Democracy Doesn't Always Happen Over Night: Regime Change in Stages and Economic Growth*

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Abstract: How substantial are the economic benefits from democratic regime change? We argue that democratisation is often not a discrete event but a two-stage process: autocracies enter into 'episodes' of political liberalisation which eventually culminate in regime change or not. To account for this chronology and the implicit counterfactual groups, we introduce a repeated-treatment difference-in-difference implementation capturing non-parallel trends and selection into treatment. We find that modelling regime change in two stages rather than a single event yields stronger long-run growth effects. Among democratizers, experiencing repeated episodes without regime change reduces growth in democracy whereas length of episode does not.

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1 Introduction

When Nelson Mandela became President of South Africa in 1994, the country had successfully overcome Apartheid following a decades-long struggle by the African National Congress (ANC) using guerrilla tactics and mass mobilisation in the form of demonstrations, strikes and boycotts. Lifting the ban on the ANC in 1990 then-President F.W. de Klerk embarked on negotiations with Mandela on behalf of the white minority to safeguard their dominant position in South African politics but ultimately the country adopted universal suffrage and became an electoral democracy in which De Klerk served as Deputy President alongside Thabo Mbeki.

A drawn-out liberalisation process eventually culminating in democratic regime change is far from uncommon: the *median* length of time spent undergoing such a 'democratisation episode' for our sample of 62 countries (1950-2014) that eventually experienced regime change is nine years[1] — we elaborate below on definitions and data sources. An episode of democratization does not necessarily culminate in a transition to democracy: An additional 43 countries spent a median of six years in episodes but never experienced regime change.

Existing studies on the growth effects of democracy neither account for this drawn-out chronology nor for differences in growth patterns between autocracies that experienced a democratization episode and those that never did. Hence, previous research by design cannot consider (i) whether growth performance varies when we assume different counterfactual samples, (ii) the implications of repeated and/or lengthy episodes for subsequent growth under democracy, or (iii) a comparison of the growth experiences during ultimately failed versus successful episodes.

The first contribution of this paper is to accommodate the chronology of democratisation as a process rather than a discrete event (e.g. Geddes, 1999; Epstein et al., 2006) in the empirical analysis of the democracy-growth nexus: countries select into democratisation episodes, and some (but not all) select out of these episodes into democratic regime change.

[1]This allows for repeated episodes. 24 countries only experienced a single episode with a median length of four years.

Our approach is situated between studies which favour binary democracy indicators (e.g. Giavazzi and Tabellini, 2005; Rodrik and Wacziarg, 2005; Papaioannou and Siourounis, 2008; Acemoglu et al., 2019) and others which favour continuous measures (e.g. Knutsen, 2013; Murtin and Wacziarg, 2014; Madsen et al., 2015) in analysing the economic implications of democratic change.

Our second contribution is that we include countries with failed attempts at democratic regime change as a separate control group in our empirical analysis and study their growth experience during episodes. Including these countries in our analysis has important implications in terms of control group choice for the study of democracy and growth: we compare and contrast the long-run growth performance of successful democratisers between alternative 'counterfactual cases'. It enables us to distinguish between those nations which attempted democratization and those that did not, whereas conventional operationalisations capturing 'democratic transitions-as-events' combine these two groups as a supposedly homogeneous counterfactual case for successful regime change (Wilson et al., 2023). Our setup provides for a deeper investigation of the heterogeneous economic effects of regime change by analysing the implications of repeated and/or lengthy democratisation episodes. We can compare growth performance during episodes in the two types of countries and are able to highlight systemic determinants why some episodes do not culminate in regime change ('failed episode').

The third contribution of this paper is methodological: we extend previous causal inference in a heterogeneous Principal Component Difference-in-Difference (PCDID) framework to our proposed two-stage setup. In the first stage, autocracies either experience a democratization episode or not. In the second stage, this episode either ends with a regime change and the country transitions to democracy, or the episode 'fails' and the country remains autocratic. To empirically model these two stages, we rely on and extend a novel empirical implementation by Chan and Kwok (2022). The single-treatment model (henceforth Single PCDID) includes one treatment dummy (regime change) and relies on one control group (autocracies). Our extension to a repeated treatment (henceforth Double PCDID) uses two treatment dummies (episode, regime change) and two control groups: (i) autocracies which never experienced an episode, and (ii) autocracies which experienced an episode but not regime change.

