in the samples of democracies ( $a = 0$ ) and autocracies ( $a = 1$ ) in  $t - 1$ . The *reduced form* expressions imply:

$$(9) \quad \frac{\partial p_t^a}{\partial x} = \frac{\bar{n}_{t-1}^a}{\bar{n}_{t-1}^a + \underline{n}_{t-1}^a} \tilde{P}_x(a_{t-1}; \bar{\theta}, x) + \frac{\underline{n}_{t-1}^a}{\bar{n}_{t-1}^a + \underline{n}_{t-1}^a} \tilde{P}_x(a_{t-1}; \underline{\theta}, x), \quad a_{t-1} = 0, 1$$

where  $\tilde{P}_x$  denotes the (vector of) partial derivative(s) of  $\tilde{P}(\cdot)$  with respect to  $d$  and  $w$ .

The first step in the empirical analysis is to estimate these partial effects from the reduced form expressions  $\tilde{P}(\cdot)$  in each of the two samples. As shown in the Web Appendix, Lemmas 3 and 4 imply:

**Prediction 1** (a) *Democratic capital decreases the probability of autocracy in the samples corresponding to both political regimes:  $\frac{\partial p_t^a}{\partial d} < 0$  for  $a_{t-1} = 0, 1$ .* (b) *Lagged income has a smaller algebraic effect on the probability of autocracy in the democracy sample than in the autocracy sample:  $\frac{\partial p_t^0}{\partial w} < \frac{\partial p_t^1}{\partial w}$ .* (c) *Higher lagged income can increase or decrease the probability of autocracy in both samples:  $\frac{\partial p_t^a}{\partial w} \lesseqgtr 0$  for  $a_{t-1} = 0, 1$ . But if  $\lambda$  is large enough, higher lagged income reduces the probability of autocracy in the sample of democracies (or even in both samples).*

Part (a) of the prediction follows from Lemma 3, as higher democratic capital reduces the probability of autocracy irrespective of the value of  $\theta$ . Part (b) is about the relative effects of lagged income in democracy and autocracy. Intuitively, higher lagged income raises investment. A higher capital stock raises the value of defending democracy if  $\theta = \bar{\theta} > 0$ , but has the opposite effect if  $\theta = \underline{\theta} < 0$ , making the first derivative in (9) negative and the second one positive. By sorting, however, more  $\bar{\theta}$  countries find themselves under democracy than under autocracy, so the weight on the negative derivative is larger under democracy than under autocracy, which makes the algebraic effect of  $w$  smaller under democracy. Finally, part (c) concerns the *sign* of lagged income. As the weight on the negative derivative in (9) is increasing in  $\lambda$ , while the weight on the positive derivative is decreasing in  $\lambda$ , the negative term prevails for a high enough value of  $\lambda$ . If we find that higher lagged income reduces the risk of exit from democracy, this constitutes indirect evidence that, in a large enough number of countries, democracy is favorable to economic development.

Next, let  $\frac{\partial y_t^a}{\partial p_t^*}$  denote the average effect on period  $t$  income of the probability of autocracy in  $t$ , within the samples of countries in state  $a = 0, 1$  in period  $t$ . The *structural form* (4) implies the following average effect in the two regimes:

$$(10) \quad \frac{\partial y_t^a}{\partial p_t^*} = \frac{\bar{n}_t^a}{\bar{n}_t^a + \underline{n}_t^a} A(a_t) K_p(p_t^*; \bar{\theta}, w) + \frac{\underline{n}_t^a}{\bar{n}_t^a + \underline{n}_t^a} A(a_t) K_p(p_t^*; \underline{\theta}, w), \quad a_t = 0, 1$$

Our second step in the empirical part is to estimate this structural-form expression separately in the sample of democracies and autocracies.<sup>4</sup> As further discussed below, we may identify the effect of expected regime changes on per-capita income by the exclusion restriction that democratic capital,  $d$ , does not enter the structural form  $K(\cdot)$ , except through  $p_t^*$ . Here, we have:

**Prediction 2** (a) *The probability of autocracy has a smaller algebraic effect on income in the democracy sample than in the autocracy sample:  $\frac{\partial y_t^0}{\partial p_t^*} < \frac{\partial y_t^1}{\partial p_t^*}$ .* (b) *A higher probability of autocracy can increase or decrease income in both samples:  $\frac{\partial y_t^a}{\partial p_t^*} \lesseqgtr 0$ . But if  $\lambda$  is large enough, a higher probability of autocracy decreases income in the sample of democracies (or even in both samples).*

Prediction 2 follows from Lemmas 2 and 4, by analogous arguments as in the proof of Prediction 1. By part (b), a finding that the probability of autocracy decreases growth is indirect evidence that, on average, democracy is favorable to economic development.

<sup>4</sup>In the empirical analysis the dependent variable is actually the growth rate rather than the level of income (see Section V).

Finally, the *reduced form* of the model shows how democratic capital,  $d$ , influences per capita income in each regime and in the full sample of countries. Our third empirical step is to estimate a reduced form for the sample of all countries, where we condition on the political regime in period  $t - 1$ .

The average effect of democratic capital on income, in each sample, is:

$$(11) \quad \frac{\partial y_t^a}{\partial d} = \frac{\bar{n}_{t-1}^a}{\bar{n}_{t-1}^a + \underline{n}_{t-1}^a} A(a_t) \tilde{K}_d(a_{t-1}; \bar{\theta}, d, w) + \frac{\underline{n}_{t-1}^a}{\bar{n}_{t-1}^a + \underline{n}_{t-1}^a} A(a_t) \tilde{K}_d(a_{t-1}; \underline{\theta}, d, w) .$$

Lemmas 3 and 4 imply:

**Prediction 3** (a) *Democratic capital has a larger effect on income in democracies than in autocracies:*  $\frac{\partial y_t^0}{\partial d} > \frac{\partial y_t^1}{\partial d}$ . (b) *Higher democratic capital can increase or decrease income in both regimes:*  $\frac{\partial y_t^a}{\partial d} \gtrless 0$ . *But if  $\lambda$  is large enough, democratic capital raises income in democracies (or even in both samples).*

The prediction follows from the same logic as in Predictions 1 and 2. As above, a finding that democratic capital raises growth in democracies is indirect evidence that democracy stimulates development in a large share of countries.

We can thus summarize the empirical predictions as follows: First, higher democratic capital reduces the probability of autocracy, under both political regimes. Second, if the fraction  $\lambda$  of countries that benefit from democracy is large enough, this in turn raises average growth under democracy, because it gives more stability to this more efficient regime. Third, due to endogenous sorting, the asymmetries across regimes are linked: on the one hand, a higher probability of autocracy has a larger algebraic effect on growth under autocracy, because the countries more often under this regime are less hurt by it; on the other hand, as income rises the effect on the probability of autocracy is also larger algebraically for the current autocracies, because the economic benefits of switching to democracy are smaller for this group of countries.

Our model also implies that conventional tests of how democracy affects growth are likely to be biased. To see this, consider an autocracy which would benefit from democracy ( $\theta = \bar{\theta}$ ). In such a country, mounting expectations of a democratic transition gradually raise expected returns (as  $p_t^*$  falls) and the investment responses gradually raise income. If an actual democratization occurs, income goes up, but much of the adjustment has already taken place. An econometrician estimating how regime changes affect income, e.g., by running difference-in-differences of income on a democracy indicator, will underestimate the true effect. Our tests of Prediction 2 will establish a link from *expected* regime changes to growth, but we will not consider (directly) the effect of *actual* regime changes. Torsten Persson and Tabellini (2006a, b) do find, however, that the estimated effect of actual transitions into democracy is higher when one controls for expected regime changes.

#### IV. Data

We use annual data on economic development and political regimes for as many countries as far back as possible. The resulting panel is unbalanced, because of data availability and countries

entering only in the year of independence. The Web Appendix gives detailed definitions and sources of all our variables.

Output (GDP) per capita (expressed as natural logs) for country  $i$  and year  $t$ ,  $y_{i,t}$ , is obtained from Angus Maddison (2001), who reports uninterrupted data from 2000 backwards as far back as to 1870 for a number of countries, and to 1820 for a few countries.

As in the model, we treat the political regime as a binary variable,  $a_{i,t} = 0, 1$ . We measure it from two alternative sources. Our main definition of democracy is based on Polity IV data, available for all countries above 1/2 million inhabitants from 1800 until 2000. Specifically, we set  $a_{i,t} = 1$  if the *polity2* variable takes a strictly positive value, and  $a_{i,t} = 0$  otherwise. This variable has a maximum of 10 and a minimum of  $-10$ , depending on the status of six different aspects of political institutions, with a focus on executive powers, executive selection and the freedom of elections. Our definition entails a rather generous definition of democracy, relative to others in the literature. It has the advantage of capturing non-gradual transformations of the political regime (the average change in the underlying *polity2* variable when  $a_{i,t}$  goes from 0 to 1, or vice versa, is more than 8).

Our second definition is based on Carles Boix and Sebastian Rosato's (2001) extension of the measure constructed by Przeworski et al. (2000). This democracy measure is more narrow than the Polity variable, and emphasizes turnover of political power in free and fair elections. It is a binary variable, available from 1800 until 1994. In the few cases when the Boix and Rosato variable is missing, while the Polity IV variable is not (e.g., Boix and Rosato do not code transition years, while *polity2* interpolates such years), we supplement the former with the latter.

By both measures, political change varies a great deal across countries. Some nations, such as Afghanistan, China, and Morocco, never experience a transition into democracy. Others, like Australia and Canada, start out as democracies right at independence and never relapse into autocracy. Yet others, as Costa Rica and Denmark, start out autocratic and make a single transition into democracy. Many countries have a more eventful history, however, with intermittent spells of democracy and autocracy. According to the Polity IV measure, Guatemala is the most extreme, having gone through six periods each of democracy and autocracy since independence in 1839.

The intersection of the economic and political data give us annual data for about 150 countries over at most 180 years. Data definitions and sources for all the variables we use are found in a data appendix.

#### A. How to measure democratic capital?

To test Predictions 1-3 in Section III, we need an operational definition of democratic capital,  $d$ . In the model, this is a stock of civic values that affects people's willingness to stand up for democracy. For a narrow set of countries and a short time period, one could think of imaginative ways of measuring democratic capital. With the sparse data in our long historical panel, we create two variables by making specific assumptions about democratic capital accumulation.

The first is *domestic* democratic capital, denoted by  $z_{i,t}$ . We assume  $z$  to accumulate over time, as members of society gradually gain experience with democracy. A number of mechanisms could make a long-standing democracy more resilient to a coup than a short-standing one, including build-up of formal and informal institutions from political parties to social norms. The same institutions would make the re-institution of democracy more likely in a nation that lapses

back into autocracy. Since such institutions might also have a direct impact on public policies, we do *not* impose the exclusion restriction that this component of democratic capital *only* influences economic outcomes through the probability of regime change.

The idea that the willingness of citizens to stand up for democracy increases with experience under this regime has intuitive appeal, and is consistent with recent analytical results. For instance, Tabellini (2008b) formulates a model of cultural transmission across generations. In that model, the institutional framework shapes the values that parents transmit to children, and better enforcement of the rule of law facilitates the gradual diffusion of generalized respect for the rights of others. Individual data also confirm that distant political history shapes attitudes and values: as shown by Tabellini (2008a), second-generation US citizens whose ancestors came from more democratic countries today display more generalized trust (a civic virtue that is correlated with active political participation).

To define domestic democratic capital, we need to specify how a particular historical path in country  $i$  up to year  $t$ ,  $\{a_{i,t-\tau}\}_{\tau=t-t_0}^{\tau=0}$ , maps into a value of  $z_{i,t}$ . We are agnostic about functional form. The simplest assumption is that democratic capital accumulates in years of democracy, and depreciates geometrically, at the rate  $(1 - \delta)$ , in years of autocracy:  $z_{i,t} = (1 - a_{i,t}) + \delta z_{i,t-1}$ . We then solve backwards to obtain (assuming  $z_{i,t_0} = 0$ ):

$$(12) \quad z(\delta)_{i,t} = (1 - \delta) \sum_{\tau=0}^{\tau=t-t_0} (1 - a_{i,t-\tau}) \delta^{\tau} ,$$

where  $t_0$  is either the year of independence or 1800, whichever comes last. Thus, democratic experience is more valuable the closer it is to the present. Note that uninterrupted democracy makes  $z_{i,t}$  eventually converge to a steady-state value. We use the notation  $z(\delta)$  to emphasize the dependence on the depreciation rate, and multiply with  $(1 - \delta)$  such that the resulting expression is scaled to  $[0, 1]$ . As further discussed below,  $\delta$  is estimated from the data.