These models estimate *country-specific* treatment effects and allow for non-parallel pre-treatment trends as well as endogenous selection into treatment. The adoption of heterogeneous treatment effect models is a crucial part of our empirics enabling us to provide new insights into the differences in the democracy-growth nexus across countries: existing research has near-unanimously assumed a common democracy-growth effect, yet the same literature recognises the potential for cross-country differences as motivated by arguments for a 'democratic legacy' (Gerring et al., 2005) or threshold levels in economic or human development as necessary conditions for a positive democracy-growth nexus (Aghion et al., 2007; Madsen et al., 2015; Acemoglu et al., 2019).[2]

In addition, we introduce a new way to present results by tying them closer to individual countries, rather than the average across or common estimate for all countries in the sample (ATET) as is standard in much of the literature: length of time spent in democracy varies greatly across countries, so that a pooled or Mean Group (Pesaran and Smith, 1995) estimate would implicitly or explicitly average across *some* countries which experienced decades in democracy and *others* which only experienced a few years. Using running line regressions we show the central tendencies in estimated country treatment effects *relative to the length of time spent in democracy*. We can further account for some of the difficulties in sample make-up which arise in cross-country data: differential sample start dates and the regime change histories of individual countries. By conditioning on the frequency of democratisation episodes, the years spent in episodes, and their estimated effect on development this approach furthermore enables us to account for the two-stage nature of democratic change we advocate. An alternative empirical specification dispenses with the running line predictions but provides a robustness check accommodating dynamic treatment effects.

The distinction between democratisation episode and regime change is quantified in the Varieties of Democracy (V-Dem) Episodes of Regime Transformation (ERT) dataset (Maerz et al., 2021; Edgell et al., 2020), which we analyse for 1950-2014, covering 227 episodes and

[2]See Eberhardt (2022) for a detailed motivation of the heterogeneous democracy-growth nexus.

70 regime changes in 105 countries.[3]

Our analysis offers a number of important new insights: first, modeling democratisation as a two-stage process yields higher long-run economic growth than assuming regime change 'over night'. Second, the magnitude of the democratic growth dividend decreases with the number of episodes a country experienced, but not with their duration in years. Third, countries that fail to successfully complete democratisation episodes appear to gain no growth benefits from these episodes. This suggests that growth dividends derive from the successful completion of an episode, not from experiencing an episode *per se*. Finally, auxiliary analysis suggests that failed episodes are associated with oil booms, pointing to a variant of the 'natural resource curse' in this political economy analysis.

The remainder of this paper is organised as follows: in the next section we discuss the conceptual foundations for political regime change as a non-binary event, introduce the data and present descriptive analysis. Section 3 covers the model setup and the empirical implementations in greater detail. Results and robustness checks are presented in Section 4, in Section 5 we conclude and speculate about the 'geographic origins' of our findings.

2 Regime Change as a Two-Stage Process

2.1 Conceptual Development

Our empirics capture two elements of democratisation: first, the notion that the initiation and completion of democratic liberalisation and regime change *takes time* (the rationale for 'episodes'); and second, a concern over those nations which initiated a process of liberalisation but were unable or unwilling to translate this into regime change (the rationale for considering an alternative counterfactual to regime change).

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[3]Our treated (control) sample comprises 62 (43) countries experiencing 141 (86) episodes, the median rate of 2 episodes per country is identical across samples. Appendix A provides details.

Empirical studies of democratisation are commonly focused on the analysis of electoral autocracies, so-called 'hybrid regimes' (Diamond, 2002; Brownlee, 2009; Geddes et al., 2014). These authors appear to tacitly agree that democratisation is an event, a single moment of "dramatic upheaval" (Gunitsky, 2014, 561) as in Huntington's (1991) 'democratic waves'.

Democratic transitions, however, are the result of a potentially lengthy process of political struggle between several actors (Rustow, 1970; Acemoglu and Robinson, 2006; Brownlee, 2007; Graham and Quiroga, 2012; Boese et al., 2021). Many formal models of nondemocratic politics can speak to this notion of the passing of time (Gehlbach et al., 2016): Liberalisation represents a period of uncertainty over the political trajectory of a country due to mass mobilisation or coalition formation. 'Cascading' protests and revolutionary movements take time to foment regime-busting power in the face of repression. Existing research in the comparative case study literature provides a self-preserving rationale for autocracies to engage in liberalisation (Magaloni, 2008; Levitsky and Way, 2010; Frantz and Kendall-Taylor, 2014), although they might end up as democracies 'by mistake' (Treisman, 2020). We can further draw on existing work on the rational delay to stabilisation policy (Alesina and Drazen, 1991), status-quo bias in the implementation of economic reforms (Fernandez and Rodrik, 1991), and the advantage of gradual economic reform under uncertainty (Dewatripont and Roland, 1995) to motivate the notion of political liberalisation episodes which 'take time.' Hence, while regime change as 'dramatic upheaval' undoubtedly does occur, these arguments suggest that establishing the political institutions of democracy frequently does not happen over night.