Figure 2 illustrates the time path of domestic democratic capital in Spain and Sweden, for two depreciation rates:  $\delta = 0.94$  (in part a) and  $0.99$  (in part b), the maximum and minimum values we estimate below.<sup>5</sup> As the political history of the two countries is very different, so is the time path of their domestic democratic capital. Sweden gains democratic status in 1910, and uninterrupted democracy brings continued accumulation until 2000. Spain first becomes a democracy in the early 1870s, but its volatile political history implies a highly non-monotonic path of democratic capital. The higher depreciation rate of 0.06 in part a makes the paths of domestic democratic capital steeper – in democracy, as well as autocracy – than in part b where the depreciation rate is 0.01. A higher depreciation rate makes the Spanish relapses into autocracy more costly and, as a result, Sweden's domestic democratic capital catches up with Spain's around 1930, rather than around 1950. Moreover, in part a Sweden has more or less converged to the democratic steady state (of 1) by the year 2000, whereas it has 40 percent of the way to go in part b.

The second component of democratic capital is based on political conditions abroad. It is easy to imagine how experience with democracy in foreign, neighboring countries could spill over into

<sup>5</sup>The definition of democracy used in Figure 2 is that by Polity IV.

greater domestic appreciation of democracy and greater willingness to defend these values; think about the orange revolution in the Ukraine. We do not directly observe these spillovers, however, and parsimoniously define *foreign* democratic capital, labeled  $f_{i,t}$ , to measure a country's "closeness to democracy", given the prevalence of democracy in neighboring countries. We tried different specifications with closeness corresponding to geography, history or culture, and both a discrete or a continuous definition of democracy. In the end we chose the definition with stronger explanatory power in the hazard rate regressions estimated below, and this corresponds to a geographic definition of closeness and a continuous measure of democracy based on the PolityIV data.

Specifically, for country  $i$  and year  $t$ , we define  $f_{i,t}$  by

$$(13) \quad f(\rho)_{i,t} = \sum_{j \neq i} (1 - a_{j,t}) \varpi(\rho)_t^{i,j},$$

where  $a_{j,t}$  is a measure of the degree of democracy in country  $j$  in year  $t$ . The weights  $\varpi(\rho)_t^{i,j}$  falls in the distance between  $i$  and  $j$  and drops to zero for distance outside radius  $\rho$ . The dependence on  $t$  reflects the varying number of countries in the sample, and the dependence on  $\rho$  is emphasized by the notation  $f(\rho)$ . We replace  $(1 - a_{j,t})$  by country  $j$ 's continuous *polity2* score and divide by 10, such that the resulting expression is scaled to  $[0, 1]$ .<sup>6</sup> Like  $\delta$ ,  $\rho$  is estimated from the data.

Figure 3 illustrates the time paths of foreign democratic capital in Belgium and Chile, when  $\rho = 1$ , so every country  $j$  in the world is included in the neighborhood.<sup>7</sup> The two variables share a general time pattern, reflecting the gradual adoption of democracy throughout the 19th century and three waves of democratization in the 20th century (see Samuel Huntington, 1991). Belgium's foreign democratic capital is more variable than that of Chile's because Belgium is closer to the coincident political transitions deteriorations – in the early 1990s, the interwar period, and the 1990s – of democratic conditions across Europe, whereas Chile is closer to the more dispersed political transitions in Latin America.

Do our boldly constructed measures of democratic capital pick up anything close to citizens' valuation of democracy? Reassuringly, both measures are strongly correlated with citizens' opinions about the value of democracy as a form of government in a large cross section of countries. In the late 1990s, the World Value Surveys asked individuals of about 60 developing and developed countries to rank (on a 1 to 4 scale) their agreement with the claim: "Democracy may have problems but it's better than any other form of government". We use average responses in each country to measure valuations of democracy.<sup>8</sup>

In column 1 of Table 1, we regress average country responses on domestic and foreign demo-

<sup>6</sup>Specifically, let  $D^{i,j}$  be the (time-invariant) great circle distance between the capitals in  $i$  and  $j$ ,  $D$  be half the length of the equator, and  $N_t$  be the number of independent countries in the world with a *polity2* score in year  $t$ . Then, we impose  $\varpi(\rho)_t^{i,j} = (1 - \frac{D^{i,j}}{D})/N_t$  if  $\frac{D^{i,j}}{D} \leq \rho$ , and  $\varpi(\rho)_t^{i,j} = 0$  if  $\frac{D^{i,j}}{D} > \rho$ .

<sup>7</sup>Since  $f(\rho)_{i,t}$  is only defined by the neighbors of country  $i$ , we can draw the (hypothetical) foreign democratic capital of Belgium and Chile before the years in which they become independent nations (in 1830 and 1818, respectively).

<sup>8</sup>In the World Value Surveys, a value of 1 corresponds to strong agreement, while 4 corresponds to strong disagreement with the statement in the text. In Table 1 and Figure 3 below, we measure the appreciation of democracy as 4 minus the country average response (times a 100); thus, higher values correspond to more appreciation for democracy.

cratic capital in 1999. Both are strongly and significantly correlated with appreciation of democracy. The following columns show that the correlation persists, and becomes stronger, when we control for economic development, the current political regime and human capital (gauged by average school attainment as in Barro and Jong-Wha Lee, 2000), all measured in 1999. Figure 4 shows that the correlations in Table 1 (column 4) are not due to outliers. Interestingly, columns 3 and 4 of the table suggest that the appreciation of democratic form of government is higher in autocracies than in democracies, once we control for democratic capital. These estimates confirm that our measures of democratic capital are not empty. Individuals indeed value democracy more if they live in a country with long democratic experience and surrounded by other democracies, irrespective of income, average education and the political regime.

Below, we follow the model and assume that foreign (though not domestic) democratic capital influences political transitions, but has no direct effect on growth. This exclusion restriction cannot be tested. But we can at least look at the correlation between democratic capital and growth-promoting institutions, exploiting a widely used measure of property rights protection (also based on perceptions). *Government Anti Diversion Policies (GADP)* is used by Hall and Jones (1999) and many others in the macroeconomic development literature to capture the effect of institutions on economic development. Columns 6 and 7 of Table 1 show that, controlling for per-capita income, current democracy, and human capital, no positive correlation is left between perceptions of property rights protection and democratic capital. If anything, foreign democratic capital appears negatively – not positively – correlated with the protection of property rights. This reassures us that the exclusion restriction may not be grossly inconsistent with the data.

## V. Political transitions

We begin our empirical study by estimating the probability of exit from democracy and autocracy. Specifically, we test all three parts of Prediction 1 in Section III, namely: (a) *The effect of democratic capital*. Measured by its two components, democratic capital reduces the probability of autocracy, irrespective of the current regime. (b) *The relative effect of per capita income*. If democracy has heterogeneous effects across countries – and sorting has taken place – per capita income has a smaller algebraic effect on the probability of autocracy in the democracy sample than in the autocracy sample. (c) *The absolute effect of per capita income*. If democracy is more productive than autocracy in a large enough share of countries (the fraction  $\lambda$  with  $\theta = \bar{\theta} > 0$  in the model), per capita income reduces the probability of autocracy for countries currently in democracy.

### A. Econometric specification

Which econometric specification do we bring to the data? To take right-censoring of our data on political regimes into account, we estimate the risk of exit from the current regime – i.e., a hazard rate,  $h_t^a$ , where  $a$  denotes the regime in year  $t - 1$ . In model notation, we estimate the reduced form of the model,  $h_t^0 = \tilde{P}(\cdot)$  and  $h_t^1 = 1 - \tilde{P}(\cdot)$  for the countries that were democratic resp. autocratic in year  $t - 1$ . Following the model's distributional assumption, the hazard rates are specified as probit. We replace  $d$  with domestic and foreign democratic capital,  $z(\delta)_{i,t-1}$  and  $f(\rho)_{i,t-1}$ , as defined in the Section IV, and  $w$  with lagged income  $y_{i,t-1}$ , as explained in Section

III. Some specifications include a number of fixed and time varying controls  $\mathbf{x}_{i,t}$  to reflect country and time-specific probabilities of a coup or an uprising, corresponding to parameter  $\chi$  in the model. In summary, we estimate spell-specific hazard rates of the form:

$$(14) \quad h_{i,t}^a = H^a(z(\delta)_{i,t-1}, f(\rho)_{i,t-1}, y_{i,t-1}, \mathbf{x}_{i,t}) + \psi_{i,t}, \quad a = 0, 1,$$

where  $\psi_{i,t}$  is an error term.

How do we carry out the estimation? Our democratic-capital variables are only defined up to parameters  $\delta$  and  $\rho$ , which enter both hazard rates. Using the definitions in (12) and (13) and imposing the constraint that  $\delta$  and  $\rho$  be equal across the two hazard rates, we obtain a well-defined likelihood function. With many regime shifts for a number of countries and many country pairs, the likelihood is highly non-linear. To find the maximum likelihood values, we first fix the values for  $\delta$  and  $\rho$ , estimate all the other parameters, and compute the value of the likelihood function. We then repeat the same procedure for a large range of values of  $\delta$  and  $\rho$ , to create an envelope to the likelihood function. Finally, we select the values of  $\delta$  and  $\rho$  (and other parameters) at the maximum of the envelope likelihood function. This yields maximum-likelihood estimates of our coefficients of interest, except that the standard errors treat the parameters  $\delta$  and  $\rho$  as known (rather than estimated).

For  $\rho$ , this iterative procedure always yields the same maximum independently of specification, namely  $\rho = 1$ , i.e., each country's neighborhood includes all countries in the world (although with weights declining in distance). For  $\delta$ , the maximum depends on the specified hazard rate, but always lies in the interval  $[0.94, 0.99]$  – see further below.

### B. Basic results

The most parsimonious specification only includes the variables of interest: domestic and foreign democratic capital and lagged per-capita income. With this specification, the maximum-likelihood estimate of the depreciation rate of democratic capital is  $\delta = 0.94$  (and the neighborhood radius  $\rho = 1$ ). Table 2 reports the effects on the probability of autocracy: column 1 shows estimates of the hazard rate out of democracy, while column 2 refers to the *negative of* the hazard rate out of autocracy. The same convention applies to the rest of Table 2 and to Table 3.

The tables display robust standard errors. We have also estimated with standard errors clustered by country, but these are very similar to the robust ones, and never change the levels of significance reported in the tables.

The estimated coefficients on both components of democratic capital are highly statistically significant and have the sign expected from Prediction 1(a): democratic capital reduces the probability of autocracy in the following year, conditional on being in democracy (column 1) or autocracy (column 2) in this year. This is in accordance with earlier results by Barro (1997) and Barro (1999), who had stressed that current democracy is explained by lagged democracy, although our measure of domestic democratic capital is more backward looking.

Higher income significantly reduces the probability of exiting democracy. According to Prediction 1(c), this is indirect evidence that democracy is more productive than autocracy for a large enough fraction  $\lambda$  of countries. The income effect is much weaker, and not significantly different from zero in the autocracy sample, however (column 2). This asymmetric income effect is



consistent with Prediction 1(b) of our model and reproduces earlier findings by Przeworski et al. (2000) and (conditional on the specification) by Acemoglu et al. (2008). We do not formally test that the estimated coefficients on income are different in the two regressions, as we don't have an estimated covariance between the two error terms. But since the 99 percent confidence intervals of the two estimated coefficients do not overlap, equality of the coefficients would be rejected unless the covariance between the errors were negative and large (in absolute value).