The conceptual distinction between episodes and regime change directly links to our second concern over the suitable control groups at each stage. Recent work by Geddes et al. (2014) highlights the relative ignorance in the empirical literature towards democratisation events which did *not* result in regime change. Levitsky and Way (2010, 52) point to the record of democratic transition during the 1990s which makes "the unidirectional implications of the word 'transitional' misleading". These thoughts create probing questions for the empirical literature on the democracy-growth nexus employing binary representations of democratic regime change: this practice assumes that within-category subjects are homogeneous (Wilson et al., 2023) and hence all 'negative' cases of transition are lumped together. A single

regime change dummy picks out the 'winners' of the liberalisation process, the null category contains the 'losers' and those who never tried.[4] What if this heterogeneity is key for understanding when democratic institutions foster economic growth? There is ample evidence for heterogeneous growth effects (Cervellati et al., 2014) and particularly so for autocracies (the main group of interest when studying transitions to democracy): the variation in growth outcomes is substantially higher among autocratic regimes, i.e. some autocracies have very high and others very poor growth outcomes (Persson and Tabellini, 2009; Knutsen, 2012). For the poorly-performing autocracies, democracy can act as a 'safety net' against disastrous economic outcomes (Knutsen, 2021) and hence they may attempt to undergo a process of liberalisation, while in the former an autocracy can perhaps 'grow itself out of' demands for political liberalisation, like China arguably has done for the past three decades.

2.2 Data Sources, Variable Transformations

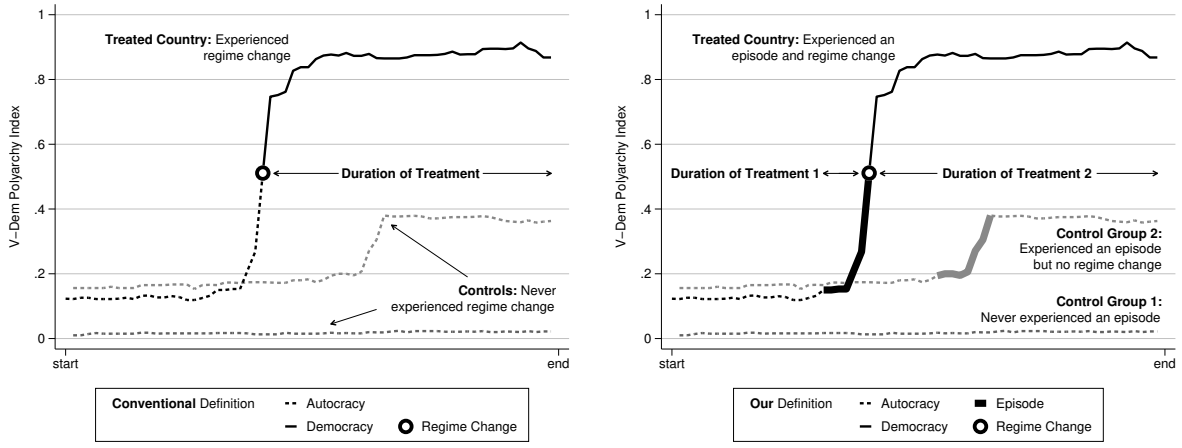
We use measures from the Episodes of Regime Transformation (ERT) dataset (Edgell et al., 2020), real per capita GDP and population from Bolt and van Zanden (2020, the 'Maddison data'), and exports and imports from Fouquin and Hugot (2016, TRADHIST). For comparison we also employ the Regimes of the World (Lührmann et al., 2018, ROW) democracy measure.[5]

We log-transform real per capita GDP and multiply this by 100: results are estimates of the percentage change in income following regime change. We use population growth and export/trade, aggregated from bilateral export and import flows, as controls: population growth is justified by the use of *per capita* GDP as dependent variable, while controlling for trade was found to substantially affect the magnitude of the estimated democracy effect