In terms of our model, the column 1 and 2 specifications assume that the opportunity of regime change is the same across countries and time: i.e., parameter  $\chi$  is the same for all  $i$  and  $t$ . We relax this in columns 3-6 by adding other covariates: indicators for years  $t$  in which country  $i$  was at war (contemporaneous and lagged once); a flexible polynomial in time to capture worldwide waves in the incidence of democracy (see the next subsection for more on time variation); several indicators for fixed-country characteristics such as legal and colonial history, geographic location, democracy at the year of independence, an indicator for (the few) countries that switched regimes more than 5 times. Columns 3 and 4 use the Polity IV definition of democracy, while columns 5 and 6 use the Boix and Rosato definition for an (almost) identical specification of the hazard rates (see the note to Table 2). With this specification, the ML estimates of the depreciation rate of democratic capital are  $\delta = 0.99$  (columns 3 and 4) and  $\delta = 0.97$  (columns 5 and 6), respectively.

Evidently, the results from the most parsimonious specifications hold up well. In particular, democratic capital always reduces the probability of autocracy in both regimes, while income reduces the probability of autocracy in democracies, but not in autocracies.<sup>9</sup> Among the other results (not reported), wars are destabilizing both for democracies and autocracies, but the effect on autocracies manifests itself a year after the war, while it is contemporaneous in democracies. Countries starting out with stronger constraints on the executive are more stable as democracies, but no less stable as autocracies (given domestic democratic capital). Most of the other historical or geographic dummy variables have statistically significant coefficients, as do the first and second component of the polynomial in time. These results are robust to alternative specifications with similar controls.

Overall, exit from democracy is more successfully explained than exit from autocracy. The pseudo  $R$ -square (roughly the percent increment in outcomes correctly predicted by the model, relative to a model with only a constant) is about 22 percent for exit out of democracy vs. half of that out of autocracy. Note that the estimated *annual* probabilities of transitions are not very high. Figure 5 shows frequency distributions of the estimated probabilities, based on the specifications in columns 3 and 4 of Table 2. Although the probability of exit from democracy (autocracy) is as high as 50 percent (30 percent) for a few observations, most of the probability mass is concentrated between 0 and 10 percent with average hazard rates around 2-3 percent.

Political transitions are thus relatively rare, and political regimes are quite persistent in line with our model. But the determinants of interest have substantial effects on the annual probability of transition. By the point estimates in columns 3 and 4, a one-time jump of domestic democratic capital from its minimum of 0 to its maximum of 1 would reduce the (average) probability of autocracy by almost 2 percentage points conditional on being in democracy, and by almost 5

<sup>9</sup>Again, the 99 percent (resp. 95 percent) confidence intervals of the estimated coefficients for income in columns 3 and 4 (resp. columns 5 and 6) don't overlap, which implies that we would not be able to reject equality of coefficients unless their covariance was negative and large in absolute value.

percentage points conditional on being in autocracy, i.e., close to or above the average transition probabilities in the sample. According to the same estimates, a hike in foreign democratic capital of about 0.4 – corresponding to the change in European countries from 1970 to 2000 (cf. Figure 3) – reduces the probability of autocracy by about 2 and 3.5 percentage points under democracy and autocracy, respectively.

A possible concern is that our results simply reflect increasing levels of schooling and human capital. Already Lipset (1959) and Almond and Verba (1963) pointed to the correlation between education and attitudes towards democracy. Recently, Glaeser, Ponzetto and Shleifer (2007) present additional evidence of such a correlation, motivated by a theoretical model where education increases the participation of citizens in support of democracy. To check for this possibility, we use annual indicators of education referring to population above 25 years of age constructed in Persson (2005) (who interpolated the five-year observations in Barro and Lee, 2000). This measure of human capital, available only from 1960 onwards, is closely correlated with domestic democratic capital, on the order of 0.6 in the full sample of countries. Columns 7 and 8 of Table 2 show the results for our most parsimonious specification (identical to that in columns 1 and 2), except for the sample period and the addition of human capital (we retain  $\delta = 0.94$ ). The estimate is indeed statistically significant with the expected sign: education reduces the probability of autocracy in both regimes. However, all parts of Prediction 1 continue to hold. Thus, our two measures of democratic capital remain significant under democracy and autocracy, and the point estimates are even higher in absolute value. Furthermore, the negative effect of income on the probability of exit from autocracy, as well as the differential income effects under democracy vs. autocracy, continue to hold.

### C. *Unobserved heterogeneity and common shocks*

The regressions in Table 2 pool together spells from all countries, exploiting both within and across country variation. This raises an important concern familiar from labor economics: state dependence versus unobserved heterogeneity. As a first check, we estimate a random-effects model. The “LR-test” row in Table 2 reports the  $p$ -value of a likelihood-ratio test of random effects against our pooled specification, i.e., a test of the null that the share of the variance explained by the random effects is zero. As shown in columns 1 and 2, this test has bite: we can reject absence of random country effects. With the more comprehensive specification in columns 3-6, however, the test-statistic indicates that the random country effects no longer explain a significant fraction of the variance. Although this test relies on a specific functional form of the hazard function, the results are robust to alternative functional forms.<sup>10</sup>

The LR-test refers to random and time-invariant omitted variables orthogonal to the other regressors. But unobserved heterogeneous effects of democracy on productivity, like the parameter  $\theta$  in our model, are likely correlated with domestic democratic capital, because sorting will take high- $\theta$  countries to democracy more often. To allow for a country effect correlated with the re-

<sup>10</sup>The LR tests in Table 2 are based on estimates of a random effects logit (vs. a pooled logit), which is consistent with a proportional hazard model with a logistic hazard and a normally distributed random country effect (see e.g., Stephen Jenkins, 2004). Similar results are obtained with a complementary loglog model. In these random effects estimates, both components of democratic capital remain highly significant in the hazard functions out of democracy, while foreign (but not domestic) democratic capital remains significant in the hazard function out of autocracy.

gressors, we can estimate the hazard rates by conditional logit including country fixed effects. This identifies the coefficients of interest from within-country variation only, exploiting only the countries that have *completed* at least one spell in the relevant regime.

Columns 1 and 2 of Table 3 show such fixed-effects estimates.<sup>11</sup> These are partly inconsistent with Prediction 1(a): the coefficients on domestic democratic capital are positive rather than negative, and statistically significant. All other parts of Prediction 1 hold up, however. Specifically, the negative effect of foreign democratic capital on both hazard rates remains robust and so does the negative effect of per-capita income on exits from autocracy, as well as the differential effect of per-capita income in democracy vs. autocracy.<sup>12</sup>

Do these results mean that domestic democratic capital necessarily captures unobserved heterogeneity rather than state dependence? Not necessarily. One reason is that the estimator drastically reduces both our samples. Because fixed-country effects perfectly predict non-exits, when estimating the risk of exit from democracy we cannot include the last spell of any long-lived democracy that never leaves its last democratic regime. Similarly, we have to drop all countries experiencing long spells of autocracy without ever becoming democracies when estimating the risk of exit from autocracy. As a result, we lose 58 out of 112 countries (and more than 60 percent of the observations) in the democracy sample, and 43 out of 118 countries (and more than 30 percent of the observations) of the autocracy sample. While we address unobserved heterogeneity, we might introduce possibly more serious selection bias by excluding all these long-lived regimes, which are obviously essential to capture the effects of domestic democratic capital on non-exit.

Is the difference in the results between Tables 2 and 3 due to the selected sample or to the inclusion of country fixed effects? To check this, we can re-estimate the specification in Table 2, column 3 without the fixed effects, on the same selected sample as in Table 3, column 1. Doing this, the estimated coefficient on domestic democratic capital is very close to zero, and insignificantly different from zero (results not reported). Thus, the difference between the two tables seems to be due to the small selected sample as well as the fixed effects: restricting the sample kills the statistical significance of the estimated coefficient on democratic capital, while adding the country fixed effects turns it positive and significant. In the autocracies sample, however, the same exercise gives a different result: without the fixed effects, but in the restricted sample, the estimated coefficient of domestic democratic capital remains negative and significant, as in Table 2. This suggests that unobserved heterogeneity rather than the selected sample may be the major problem.

There is a second reason to argue that the estimated coefficient on domestic democratic capital reflects an independent causal effect, however. If the estimated coefficient of domestic democratic capital *only* reflected time-invariant unobserved heterogeneity, we should find that distant and more recent democratic experience has similar effects. Our estimate of a depreciation rate in the range of 1 percent to 6 percent suggests otherwise. To further assess the contribution of distant vs. recent democratic experience, column 3 of Table 3 estimates the risk of exit from democracy, using the specification of column 3 in Table 2 but replacing domestic democratic capital with

<sup>11</sup>Throughout Table 3 we retain the values of  $\delta = 0.99$  and  $\rho = 1$  used in the comprehensive specification of Table 2.

<sup>12</sup>The findings of income and democracy contrast with those of Acemoglu et al. (2008), although these authors use as the dependent value the full discrete (21-step) polity2 score rather than a binary indicator.

two components. “Current domestic democratic capital” is the part accumulated in the current democratic spell, i.e., a variable that starts at zero in each new democratic spell. “Past democratic capital” is the remaining part. Only the current component is statistically significant, while more distant democratic history does not seem to matter, suggesting that domestic democratic capital does indeed pick up some true state dependence and not just time-invariant unobserved heterogeneity.

We then ask the same question about the risk of remaining under autocracy. Here, we cannot decompose democratic capital into current and past, because no democratic capital is accumulated under autocracy. Instead, in column 4 we add the duration of the current autocratic spell as a regressor to our basic specification. Perhaps surprisingly, the duration of the current spell is not statistically significant, providing some evidence against a simple version of unobserved heterogeneity, while domestic democratic capital retains its sign and significance. Thus, the data suggest that distant democratic experience helps explain exit from autocracy, while duration of the current democratic spell makes democracies more stable.

On balance, these robustness checks suggest that the estimated coefficients on domestic democratic capital reported in Table 2 are probably biased upwards (in absolute value), as they are likely to reflect a mixture of state dependence and unobserved heterogeneity.

Another possible concern about our basic results is that they are driven by the waves of democracy that swept the world over time – perhaps the negative effects of foreign democratic capital are due to unobserved common shocks to democracy, rather than foreign spillovers. Addressing this issue by a fixed-year estimator runs into an analogous problem to the fixed-country estimator: fixed-year effects perfectly predict years without exits, in the same way as fixed-country effects perfectly predict countries without exits. This means that we have to drop all years without any transition into democracy and identify the hazard rate out of autocracy only from the years of such transitions (and thereby systematically underestimate the contribution of democratic capital to regime persistence).

To capture time-varying incidence of democracy without cutting sample size too much, we extend the basic specifications in columns 3 and 4 of Table 2 with 10-year and 5-year fixed effects, respectively. The estimated effects of domestic democratic capital and per-capita income hold up in both samples. The effects of foreign democratic capital are negative and statistically significant with 10-year indicators (columns 5 and 6). With 5-year indicators (columns 7 and 8), the effects remain negative, but less precisely estimated, and the point estimates are almost identical to those found in Table 2 (columns 3 and 4).

What happens if we include both country and year fixed effects at the same time? Column 9 reports conditional logit estimates in the sample of autocracies. Here, the estimated coefficients on both components of democratic capital contradict the predictions: domestic as well as foreign democratic capital increase the probability of autocracy, and the estimated coefficients are very large. The result appears to be due to the inclusion of the country and year fixed effects: if we re-estimate the same regression on this restricted sample without the year and country fixed effects, the basic results of Table 2 hold up. Thus, in this restricted sample of autocracies, the within-country and within-year variation of political regimes violate the theoretical predictions. Given the relative infrequency of regime changes, this fragility is perhaps not too surprising, although it highlights the role of the identifying assumptions of no unobserved heterogeneity and absence

of common shocks.

In the democracy sample, the algorithm for maximizing the likelihood function fails to converge, so we cannot report any estimates. But we can estimate the same specification without the fixed effects on the sample of democracies not excluded by the fixed effects. In this highly selected sample – a mere 827 observations, less than 25 percent of the original ones – foreign democratic capital and lagged income maintain their signs from Table 2. Domestic democratic capital has a coefficient close to zero ( $-0.137$ ) with a large standard error (0.614), however, providing indirect evidence of sample selection bias.