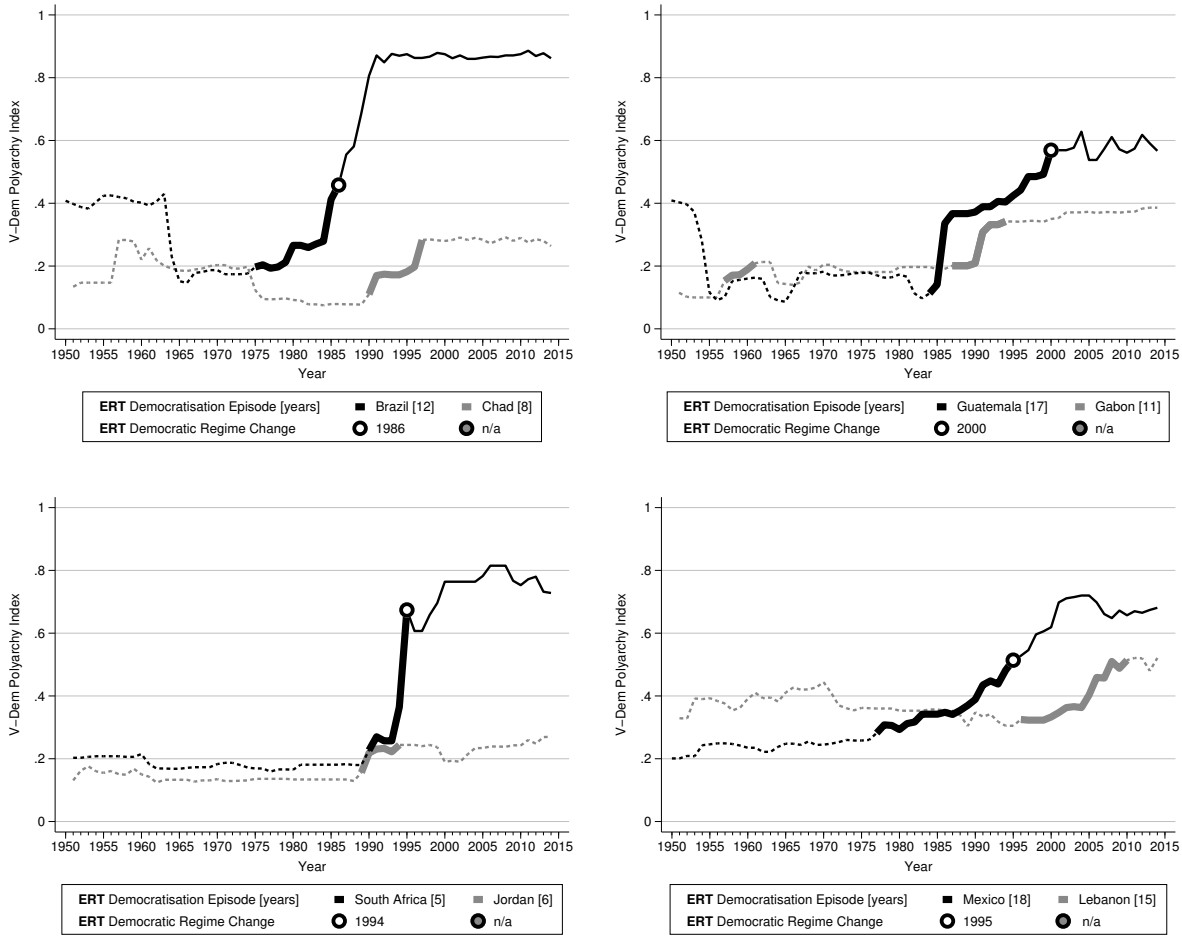
[4] In the literature using *continuous* democracy measures (e.g. Knutsen, 2013; Murtin and Wacziarg, 2014; Madsen et al., 2015) failed liberalisations are likewise undistinguished.

[5] Both, the ERT data and the ROW measure capture electoral democracy, i.e. free and fair elections, freedom of association and expression (Boese, 2019).

Figure 1: Some (Stylised) Examples of Democratisation



(a) Conventional (left) vs Episode-Regime Change (right) Chronology of Democratisation



(b) Some Examples of Successful and Failed democratisation episodes

Notes: We present the V-Dem polyarchy index evolution for country pairs, where the country in black experienced regime change and the country in gray did not. The period highlighted by the thick line represents the democratisation episode, following ERT (the length of each episodes in years is indicated in the legend). The 'Eastern' end of the thick black lines always coincides with the year of democratic regime change. A dashed (solid) thin line indicates the country regime is in autocracy (democracy) following the ERT definition. The circular marker indicates the year of democratic regime change (if applicable), which is required to include a 'founding election' (this explains the absence of regime change in Lebanon). We provide more examples in Appendix Figure A-2.