## VI. Economic growth

In this section, we use our panel to estimate structural and reduced forms corresponding to the economic part of our theoretical model. The first subsection deals with democratic and autocratic regimes separately, looking at the effect of regime expectations on growth. The second subsection considers the full sample and estimates the reduced form, asking whether democratic capital influences economic growth.

### A. Within regimes – structural form

Motivated by Prediction 2, we estimate the effect of the expected regime on growth. Specifically, we test (a): *The relative effect of expected autocracy in democracy vs. autocracy*. With sorting according to heterogeneous economic effects of democracy, a higher probability of autocracy has an (algebraically) smaller effect on growth in the sample of democracies than in the sample of autocracies. We also test (b): *The absolute effect of expected autocracy*. If democracy is more productive than autocracy for a large enough share of countries, a higher probability of autocracy reduces economic growth for the sample of current democracies (or even in the current autocracies).

ECONOMETRIC SPECIFICATION. — We estimate by OLS a linear version of (10), expressed in first differences. Since income is measured in logs, the dependent variable is yearly economic growth. To allow for conditional convergence, we always include lagged income,  $y_{i,t-1}$ . The probability that country  $i$  finds itself in autocracy in period  $t$  is obtained from the predicted hazard rates estimated in Section IV (columns 3 and 4 of Table 2), and is denoted by  $\hat{p}_{i,t}^a$ , where  $a$  refers to the regime in period  $t - 1$ .<sup>13</sup>

We start with the parsimonious formulation directly suggested by the model. But to remove the effect of omitted variables – such as  $\theta$  in the model – that vary only by country or year, in subsequent specifications we include country and year fixed effects,  $\alpha_i$  and  $\phi_t$  respectively, as well as a vector of additional regressors,  $\mathbf{x}_{i,t}$ , defined below (given the continuous dependent variables, here we can include country and year fixed effects without shrinking the sample size and

<sup>13</sup>OLS estimates of the coefficient on the lagged endogenous variable may be biased when also including country fixed effects, but given our long sample size this is not a concern (Stephen Nickell 1981): on average we have 34 or 37 periods per country, depending on the samples.

hence without running into sample selection bias). Thus, we estimate a version of the following equation regime by regime:

$$(15) \quad y_{i,t} - y_{i,t-1} = \gamma^a \widehat{p}_{i,t}^a + \beta y_{i,t-1} + \sigma \mathbf{x}_{i,t} + \omega z_{i,t} + \alpha_i + \phi_t + \varepsilon_{i,t}, \quad a_t = a_{t-1} = 0, 1.$$

When country and year fixed effects are included, the parameter of interest,  $\gamma^a$ , is only identified by the country-specific time variation in the estimated probability of autocracy. By the specification of the hazard rate, this variation is due to four sources: indicators for wars and lagged wars, time variation in the domestic and foreign components of democratic capital, and lagged income. To take care of the likely direct effect of wars on growth, the vector  $\mathbf{x}_{i,t}$  includes the two war indicators. Since years of democratic experience may directly influence economic growth, for instance through the accumulation of human capital or by inducing specific policy outcomes, we also include the domestic component of democratic capital  $z_{i,t}$  among the regressors.

We identify parameter  $\gamma$  by the restriction that the foreign component of democratic capital  $f_{i,t}$  can be excluded from the right-hand side of (15), and by the functional-form assumption implicit in the hazard rates. The exclusion restriction hinges on the incidence of democracy in neighboring countries being uncorrelated with domestic growth. This may pose a problem if (i) nearby democracies have higher incomes than nearby autocracies, and (ii) higher incomes abroad generate higher domestic growth (say through international trade). To remove this concern,  $\mathbf{x}_{i,t}$  includes a measure of foreign income for country  $i$  in year  $t$ , defined as:

$$(16) \quad y_{i,t}^f = \sum_{j \neq i} \varpi(\rho)_t^{i,j} y_{j,t},$$

where the bilateral weights  $\varpi(\rho)_t^{i,j}$  are identical to the weights used in the definition of foreign democratic capital (i.e., they decline geometrically in geographic distance).

Another possible concern is that during years of political transitions, the probability of a coup is high at the same time as growth suffers because of disruptions or political violence. It is not obvious why this should be a serious concern: by construction, our *estimated* hazard rates are just functions of the same variables as those in our growth regressions plus the two excluded components of democratic capital. Nevertheless, to ensure that our estimates are not driven by transition years, we also include in  $\mathbf{x}_{i,t}$  an indicator for transition years (the year of entry in autocracy or democracy, as well as the preceding year).

Finally, in the 1990s many socialist regimes in Central and Eastern Europe did not only undergo political transformations, but also a deep change of their economic systems which also affected their growth process. To avoid confounding these economic and political transitions, we include in  $\mathbf{x}_{i,t}$  a dummy variable equal to unity after 1989 in the former socialist countries of Central and Eastern Europe, and in the Asian provinces of the former Soviet Union (only in the regressions under autocracy, as the dummy variable is collinear with other regressors under democracy).

RESULTS. — Columns 1 and 2 in Table 4 display estimates from the parsimonious specification of (15), omitting any fixed effects as well as control vector  $\mathbf{x}_{i,t}$ , in the sample of democracies and autocracies, respectively. By contrast to the estimated hazard rates, there is now sometimes

a substantial difference between Huber-White robust standard errors (adjusted for possible heteroskedasticity) and standard errors clustered by country (allowing for arbitrary patterns of serial correlation). The robust standard errors may be an underestimate. But the clustered standard errors are likely to be an overestimate, due to measurement error in the dependent variable and low serial correlation of the residuals (on the order of 0.1). Therefore, we report robust (in the first brackets) as well as clustered (in the second brackets) standard errors underneath each coefficient. We do not adjust the standard errors for the fact that  $\hat{p}_{i,t}^a$  is a generated regressor, however.<sup>14</sup>

The coefficient of interest,  $\gamma^a$ , is negative under democracy but positive under autocracy, and both estimates are statistically significant. Thus, a higher probability of autocracy is harmful for growth under democracy, but increases growth under autocracy. The first finding is consistent with Prediction 2(b), under the maintained assumption that the fraction  $\lambda$  of countries benefiting from democracy is large enough. The finding that  $\hat{p}_{i,t}^a$  has a larger (algebraic) effect in autocracy than in democracy is also consistent with the theory, Prediction 2(a), and can be interpreted as due to endogenous sorting. Both findings reinforce the inference in Section V, based on the effects of income on the probability of regime transition. The theory has no unambiguous prediction on the sign of  $\gamma^a$  under autocracy. But a positive estimated coefficient suggests that productivity gains from autocracy, for the countries that are more frequently in autocracy, are about as high as the productivity gains from democracy, for the countries that are more frequently in that regime.<sup>15</sup>

The subsequent columns show how these basic results survive a more general specification, with fixed effects and the full set of control variables. Column 3 in Table 4 refers to democracies. The coefficient on the probability of autocracy retains the negative sign, as expected, and doubles in size; it is highly statistically significant with robust standard errors, but not with the less efficient clustered estimates. Also as expected, lagged income now has a negative and statistically significant coefficient, indicating income convergence of about 4 percent per year. The estimated coefficient on domestic democratic capital is positive but not statistically significant, and the results are unaffected if this variable is omitted. Finally, the estimated coefficients on transition years and foreign income (not shown) are respectively positive and negative, although only the latter is statistically significant.

The estimated coefficient on the probability of a transition out of democracy is about  $-12$ . This looks large: if the probability of a transition to autocracy were to jump from 0 to 1, the impact on growth would be a fall by 12 percentage points. As discussed in connection with Figure 5, however, the range of variation of the estimated hazard rates within regimes is small. Let us consider the experiments discussed in Section III. Suppose domestic democratic capital were to increase from its minimum of 0 to its maximum of 1, cutting the hazard rate out of democracy by about 2 percentage points. Given the point estimates in column 3, this would raise yearly growth by 0.25 percentage points, and – with a convergence rate of 0.04 – long-run income by over 6 percent. An increase in foreign democratic capital by 0.4 units, the difference in Europe between the 1970s and the present time (cf. Figure 2), would also cut the hazard rate out of democracy by

<sup>14</sup>Under the null hypothesis that  $\gamma^a = 0$ , the standard errors are still correctly estimated and thus, the t-statistics are still valid tests of the null (Adrian Pagan 1984).

<sup>15</sup>Przeworski et al. (2000) also find that the probability of remaining under autocracy increases growth in the countries currently under autocracy. Again, without an estimate for the covariance we cannot formally test equality of the coefficients of the probability of autocracy in columns 1 and 2. But once more, the 99 percent robust confidence intervals do not overlap, so that we would reject of equality unless the covariance was negative and large in absolute value.

just below 2 percentage points and have a similar effect on long-run income.<sup>16</sup>

As already discussed, identification hinges on the exclusion restriction that foreign democratic capital has no direct effect on growth. In column 4, we gauge the validity of this assumption by adding foreign democratic capital linearly, something we can do because democratic capital enters the predicted hazard rate in a highly non-linear fashion (i.e., the identification relies on functional form). While the coefficients on the hazard rate and the other variables are basically unaltered, the two components of democratic capital are neither individually nor jointly significant (cf. the  $F$  statistic in column 2). Of course this test relies on the functional form restriction, on which we cannot have much *a priori* confidence.

The functional form is also an issue when it comes to domestic democratic capital. The generated variable  $\hat{p}_{i,t}^a$  is a non-linear function of the regressors in the hazard rates. Suppose that, contrary to our assumptions, domestic democratic capital has direct non-linear effects on growth. Then, despite the inclusion of a linear term in domestic democratic capital, the estimated coefficient of  $\hat{p}_{i,t}^a$  might just reflect the direct growth effect of domestic democratic capital rather than a true causal effect of the probability of regime change on growth. To make sure that we identify parameter  $\gamma$  by the exclusion restriction rather than by an assumption on functional form, we re-estimate the variable  $\hat{p}_{i,t}^a$  from a first-stage hazard-rate regression that omits the domestic component of democratic capital. Thus, the generated regressor is not a function of domestic democratic capital at all. The results are reported in column 5, where we still control (linearly) for domestic democratic capital. The estimated coefficient on the probability of autocracy becomes even more negative and gains statistical significance, suggesting that this is not a concern.

These results are robust. They hold for a wide range of estimates for the value of  $\delta$  in our definition of democratic capital (for  $\delta$  between 0.94 and 0.99), and if we add non-parametric time trends by continents (interactions between year and continent indicators) to allow for omitted variables influencing the time profile of growth in different ways across groups of countries.

As a final check on the importance of functional form, column 6 shows results from an IV specification analogous to that in column 3, but estimated by 2SLS postulating a linear probability model for the hazard rate. Thus, we replace the regime-change probability generated from a non-linear hazard by a first stage, where the dependent variable is a binary indicator that equals one in country-years when democracy is abandoned, and zero otherwise. The second stage is the same growth regression as in column 3, where we exclude foreign democratic capital. (All other regressors, including domestic democratic capital, enter both the first and second stage regression). When we instrument the probability of autocracy with foreign democratic capital in this way, the coefficient on the probability of autocracy is surprisingly stable (and higher in absolute value). It is much less precisely estimated than earlier, which is not too surprising as the estimation procedure is less efficient. But the stability of the point estimate suggests that identification through the functional form plays a limited role.

Columns 7 through 10 show corresponding estimates for autocracies. Here, the fit is generally worse (the adjusted  $R$ -square drops considerably), but the basic result from column 2 is robust. The probability of autocracy has a larger (algebraic) effect on growth than in the sample of democracies, as predicted under endogenous sorting, and the effect is actually positive

<sup>16</sup>These computations hold the current political regime constant, and thus neglect the fact that changing democratic capital would have additional economic effects through actual (as opposed to only expected) regime changes.



and marginally significant.<sup>17</sup> These estimates are less precise and less robust than those under democracy, and the estimated coefficient on the probability of autocracy becomes statistically insignificant (though still positive) if we include interactions between year indicators and continent indicators.