(Papaioannou and Siourounis 2008, Table 3 [5]; Acemoglu et al. 2019, Table 6 [6]).[6]

We adopt the democratic regime transformation dummy from ERT alongside the democratization episode dummy. The former builds on the ROW categorisation of democracy but further requires a founding democratic election to occur. A democratisation episode[7] requires (i) a small increase (0.01) in the V-Dem polyarchy index[8] for a country classified as 'closed' or 'electoral autocracy' (following the ROW categorization: Lührmann et al., 2018); and (ii) a total increase of at least 0.1 in the same measure over the course of the episode. An episode ends after a final year with an increase of at least 0.01 if this is followed by a year-on-year drop of 0.03, a cumulative drop of 0.1 over several years, or a 5-year stasis. Appendix E provides results using a range of alternative parameter values to define episodes.

Figure 1 highlights the difference between thinking of democratisation as a binary event vs a two-stage process: Panel (a) contrasts the single treatment approach (left diagram), including the conflation of heterogeneous control groups, with the two-stage treatment approach suggested in this paper (on the right), highlighting democratisation episodes as first-stage treatments followed by democratic regime change as second-stage treatments along with respective control groups. Panel (b) charts the development of electoral democracy

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[6] In robustness analysis we run the PCDID regressions without these controls.

[7] Our analysis focuses on episodes of democratisation originating in autocracies. In order to obtain separate treatment effect estimates for episodes and regime changes we exclude episodes of democratic deepening from our analysis and adopt the ERT episode indicator for a 'liberalizing autocracy': our episode dummy always reverts to 0 in the first year of democracy.

[8] Polyarchy is also referred to as the Electoral Democracy Index. It is continuous, $\in [0, 1]$ and represents a minimal definition of democracy favored in political science (Teorell et al., 2019; Boese, 2019). The 0.01 annual increment may seem small, 1% of the range of the index, yet between 1900 and 2018 over 70% of annual increments in the polyarchy index are between -0.01 and 0.01 (Wilson et al., 2023).

(V-Dem's polyarchy index) in four country pairs which experienced democratisation episodes (thick lines) but with differential outcomes (regime change, solid thin line, or not, dashed thin line) — Appendix Figure A-2 provides additional examples. These graphs demonstrate that the outcome of political episodes is uncertain: country pairs starting out with near-identical polyarchy scores in the 1950s at times end up at opposite ends of the scale in 2014.

All variables are available from 1901 to 2014, but we limit our analysis to 1950-2014: our methodology, which relies on common factors extracted from two sets of control groups, would not yield reliable results for the longer panel since only a handful of countries in the respective control groups have observations in the first half of the 20th century. This highlights that our approach forces us to consider the relative sample sizes of treated and various control groups — we regard this as a core strength of this methodology.

Our 1950-2014 sample covers 62 'treated' countries which experienced episodes and regime change (n=3,724),[9] 43 autocratic countries which only experienced episodes (n=2,515; control group 2), and 15 autocratic countries which never experienced episodes (n=646; control group 1). The median episode length in treated countries is nine years (stdev. 5.8), and eleven years (stdev. 8.1) in countries where episodes did not lead to regime change; in either group there were a median of two episodes per country (stdev. 1.1). We provide descriptive statistics, graphs and further details on the three samples under analysis in Appendix A.

3 Empirical Strategies

In this section we introduce the novel empirical implementations we employ to study the economic effect of democratisation when regime change is modelled either as a single or a repeated 'treatment'. We discuss the Chan and Kwok (2022) Principal Component Difference-in-Difference estimator (Single PCDID) and our extension (Double PCDID) for these respec-

[9]We cannot use all 71 countries available since nine of them have no pre-episodal observations which prevents separate identification of episode and regime change effects (see Appendix Table A-3).

tive cases. The final part of the section introduces our novel presentation of heterogeneous treatment effects using predictions from running line regressions.

3.1 Single PCDID

In the Single PCDID approach democratisation is modelled as a binary event. The PCDID estimator allows for endogenous selection into regime change and potentially non-parallel pre-treatment trends between treated and non-treated (never-regime changing) countries. This is achieved by including estimated common factors — extracted via Principal Component Analysis (PCA) from a control sample regression[10] — in the treatment regression. The use of common factors has a long tradition in the macro panel literature to capture strong cross-section dependence (e.g. Pesaran, 2006; Bai, 2009), a form of unobserved, time-varying heterogeneity.[11] The most recent contributions extended the use of common factors to the empirics of policy evaluation (Gobillon and Magnac, 2016; Xu, 2017; Chan and Kwok, 2022).