Using the alternative definition of democracy by Boix and Rosato yields similar results.<sup>18</sup> The probability of autocracy has a negative effect on growth under democracy, and a positive effect under autocracy. The effect in democracies is again more precisely estimated (and statistically significant) than that within autocracies.

### B. Across regimes – reduced form

We now consider the full sample, pooling together observations under democracy and autocracy. We refrain from estimating the structural form across political regimes, because regime transitions are endogenous and we don't have reliable instruments for the current regime. Moreover, since transitions are rare, the estimated probability of autocracy is highly collinear with the actual political regime, which makes it difficult to disentangle the effects of actual vs. expected political transitions.

Instead we test Prediction 3 about the reduced form. (a) *The relative effect of democratic capital on growth in democracies and autocracies.* Sorting implies that democratic capital (domestic or foreign) has a larger effect on growth for countries that in the previous period were under democracy, compared to those that were under autocracy. (b) *The absolute effect of democratic capital on growth.* Higher democratic capital raises economic growth in democracy (or even under autocracy) if democracy stimulates development in a sufficiently large number of countries. Indeed, these effects should be stronger the higher is the share of countries where democracy brings about economic gains (parameter  $\lambda$  in the model).

ECONOMETRIC SPECIFICATION. — The econometric specification is similar to that of the previous subsection, except that we replace the estimated probability of autocracy in (15),  $\hat{p}_{i,t}^a$ , with both components of (domestic and foreign) of democratic capital. Moreover, we estimate over the full sample rather than separately across regimes. To check whether democratic capital has differential growth effects depending on the previous period political regime, we interact it with lagged democracy, possibly also including the lagged democracy indicator on its own,  $(1 - a_{t-1})$ , to avoid identifying differential effects of democratic capital from an exclusion restriction. The specification of the vector of control variables  $\mathbf{x}_{i,t}$  is otherwise the same as in the previous subsection.

We report specifications with and without dummy variables for transition years. On the one hand, such dummies are a virtue: transition years are typically associated with unusual volatility and uncertainty, so including them decreases the risk of confounding effects. On the other hand, they are a vice: we identify the parameters of interest from country-specific time variation which

<sup>17</sup>Once more, the 99 percent robust confidence intervals of the estimated effects of the probability of autocracy on growth, under democracy vs autocracy, do not overlap.

<sup>18</sup>Here, we use the hazard rates estimated in columns 5 and 6 of Table 2.

is maximized around the transition, so including transition years removes useful variation in democratic capital around regime changes.

**RESULTS.** — Results are reported in Table 5. We start out with a specification including country and year fixed effects and the relevant covariates. Again, we report both robust and clustered standard errors. Columns 1 and 2 display the simplest reduced form (with and without transition years), constraining the coefficients of democratic capital to be the same under lagged democracy and lagged autocracy. Domestic democratic capital has a positive estimated coefficient, which is more strongly significant when standard errors are not clustered by country. This is consistent with Prediction 3(b), if there are enough countries for which democracy is productive. Foreign democratic capital, on the other hand, has a negative estimated coefficient not significantly different from zero.

Columns 3 and 4 allow the coefficients of democratic capital to differ by lagged political regime, with and without controls for transition years. Consistent with the earlier results and the presence of sorting, Prediction 3(a), positive effect of democratic capital is only present among democracies in the past year. The domestic component of democratic capital has a larger or more precisely estimated coefficient than the foreign one.

Finally, columns 5 and 6 add the lagged democracy indicator on its own. This is more demanding on the data, because of collinearity (the correlation between lagged democracy and domestic democratic capital is close to 0.8). Nevertheless, the results in previous columns hold up pretty well, particularly when the transition-year dummy is excluded. Again, these findings support the hypothesis that becoming a democracy on average leads to an improvement in growth, and that countries sort themselves into political regimes by the relative productivity of democracy.

The point estimates imply powerful effects of domestic democratic capital on growth and long-run income in democracy. Suppose we reconsider the experiments discussed in Sections IV and V.A above. Given the estimated convergence rates and coefficients of domestic democratic capital in columns 3 to 5, a switch from 0 to 1, from minimum to maximum domestic democratic capital, would raise long-run income in a democracy by more than 75 percent<sup>19</sup> Note that here, unlike in Section IV, unobserved heterogeneity is not an issue, since we include fixed country effects.

These results are robust to measuring democratic capital with higher values of the depreciation rate ( $1 - \delta$ ), or to the inclusion of non-parametric continental growth trends. Overall, the findings confirm that the positive effect of democratic capital within democracies is reasonably robust and quantitatively relevant. It is a third piece of evidence that, on average, democracy is better for growth than autocracy, and that democratic stability is particularly important.

## VII. Conclusions

Our results suggest the possibility of a virtuous circle, of the type mentioned in the introduction. Having long-time democratic experience favors economic development through physical capital accumulation, which helps further consolidate democracy. This, in turn, leads to the accu-

<sup>19</sup>These computations neglect the additional long run effects of democratic capital on actual regime changes, operating through the lagged democracy variable.

mulation of more democratic capital, with additional positive effects on income and democratic stability.

These results may help explain the triangular pattern in Figure 1, where experience with democracy appears to be a sufficient but not a necessary condition for economic development. Observations in the right part of the graph, with high incomes and long democratic experience, may thus reflect positive feedback effects between physical and democratic capital accumulation in a virtuous circle. Getting into such a virtuous circle is difficult, however, because democratic stability is hard to achieve instantaneously.

Our results also point to three asymmetries across political regimes. First, higher income makes democracies more stable, but does not make dictators more precarious. Second, while the probability of switching from democracy to autocracy hurts growth, the probability of remaining under autocracy has no effect on growth, or – if anything – a positive effect. Third, the positive influence of democratic capital on growth is due to democracies, not to autocracies. These asymmetries are consistent with a heterogeneous economic effect of democracy, and endogenous sorting of countries into political regimes on the basis of economic expediency.

Heterogeneous effects of democracy helps account for another aspect of Figure 1, namely that long democratic experience, although sufficient, is not a necessary condition for economic development. Heterogeneity is consistent with highly successful autocracies, like Singapore, that have never experienced democratic forms of government. The leftmost part of the graph, where countries with little or no democratic experience are found at all income levels, squares well with the empirical result that high income does not promote transition from autocracy to democracy.

Our inference is conditional on three identifying assumptions. One caveat concerns the effects of the domestic component of democratic capital on regime transitions. This component measures the time spent under democracy in the (possibly distant) past. In attributing a full causal effect to domestic democratic capital, we must assume that no unobserved variables make some democracies more stable than others. Given the role of unobserved heterogeneity and sorting in our model, we are likely to over-estimate the true causal effect of domestic democratic capital in the consolidation of democracy. In our estimated hazard rates, this variable is likely to capture a mix of unobserved heterogeneity and state dependence. Time-invariant unobserved heterogeneity is less of an issue in the growth regressions, however, where we identify the effects from country-specific time variation in the data.

The second issue concerns the effect of foreign democratic capital, and our inference that being surrounded by democratic countries helps stabilize democracy at home. Since foreign democratic capital covaries over time for many countries, it can be confounded with unobserved common shocks. To identify a causal effect, we have taken a stance about the frequency of such common shocks. The results hold up if unobserved common shocks follow a non-linear time trend, or occur at the frequency of decades or, to a lesser extent, of five years. Our identification rules out common shocks at the yearly frequency, however.

The third important identifying assumption is that the foreign component of democratic capital does not directly affect economic growth, conditional on covariates in the regression. We think this is credible, since the growth estimates only exploit within-country variation and foreign income is one of the included regressors.

Our findings suggest avenues for future research. To understand better the sources of hetero-

geneity in the economic gains from democracy, we can exploit a large literature in comparative politics on a variety of democratic institutions, such as the electoral rule (majoritarian vs. proportional), the form of government (presidential vs. parliamentary), and the extent of vertical delegation (federal vs. unitary). These institutions may entail different degrees of political participation. If democratic capital accumulates through active participation, its accumulation and depreciation rates may systematically differ across forms of democracy. Our empirical findings suggest that understanding the differences between various types of autocracies may be even more important.<sup>20</sup> Persson and Tabellini (2008) focus explicitly on heterogeneous effects of regime transitions, combining a difference-in-difference approach with propensity-score matching.

We would also like to sharpen the notion of democratic capital. Which democratic values and norms are essential and how do they relate to cultural and social attitudes? How important is education in the accumulation of these values and norms? Does democratic consolidation require a rising middle class with democratic values? Are independent media crucial in mobilizing support for democracy? Through which mechanisms do foreign political regimes impact on the evolution of domestic institutions? Telling these forces apart and pinpointing their roles in democratic capital accumulation is an important topic for further research.

## REFERENCES

- Acemoglu, Daron, Simon Johnson, James Robinson, and Pierre Yared.** 2005. "From Education to Democracy." *American Economic Review Papers and Proceedings*, 95: 44–49.
- Acemoglu, Daron, Simon Johnson, James Robinson, and Pierre Yared.** 2008. "Income and Democracy." *American Economic Review*, 98: 808–842.
- Daron Acemoglu, and James Robinson.** 2005. *Economic Origins of Dictatorship and Democracy*. Cambridge: Cambridge University Press.
- Almond, Gabriel, and Sidney Verba.** 1963. *The Civic Culture: Political Attitudes and Democracy in Five Nations*. Princeton: Princeton University Press.
- Barro, Robert.** 1996. "Democracy and Growth." *Journal of Economic Growth*, 1: 1–27.
- Barro, Robert.** 1997. *Determinants of Economic Growth: A Cross-Country Empirical Study*. Cambridge: MIT Press.
- Barro, Robert.** 1999. "The Determinants of Democracy." *Journal of Political Economy*, 107: S158–S183.
- Barro, Robert, and Jong-Wha Lee.** 2000. "International Data on Educational Attainment: Updates and Implications." CID Working Paper No 42.
- Becker, Gary.** 1973. "A Theory of the Marriage Market: Part 1." *Journal of Political Economy*, 81: 813–866.
- Bermeo, Nancy.** 2003. *Ordinary Citizens in Extraordinary Times: The Citizenry and the Breakdown of Democracy*. Princeton: Princeton University Press.
- Boix, Carles.** 2003. *Democracy and Redistribution*. Cambridge: Cambridge University Press.

<sup>20</sup>See here the analysis by Daron Acemoglu and James Robinson (2005) and the references they cite.

- Boix, Carles, and Susan Stokes.** 2002. "Endogenous Democratization: A Critique." Unpublished. University of Chicago.
- Boix, Carles, and Sebastian Rosato.** 2001. "A Complete Data Set of Political Regimes, 1800-1999." Unpublished, University of Chicago.
- Bueno de Mesquita, Bruce, Alastair Smith, Randolph Siverson, and James Morrow.** 2003. *The Logic of Political Survival*. Cambridge MA: MIT Press.
- Collier, Ruth Berins.** 1999. *Paths Toward Democracy: The Working Class and Elites in Western Europe and South America*. Cambridge: Cambridge University Press.
- Jan Eeckhout, and Boyan Jovanovic.** 2007. "Occupational Choice and Development." Unpublished.
- Gabaix, Xavier, and Augustin Landier.** 2008. "Why Has CEO Pay Increased so Much?" *Quarterly Journal of Economics*, 123: 49–100.
- Geddes, Barbara.** 1999. "What Do We Know about Democratization after Twenty Years?" *Annual Review of Political Science*, 2: 115–144.
- Gerring, John, Philip Bond, William T. Barndt, and Carola Moreno.** 2005. "Democracy and Economic Growth: A Historical Perspective." *World Politics*, 57: 323–364.
- Giaavazzi, Francesco, and Guido Tabellini.** 2005. "Economic and Political Liberalizations." *Journal of Monetary Economics*, 52: 1297–1330.
- Gibson, James.** 1997. "Mass Opposition to the Soviet Putsch of August 1991: Collective Action, Rational Choice, and Democratic Values in the Former Soviet Union." *American Political Science Review*, 91: 671–684.
- Glaeser, Edward, Giacomo Ponzetto, and Andrei Shleifer.** 2007. "Why Does Democracy Need Education?" *Journal of Economic Growth*, 12: 77–99.
- Hadenius, Axel, and Jan Teorell.** 2005. "Cultural and Economic Prerequisites of Democracy: Reassessing Recent Evidence." *Studies in Comparative International Development*, 39: 87–206.
- Hall, Robert, and Charles Jones.** 1999. "Why Do Some Countries Produce So Much More Output per Worker than Others?" *Quarterly Journal of Economics*, 114: 83–116.
- Helliwell, John.** 1994. "Empirical Linkages Between Democracy and Economic Growth." *British Journal of Political Science*, 24: 225–248.
- Huntington, Samuel.** 1991. *The Third Wave: Democratization in the Late Twentieth Century*. London: University of Oklahoma Press.
- Inglehart, Ronald, and Christian Welzel.** 2005. *Modernization, Cultural Change, and Democracy: The Human Development Sequence*. New York: Cambridge University Press.
- Jenkins, Stephen.** 2004. *Survival Analysis*. Unpublished, University of Essex.
- Jones, Ben, and Benjamin Olken.** 2005. "Do Leaders Matter? National Leadership and Growth since World War II." *Quarterly Journal of Economics*, 120: 835–864.
- Lipset, Seymour Martin.** 1959. "Some Social Requisites of Democracy: Economic Development and Political Legitimacy." *American Political Science Review*, 53: 69–105.
- Londregan, John, and Keith Poole.** 1990. "Poverty, the Coup Trap, and the Seizure of Executive Power." *World Politics*, 42: 151–183.

- Maddison, Angus.** 2001. *The World Economy: Historical Statistics*. Paris: OECD.
- Mulligan, Casey B., Xavier Sala-i-Martin, and Ricard Gil.** 2004. "Do Democracies Have Different Public Policies than Nondemocracies?" *Journal of Economic Perspectives*, 18: 51–74.
- Nickell, Stephen.** 1981. "Biases in Dynamic Models with Fixed Effects." *Econometrica*, 6: 1417–1426.
- Opp, Karl-Dieter.** 1999. "Contending Conceptions of the Theory of Rational Action." *Journal of Theoretical Politics*, 11: 171–202.
- Pagan, Adrian.** 1984. "Econometric Issues in the Analysis of Regressions with Generated Regressors." *International Economic Review*, 25: 221–247.
- Papaioannou, Elias, and Gregorios Siourounis.** 2007. Forthcoming. "Democratization and Growth." *Economic Journal*.
- Persson, Torsten.** 2005. "Forms of Democracy, Policy and Economic Development." NBER Working Paper No. 11171.
- Persson, Torsten, and Guido Tabellini.** 2006a. "Democracy and Development: The Devil in the Details." *American Economic Review Papers and Proceedings*, 96: 319–324.
- Persson, Torsten, and Guido Tabellini.** 2006b. "Democratic Capital: The Nexus of Economic and Political Change." NBER Working Paper No. 12175.
- Persson, Torsten, and Guido Tabellini.** 2008. "The Growth Effect of Democracy: Is it Heterogeneous and How Can it Be Estimated?" In *Institutions and Economic Performance*, ed. Elhanan Helpman, 544–585. Cambridge MA: Harvard University Press.
- Przeworski, Adam, Michael Alvarez, Jose Cheibub, and Fernando Limongi.** 2000. *Democracy and Development: Political Institutions and Well-Being in the World 1950-1900*. Cambridge: Cambridge University Press.
- Przeworski, Adam, and Fernando Limongi.** 1993. "Political Regimes and Economic Growth." *Journal of Economic Perspectives*, 7: 51–69.
- Putnam, Robert.** 1993. *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton: Princeton University Press.
- Rodrik, Dani, and Romain Wacziarg.** 2005. "Do Democratic Transitions Produce Bad Economic Outcomes?" *American Economic Review Papers and Proceedings*, 95: 50–56.
- Roll, Richard, and John Talbott.** 2003. "Political and Economic Freedoms and Prosperity." Unpublished, University of California, Los Angeles.
- Shimer, Robert, and Lones Smith.** 2000. "Assortative Matching and Search." *Econometrica*, 68: 343–369.
- Tabellini, Guido.** 2008a. "Institutions and Culture." 2007 Presidential Address, *Journal of the European Economic Association*, 6: 255–294.
- Tabellini, Guido.** 2008b. "The Scope of Cooperation: Values and Incentives." *Quarterly Journal of Economics*, 123: 905–950.



Figure 2 Domestic democratic capital in two countries

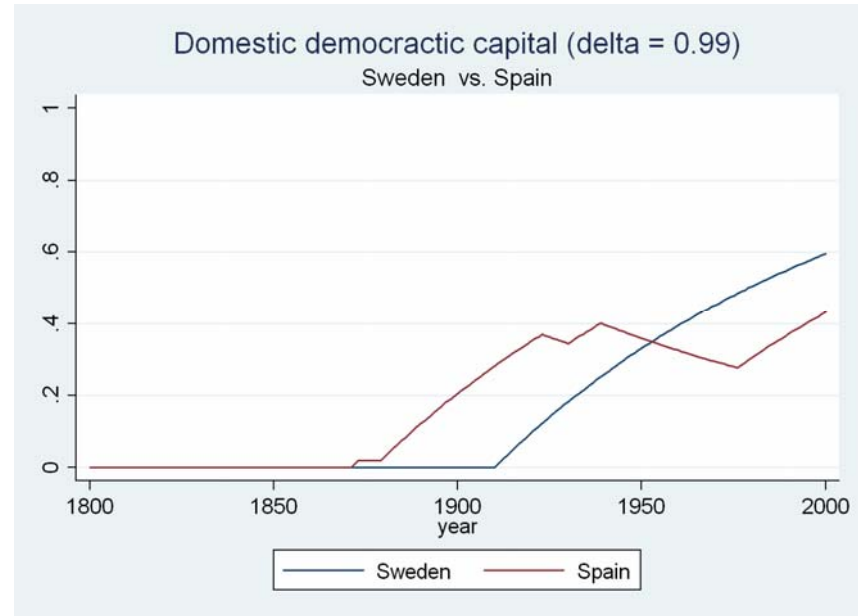
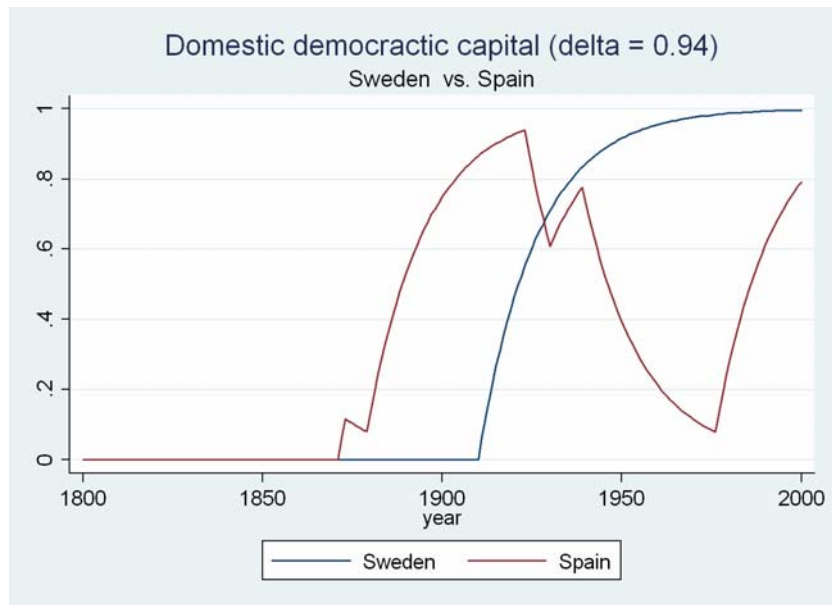