Assumptions of parallel trends and exogenous treatment in standard treatment effects models are violated if time-varying unobservables are correlated with the treatment variable.[12] Chan and Kwok's (2022) PCDID estimator by-passes these problems by adding proxies (estimated factors) for the time-varying unobservables as additional controls in heterogeneous

[10]The principal components are estimated from the residuals of a country-specific regression of income per capita on export/trade, population growth and an intercept. An alternative version omits the covariates.

[11]Strong cross-section correlation is distinct from weaker forms of dependence (e.g. spatial correlation) and can lead to omitted variable bias in the estimated coefficients (Phillips and Sul, 2003; Andrews, 2005).

[12]As an analogy, estimating production function regressions using OLS, the presence of unobserved TFP creates biased estimates for capital and labour due to the correlation between TFP and these inputs.

treatment regressions.[13]

Since these factors are added to *country-specific* regressions, the proxied unobservables can have a different impact across countries. Most importantly: treated and control country outcomes can have different trends. Furthermore, because these factors can be correlated with the treatment variable, we can suggest that democratic regime change can be correlated with unobserved determinants of economic development (e.g. absorptive capacity, culture): regime change can be endogenous. We now discuss this more formally.

Setup Using the potential outcomes framework, the observed outcome of a single treatment D_{it} for panel unit i at time T_0 can be written as

$$y_{it} = D_{it}y_{it}(1) + (1 - D_{it})y_{it}(0) = \Delta_{it}\mathbf{1}_{\{i\in E\}}\mathbf{1}_{\{t>T_{0i}\}} + y_{it}(0) \tag{1}$$

$$\text{with} \qquad y_{it}(0) = \varsigma_i + \beta_i' x_{it} + \mu_i' f_t + \widetilde{\epsilon}_{it}, \tag{2}$$

where the indicator variables $\mathbf{1}_{\{\cdot\}}$ are for the panel unit and the time period treated, respectively, Δ_{it} is the time-varying heterogeneous treatment effect, x the observed covariates with associated country-specific parameters β_i,[14] $\mu_i' f_t$ represents a set of unobserved common factors f_t with country-specific factor loadings μ_i, and $\widetilde{\epsilon}_{it}$ is the error term.

The treatment effect is assumed to decompose into $\Delta_{it} = \overline{\Delta}_i + \widetilde{\Delta}_{it}$, with $E(\widetilde{\Delta}_{it}|t >$

[13]The basic intuition for the PCDID follows that of the control function approach in microeconometric analysis of production functions (Olley and Pakes, 1996) which use combinations of observed choice variables (like material inputs) to construct a proxy of how firms react to changes in unobserved TFP. Continuing the analogy, the common factor structure, f, in combination with a heterogeneous parameter regression, μ_i, can proxy time-varying, heterogeneous TFP and hence eradicates the omitted variable bias problem (see Eberhardt and Teal, 2011).

[14]As common in the literature (Pesaran, 2006) we assume $\beta_i = \bar{\beta} + \tilde{\beta}_i$ where $E[\tilde{\beta}_i|x_{it}, f_t, \varepsilon_{it}] = 0$. Covariates x and factors f can be orthogonal or correlated.

$T_{0i}) = 0 \ \forall i \in E$ since $\widetilde{\Delta}_{it}$ is the demeaned, time-varying idiosyncratic component of Δ_{it}; we refer to $\overline{\Delta}_i$ as ITET, the individual treatment effect averaged over the treatment period — our key parameter of interest. The reduced form model is

$$y_{it} = \overline{\Delta}_i \mathbf{1}_{\{i \in E\}} \mathbf{1}_{\{t > T_{0i}\}} + \varsigma_i + \beta_i' x_{it} + \mu_i' f_t + \epsilon_{it} \qquad \text{with} \qquad \epsilon_{it} = \widetilde{\epsilon}_{it} + \widetilde{\Delta}_{it} \mathbf{1}_{\{i \in E\}} \mathbf{1}_{\{t > T_{0i}\}}, \quad (3)$$

where given the treatment effect decomposition the composite error ϵ_{it} has zero mean but can be heteroskedastic and/or weakly dependent (spatially/serially correlated).

The combination of common factors and heterogeneous parameters allows for potentially non-parallel trends across panel units, most importantly between treated and control units. The above setup can accommodate endogeneity of treatment D_{it} in the form of *inter alia* correlation between treated units and factor loadings, the timing of treatment and factor loadings, or between observed covariates and timing or units of treatment. Finally, the implementation allows for nonstationary factors f_t.