Figure 3 Foreign democratic capital in two countries

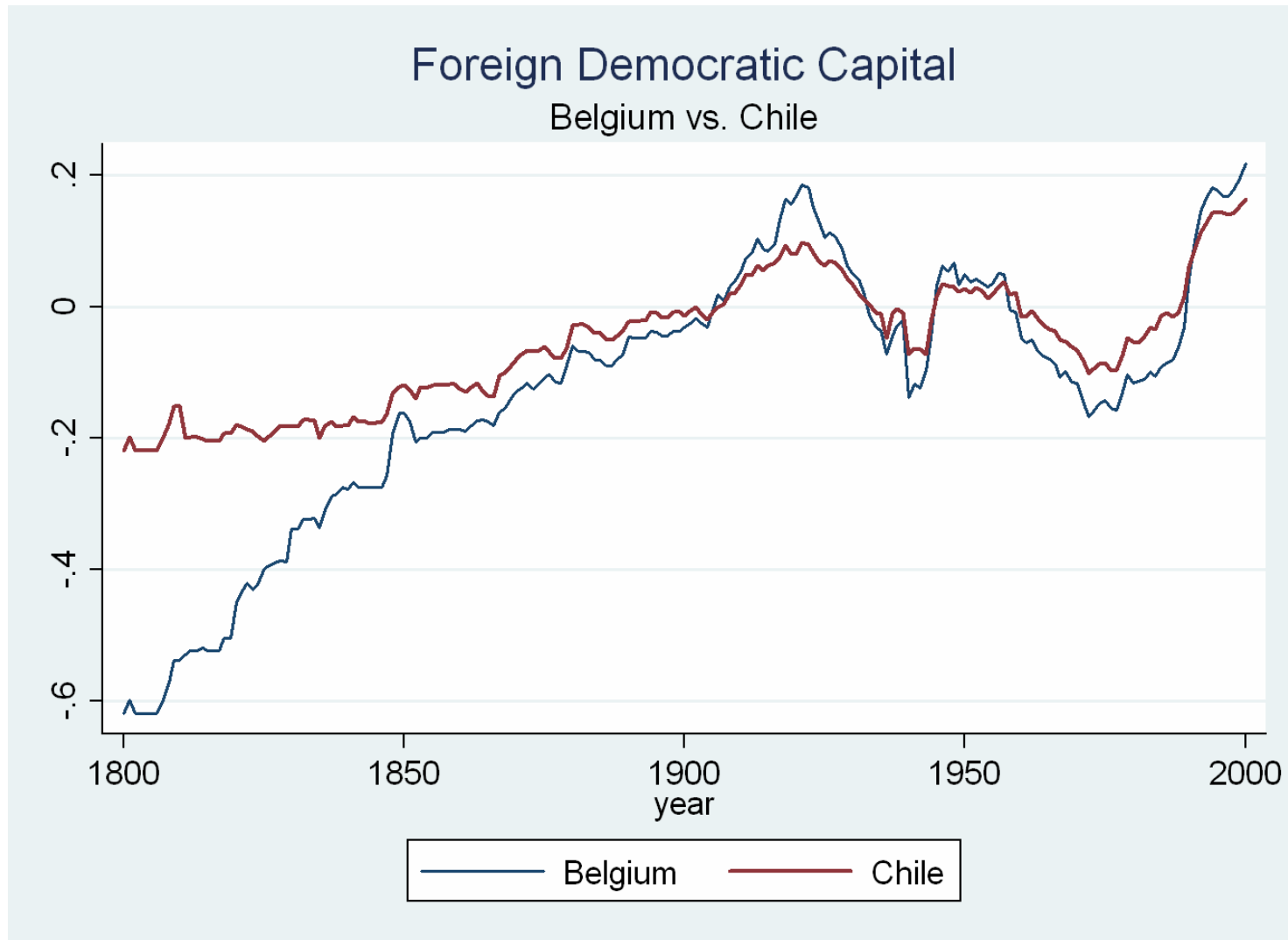
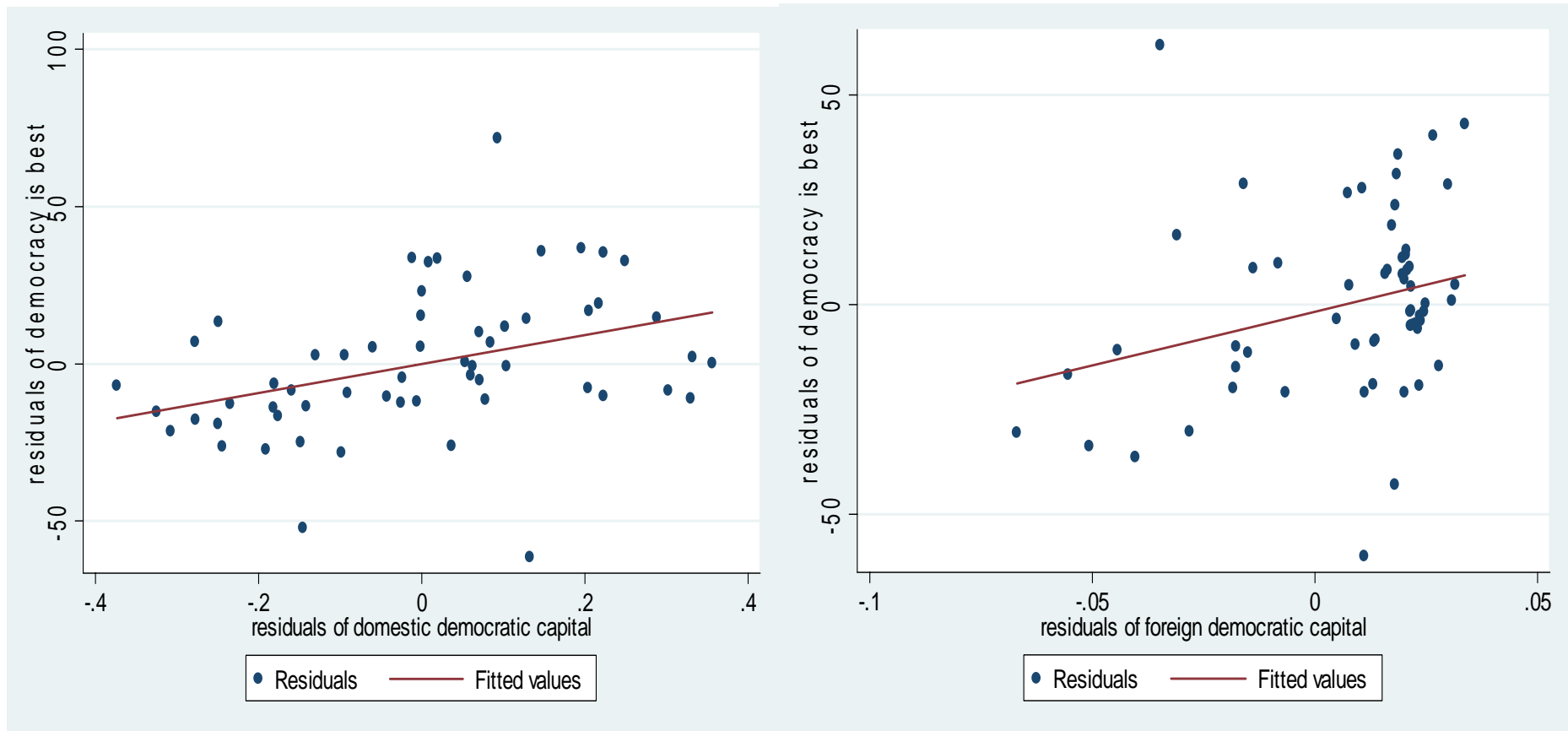
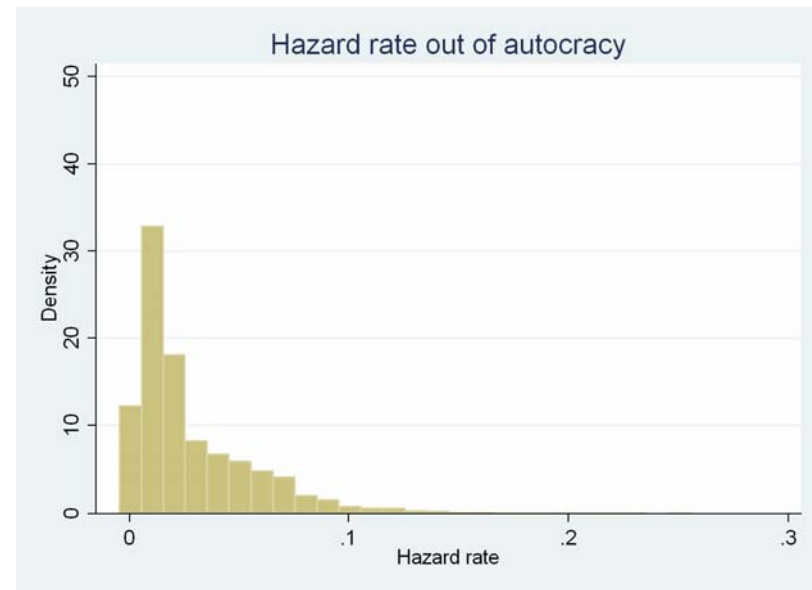
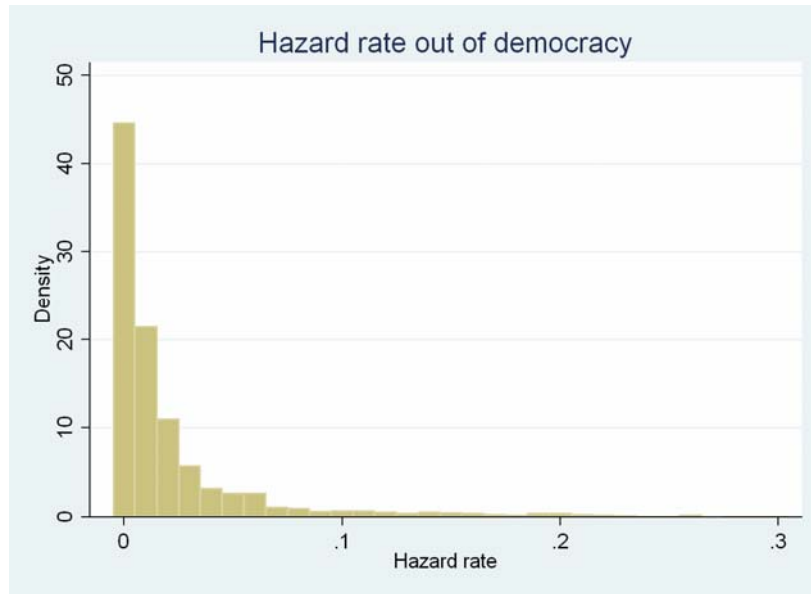


Figure 4 Democratic capital and opinions on democracy



**Figure 5 Predicted hazard rates out of political regimes**



**Table 1 Democratic capital and perceptions of democracy and protection of property rights**

|                             | (1)<br>Thinks<br>democracy is<br>best system | (2)<br>Thinks<br>democracy is<br>best system | (3)<br>Thinks<br>democracy is<br>best system | (4)<br>Thinks<br>democracy is<br>best system | (5)<br>Thinks<br>democracy is<br>best system | (6)<br>Perception of<br>government<br>anti diversion<br>policies | (7)<br>Perception of<br>government<br>anti diversion<br>policies |
|-----------------------------|--|--|--|--|--|--|--|
| Domestic democratic capital | 29.14***<br>(10.93)                          | 42.93**<br>(16.10)                           | 43.52***<br>(11.58)                          | 46.22***<br>(15.51)                          | 46.08***<br>(13.98)                          | 4.35<br>(5.44)   | 2.79<br>(5.22)   |
| Foreign democratic capital  | 263.57**<br>(114.77)                         | 345.63**<br>(136.94)                         | 288.26<br>(110.58)**                         | 321.40**<br>(131.83)                         | 396.89***<br>(128.84)                        | -61.76*<br>(32.93)   | -49.29<br>(32.18)  |
| Per capita income           |  | -6.23<br>(4.92)                              |  | -2.29<br>(5.01)                              | -1.14<br>(5.82)                              | 11.82***<br>(1.05)   | 9.36***<br>(1.59)  |
| Democracy                   |  |  | -20.92***<br>(7.77)                          | -19.90**<br>(8.34)                           | -3.50<br>(9.42)                              | -0.07<br>(2.30)  | -0.12<br>(2.50)  |
| Human capital               |  |  |  |  | -19.87**<br>(7.93)                           |  | 7.22**<br>(2.96)   |
| Number of observations      | 62   | 59   | 61   | 59   | 46   | 113  | 90   |
| Adj. R-squared              | 0.17   | 0.17   | 0.26   | 0.23   | 0.33   | 0.69   | 0.74   |

**Notes:** Variables explained in text. All specifications estimated by Ordinary Least Squares. Standard errors in brackets: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. All variables measured in 1999, except perception of government anti diversion policies, which is measured in 1997.

**Table 2 Probability of autocracy**

|                             | (1)<br>From<br>democracy | (2)<br>From<br>autocracy | (3)<br>From<br>democracy | (4)<br>From<br>autocracy | (5)<br>From<br>democracy | (6)<br>From<br>autocracy | (7)<br>From<br>democracy | (8)<br>From<br>autocracy |
|-----------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| Domestic democratic capital | -0.49***<br>(0.19)       | -0.83***<br>(0.18)       | -0.86**<br>(0.37)        | -1.06***<br>(0.39)       | -0.83***<br>(0.27)       | -0.44*<br>(0.27)         | - 0.55*<br>(0.30)        | -1.09***<br>(0.27)       |
| Foreign democratic capital  | -1.10**<br>(0.47)        | -1.92***<br>(0.37)       | -2.36***<br>(0.70)       | -1.84***<br>(0.38)       | -3.29***<br>(0.83)       | -2.22***<br>(0.43)       | - 1.81**<br>(0.75)       | -2.14***<br>(0.49)       |
| Lagged per capita income    | -0.50***<br>(0.07)       | -0.054<br>(0.052)        | -0.41***<br>(0.07)       | 0.004<br>(0.06)          | -0.36***<br>(0.08)       | -0.01<br>(0.07)          | - 0.34***<br>(0.11)      | 0.087<br>(0.086)         |
| Human Capital               |                          |                          |                          |                          |                          |                          | - 0.49*<br>(0.26)        | -0.34*<br>(0.19)         |
| $\delta, \rho$              | 0.94, 1                  | 0.94, 1                  | 0.99, 1                  | 0.99, 1                  | 0.97, 1                  | 0.97, 1                  | 0.94, 1                  | 0.94, 1                  |
| Covariates                  | No                       | No                       | Yes                      | Yes                      | Yes                      | Yes                      | No                       | No                       |
| Democracy definition        | Polity4                  | Polity4                  | Polity4                  | Polity4                  | Boix-Rosato              | Boix-Rosato              | Polity4                  | Polity4                  |
| Method                      | ML Probit                | ML Probit                | ML Probit                | ML Probit                | ML Probit                | ML Probit                | ML Probit                | ML Probit                |
| LR-test ( $p$ -value)       | 0.00***                  | 0.11                     | 0.37                     | 0.14                     | 0.29                     | 0.47                     | 0.24                     | 0.06*                    |
| Number of obs.              | 3848                     | 4420                     | 3786                     | 4349                     | 3969                     | 4115                     | 1947                     | 1924                     |
| Pseudo R-square             | 0.14                     | 0.04                     | 0.23                     | 0.10                     | 0.23                     | 0.12                     | 0.22                     | 0.06                     |

**Notes:** Variables explained in text. Robust standard errors in brackets: \* denotes significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Covariates are indicators for wartime (current year and lagged year), socialist legal origin, British colonial origin, Spanish colonial origin, African location, Middle-Eastern location; in cols 3-4, scores of democracy and constraints on the executive (both from Polity IV) in first year of independence, in cols 5-6, indicator for democracy (by Boix and Rosato) in first year of independence. LR-test: random-effects panel specification (estimated by panel logit) against null of pooled specification (also estimated by logit) – a high  $p$ -value means we cannot reject that unobserved heterogeneity is absent.

**Table 3 Probability of autocracy – auxiliary results**

| Panel A                             |                       |                       |                       |                       |
|-------------------------------------|-----------------------|-----------------------|-----------------------|-----------------------|
|                                     | (1)<br>From democracy | (2)<br>From autocracy | (3)<br>From democracy | (4)<br>From autocracy |
| Domestic democratic capital         | 38.28***<br>(6.25)    | 21.21***<br>(3.73)    |                       | -1.10**<br>(0.47)     |
| Foreign democratic capital          | -8.40***<br>(1.98)    | -9.66***<br>(1.56)    | - 2.32***<br>(0.70)   | -2.07***<br>(0.46)    |
| Lagged per capita income            | -2.55***<br>(0.77)    | -1.04*<br>(0.54)      | - 0.41***<br>(0.07)   | -0.00<br>(0.07)       |
| Current domestic democratic capital |                       |                       | -0.983**<br>(0.400)   |                       |
| Past domestic democratic capital    |                       |                       | -0.539<br>(0.573)     |                       |
| Duration of current spell           |                       |                       |                       | 0.000<br>(0.001)      |
| Fixed effects                       | Country               | Country               | No                    | No                    |
| LR-test ( <i>p</i> -value)          |                       |                       | 0.36                  | 0.00***               |
| N. observations                     | 1569                  | 2966                  | 3786                  | 4329                  |
| Pseudo R-square                     | 0.23                  | 0.22                  | 0.23                  | 0.12                  |

| Panel B                     |                    |                    |                    |                    |                    |
|-----------------------------|--------------------|--------------------|--------------------|--------------------|--------------------|
|                             | (5)                | (6)                | (7)                | (8)                | (9)                |
|                             | From democracy     | From autocracy     | From democracy     | From autocracy     | From autocracy     |
| Domestic democratic capital | -1.99*<br>(1.04)   | -1.72**<br>(0.79)  | -1.91**<br>(0.97)  | -2.36***<br>(0.80) | 17.99***<br>(4.03) |
| Foreign democratic capital  | -4.03**<br>(1.65)  | -4.83***<br>(1.18) | -2.32<br>(2.20)    | -2.32<br>(1.88)    | 39.39***<br>(9.21) |
| Lagged per capita income    | -0.87***<br>(0.20) | -0.08<br>(0.15)    | -0.91***<br>(0.18) | -0.06<br>(0.14)    | -0.47<br>(0.62)    |
| Fixed effects               | 10 year            | 10 year            | 5 year             | 5 year             | Country<br>& year  |
| LR-test ( <i>p</i> -value)  | 0.14               | 0.08*              | 0.20               | 0.14               |                    |
| N. observations             | 3382               | 4181               | 3316               | 3992               | 2966               |
| Pseudo R-square             | 0.23               | 0.09               | 0.24               | 0.11               |                    |

**Notes:** Variables explained in text. Robust standard errors in brackets: \* denotes significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Covariates: cols 1,2: wars (current and lagged), linear and a quadratic time trend; cols 3-8: wars (current and lagged), socialist legal origin, British colonial origin, Spanish colonial origin, African location, Middle-Eastern location, a, an indicator for countries that switched regime more than 5 times, polity2 scores of democracy and constraints on the executive in first year of independence.; col. 9: wars (current and lagged) . LR-test: random-effects panel specification (estimated by logit) against the null of pooled specification (also estimated by logit) – a high *p*-value means we cannot reject that unobserved heterogeneity is absent. Parameter  $\rho$  equals 1 in all columns, and parameter  $\delta$  equals 0.99 in all columns.

**Table 4 Growth rates within political regimes – structural estimates**

| Panel A                     |   |  |  |  |  |
|-----------------------------|---|--|--|--|--|
| Sample                      | Democracy<br>(1)  | Autocracy.<br>(2)                                    | Democracy<br>(3)   | Democracy.<br>(4)  | Democracy<br>(5)   |
| Probability of autocracy    | - 6.76<br>(2.36) <sup>***</sup><br>(2.60) <sup>**</sup> | 12.56<br>(4.98) <sup>**</sup><br>(6.55) <sup>*</sup> | - 12.23<br>(4.55) <sup>***</sup><br>(7.98)               | - 12.31<br>(4.55) <sup>***</sup><br>(8.02)               | - 14.72<br>(5.18) <sup>***</sup><br>(8.63) <sup>*</sup>  |
| Lagged income per capita    | - 0.02<br>(0.11)<br>(0.12)                              | - 0.12<br>(0.17)<br>(0.29)                           | - 4.35<br>(0.62) <sup>***</sup><br>(0.97) <sup>***</sup> | - 4.41<br>(0.62) <sup>***</sup><br>(0.99) <sup>***</sup> | - 4.47<br>(0.63) <sup>***</sup><br>(0.96) <sup>***</sup> |
| Domestic democratic capital |   |  | 1.70<br>(1.62)<br>(2.64)                                 | 1.76<br>(1.62)<br>(2.61)                                 | 2.03<br>(1.61)<br>(2.63)                                 |
| Foreign democratic capital  |   |  |  | - 4.46<br>(3.83)<br>(3.92)                               |  |
| Transition years            |   |  | - 0.82<br>(0.48) <sup>*</sup><br>(0.50)                  | - 0.80<br>(0.48) <sup>*</sup><br>(0.50)                  | - 0.81<br>(0.48) <sup>*</sup><br>(0.50)                  |
| Fixed effects               |   |  | Country & year   | Country & year   | Country & year   |
| Covariates                  | No  | No   | Yes  | Yes  | Yes  |
| F-statistic                 |   |  |  | 1.15   |  |
| N. observations             | 3774  | 4296   | 3774   | 3774   | 3774   |
| Adj. R-squared              | 0.003   | 0.003  | 0.20   | 0.20   | 0.21   |



Panel B

| Sample                      | Democracy<br>(6)                 | Autocracy<br>(7)                 | Autocracy.<br>(8)                | Autocracy<br>(9)                 | Autocracy.<br>(10)               |
|-----------------------------|----------------------------------|----------------------------------|----------------------------------|----------------------------------|----------------------------------|
| Probability of autocracy    | - 16.15<br>(15.54)<br>(14.45)    | 26.00<br>(15.46)*<br>(18.89)     | 26.94<br>(15.77)*<br>(19.12)     | 23.09<br>(13.66)*<br>(17.16)     | 5.76<br>(10.35)<br>(17.05)       |
| Lagged income per capita    | - 4.22<br>(0.62)***<br>(1.06)*** | - 2.79<br>(0.55)***<br>(0.70)*** | - 2.70<br>(0.57)***<br>(0.76)*** | - 2.74<br>(0.56)***<br>(0.70)*** | - 2.88<br>(0.56)***<br>(0.73)*** |
| Domestic democratic capital | 5.96<br>(4.52)<br>(5.01)         | 0.16<br>(3.65)<br>(4.55)         | 0.16<br>(3.65)<br>(4.57)         | -2.54<br>(3.78)<br>(4.09)        | -4.13<br>(4.82)<br>(5.53)        |
| Foreign democratic capital  |                                  |                                  | 7.92<br>(8.45)<br>(13.31)        |                                  |                                  |
| Transition years            | 3.87<br>(4.68)<br>(4.34)         | - 1.63<br>(0.52)***<br>(0.56)*** | - 1.59<br>(0.52)***<br>(0.56)*** | - 1.63<br>(0.53)***<br>(0.57)*** | 0.44<br>(4.10)<br>(6.77)         |
| Fixed effects               | Country & year                   | Country & year                   | Country & year                   | Country & year                   | Country & year                   |
| Covariates                  | Yes                              | Yes                              | Yes                              | Yes                              | Yes                              |
| F-statistic                 |                                  |                                  | 0.44                             |                                  |                                  |
| N. observations             | 3774                             | 4296                             | 4296                             | 4296                             | 4296                             |
| Adj. R-squared              |                                  | 0.12                             | 0.12                             | 0.12                             |                                  |

**Notes:** Variables explained in text. Robust standard errors in first brackets, clustered by country in second brackets: \* denotes significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Democracy defined according to Polity IV. Country and year fixed effects included as indicated. Non-displayed covariates are indicators for war years, lagged war years and a measure of foreign income. Columns 7-10 also include a dummy variable for years after 1989 in former socialist countries in Central and Eastern Europe and in the Asian provinces in the former Soviet Union. F-statistic is the test statistic for the joint significance of the latter two variables. Probability of autocracy for past democracies and autocracies computed from estimates in columns 3 and 4 of Table 2, respectively, except in cols 5, 6, 9, and 10. Columns 5 and 9 use a probability of autocracy generated by the same hazard specification, except that Domestic Democratic Capital is excluded. Columns 6 and 10 show 2SLS estimates, where Foreign Democratic Capital is the instrument for actual regime change in a first-stage linear probability model, which replaces the probability generated by a (non-linear) hazard.

**Table 5 Growth rates across political regimes – reduced-form estimates**

|   | (1)<br>Growth                    | (2)<br>Growth                    | (3)<br>Growth                    | (4)<br>Growth                    | (5)<br>Growth                    | (6)<br>Growth                    |
|---|----------------------------------|----------------------------------|----------------------------------|----------------------------------|----------------------------------|----------------------------------|
| Domestic democratic capital                       | 3.34<br>(1.08)***<br>(1.82)*     | 2.95<br>(1.07)***<br>(1.83)      | - 0.24<br>(1.63)<br>(2.17)       | -0.21<br>(1.63)<br>(2.12)        | - 0.64<br>(1.82)<br>(2.59)       | - 0.22<br>(1.81)<br>(2.56)       |
| Foreign democratic capital                        | - 2.93<br>(3.36)<br>(4.65)       | - 2.79<br>(3.36)<br>(4.63)       | - 2.22<br>(3.55)<br>(4.85)       | - 1.92<br>(3.54)<br>(4.85)       | - 2.58<br>(3.60)<br>(4.78)       | - 1.93<br>(3.60)<br>(4.79)       |
| Domestic democratic capital in (lagged) democracy |                                  |                                  | 2.68<br>(1.24)**<br>(1.40)*      | 2.40<br>(1.23)*<br>(1.31)*       | 3.16<br>(1.51)**<br>(1.99)       | 2.40<br>(1.50)<br>(1.91)         |
| Foreign democratic capital in (lagged) democracy  |                                  |                                  | 2.61<br>(1.39)*<br>(1.97)        | 1.84<br>(1.40)<br>(1.99)         | 2.53<br>(1.43)*<br>(2.03)        | 1.84<br>(1.43)<br>(2.04)         |
| Lagged democracy                                  |                                  |                                  |                                  |                                  | -0.16<br>(0.29)<br>(0.44)        | -0.00<br>(0.30)<br>(0.44)        |
| Lagged income per capita                          | - 2.77<br>(0.36)***<br>(0.51)*** | - 2.81<br>(0.36)***<br>(0.52)*** | - 2.89<br>(0.37)***<br>(0.53)*** | - 2.90<br>(0.37)***<br>(0.53)*** | - 2.89<br>(0.37)***<br>(0.53)*** | - 2.90<br>(0.37)***<br>(0.53)*** |
| Transition years                                  |                                  | -1.64<br>(0.37)***<br>(0.42)***  |                                  | - 1.47<br>(0.35)***<br>(0.41)*** |                                  | -1.47<br>(0.35)***<br>(0.42)***  |
| Number of observations                            | 8379                             | 8379                             | 8127                             | 8127                             | 8127                             | 8127                             |
| Adj. R-squared                                    | 0.14                             | 0.14                             | 0.14                             | 0.14                             | 0.14                             | 0.14                             |

**Notes:** Variables explained in text. Robust standard errors in first brackets, clustered by country in second brackets: \* denotes significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Democracy defined according to Polity IV. Democratic capital variables computed with  $\delta = 0.99$  and  $\rho = 1.0$ , in consistency with the estimates in columns 3 and 4 of Table 2. All specifications include country and year fixed effects, indicators for war years and lagged war years, and an indicator for formerly socialist countries in Central and Eastern Europe and the Asian provinces of the former Soviet Union after 1989.