Implementation The estimation of the country-specific treatment effect (ITET) $\overline{\Delta}_i$ proceeds in two steps: first, using PCA, we estimate proxies of the unobserved common factors from data in a control group equation; second, country-specific least squares regressions for treatment group countries are augmented with these factor proxies as additional regressors.

The estimation equation for treated country $i \in E$ is then:

$$y_{it} = b_{0i} + d_i \mathbf{1}_{\{t > T_{0i}\}} + a_i' \hat{f}_t + b_{1i}' x_{it} + u_{it}, \quad (4)$$

where $\hat{f}$ are the estimated factors obtained by PCA on the residuals $\hat{e}$ from the heterogeneous regression $y_{it} = b_{0i} + b_{1i}' x_{it} + e_{it}$ in the control group sample, and d_i is the country-specific parameter of interest. We estimate (4) augmented with one to six common factors. See Section 3.3 for inference.

Assumptions The main assumptions required for the consistency of ITET estimates are that the unobservables can be represented by a low-dimensional multi-factor error structure,[15] $\mu_i' f_t$ (as in Pesaran, 2006; Bai, 2009; Athey et al., 2021), and that u is orthogonal to all conditioning components in equation (4): this implies that all aspects of treatment endogeneity and non-parallel trends are assumed to be captured by the factors, the controls, and the deterministic term as well as their combinations/correlation with the treatment variable. We discuss threats to identification and how we test for these below.

3.2 Double PCDID

The 'double-treatment' case argues for democratic regime change as a repeated selection problem: (i) At time T_0 an autocracy starts democratic liberalisation, i.e. it endogenously selects into a democratisation episode as defined by ERT. The control group for this first treatment are all autocracies which never experience an episode. (ii) Of those autocracies which experienced a democratisation episode we find two types: first, those which successfully transitioned into democracy, and second, those which failed. From the pool of autocracies experiencing an episode we thus have a country which at time T_1 endogenously selects into democratic regime change as defined by ERT. The control group for this second treatment constitutes all autocracies with at least one episode but which never transition into democracy. We posit that countries that tried and failed in their quest for democracy are an interesting and meaningful control group for countries which successfully transitioned.

Correcting for repeated treatment requires the use of estimated common factors from two control groups. The two sets of common factors account for non-parallel trends prior to the two treatments, and in analogy to the single treatment case above, these common

[15] Since factor proxies are measured with error, the idiosyncratic errors of treated and non-treated units may be correlated — the resulting bias disappears asymptotically if $\sqrt{T}/N_C \to 0$, where T is the time series dimension of the treated sample and N_C is the number of control sample units.

factors can be correlated with treatments or observed covariates, amounting to treatment endogeneity.

Setup We extend the PCDID to a repeated-treatment 'Double PCDID' specification:

$$\begin{aligned} y_{it} &= \Delta_{it}^{\mathsf{A}} \mathbf{1}_{\{i \in E^*\}} \mathbf{1}_{\{t > T_{0i}\}} + \Delta_{it}^{\mathsf{B}} \mathbf{1}_{\{i \in E^*\}} \mathbf{1}_{\{t > T_{1i} > T_{0i}\}} \\ &\quad + \varsigma_i + \beta_i' x_{it} + \mu_i^{\mathsf{A}\prime} f_t^{\mathsf{A}} + \mu_i^{\mathsf{AB}\prime} f_t^{\mathsf{AB}} + \tilde{\epsilon}_{it}. \end{aligned} \tag{5}$$

We now distinguish two treatments: A for the treatment at T_0 and B for a second, later treatment at $T_1 > T_0$, yet conditional on having received treatment A. The treatment group is now made up of those panel units which experienced *both* treatments ($i \in E^*$). In analogy there are now two control groups: (1) all those units which never experienced treatment A, and (2) those units which experienced treatment A but not treatment B (see below for notation). We now assume two sets of multi-factor error terms: one for each counterfactual group. The reduced form is

$$\begin{aligned} y_{it} &= \overline{\Delta}_i^{\mathsf{A}} \mathbf{1}_{\{i \in E^*\}} \mathbf{1}_{\{t > T_{0i}\}} + \overline{\Delta}_i^{\mathsf{B}} \mathbf{1}_{\{i \in E^*\}} \mathbf{1}_{\{t > T_{1i} > T_{0i}\}} \\ &\quad + \varsigma_i + \beta_i' x_{it} + \mu_i^{\mathsf{A}\prime} f_t^{\mathsf{A}} + \mu_i^{\mathsf{AB}\prime} f_t^{\mathsf{AB}} + \epsilon_{it} \end{aligned} \tag{6}$$

using similar arguments as in the single intervention case. The assumptions from the Single PCDID case extend to this model.

Implementation The estimation of the regime change ITET $\overline{\Delta}_i^B$ again proceeds in two steps: first, using PCA we separately estimate proxies of the common factors in the two control groups; second, the estimation equation for treated country $i \in E^*$ is

$$y_{it} = b_{0i} + d_i^A \mathbf{1}_{\{t > T_{0i}\}}^A + d_i^B \mathbf{1}_{\{t > T_{1i} > T_{0i}\}}^B + a_{1i}^{\mathsf{A}\prime} \hat{f}_t^{\mathsf{A}} + a_{2i}^{\mathsf{AB}\prime} \hat{f}_t^{\mathsf{AB}} + b_{1i}' x_{it} + e_{it}, \tag{7}$$

where d_i^A and d_i^B are the country-specific treatment parameters for episodes and regime change. The $\hat{f}$ with superscript A are the estimated factors obtained by PCA from the

residuals $\hat{e}$ of a heterogeneous regression $y_{it} = b_{0i} + b'_{1i}x_{it} + e_{it}$ in the first control group. The $\hat{f}$ with superscript (A~~B~~) are estimated from the residuals of the following regression in the second control group: $y_{it} = b_{0i} + d_i^A \mathbf{1}^A_{\{t>T_{0i}\}} + a_{1i}^{A\prime} \hat{f}_t^{A} + b'_{1i}x_{it} + e_{it}$, where the presence of the episode dummy and the $\hat{f}_t^{A}$ accounts for the endogeneous selection of countries into episodes.[16] We estimate (7) with one to six common factors extracted from each control group. See Section 3.3 for inference.

Threats to Identification One concern is the effect of idiosyncratic shocks which may induce countries to trigger regime change: a country experiencing a democratisation episode may transition to democracy because of a fortunate natural resource discovery, or it might have been hindered by a financial crisis or natural disaster. We know that oil exploration is guided by global prices, while financial crises have sizeable international dimensions (Cesa-Bianchi et al., 2019; Arellano et al., 2017) — all arguments in favour of our factor structure. In Appendix C we run separate event analyses for GDPpc growth and change in V-Dem's polyarchy index in treatment and control samples adopting event dummies constructed from data collated by Reinhart and Rogoff (2009), Cotet and Tsui (2013), Laeven and Valencia (2020) and EM-Dat. These suggest no systematic differences in the effects between the two groups.

We also study the parallel trend assumption in adjusting the parallel trend test under factor structure by Chan and Kwok (2022) for our Double PCDID setup. Appendix D introduces this test and results.

Finally, although we know that adding 'too many' estimated factor in principle does little harm to our treatment estimates (Moon and Weidner, 2015), the Double PCDID requires substantially more degrees of freedom and we check its robustness using a range of factor augmentations in Appendix Figure B-2.

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[16] We are grateful to a referee for pointing out that our original auxiliary regression would lead to inconsistent estimates of $\hat{f}_t^{A~~B~~}$.

3.3 Heterogeneous Treatment Effects and Inference

We estimate both of our models country by country. Consequently, the Single and Double PCDID models yield N country-specific treatment estimates (ITET) for regime change. A typically useful estimate to present is the ATET, which in our setup would be $\overline{\overline{\Delta}} = E(\overline{\Delta}_i)$, the average of the ITET across treated units $i \in E$ or E^*.[17] Focusing on the ATET would make sense when studying a treatment effect that manifests itself *in its entirety* after a small number of years, as would be the case for many medical interventions.[18] In the context of the democracy-growth nexus we propose an alternative means of presentation, namely predictions from running line regressions of the estimated ITET for democratic regime change, $\hat{d}_i$ or $\hat{d}_i^B$ for Single and Double PCDID, on the years of treatment.

We chose this form of presentation since the effect of democracy on growth potentially differs within countries over time: New democracies may suffer from 'democratic overload', drawn to short-termism, and with too many processes not yet formalised they frequently represent "boisterous, obstreperous affairs" (Gerring et al., 2005, 335). But *over time*, politicians, bureaucrats and citizens learn how democracy works, while decisions and bureaucratic processes become formalised and hence predictable (ibid).

A running line regression smooths the dependent variable against an independent variable by using subsets of nearest neighbours in local linear regressions. Using predictions from *multivariate* running line regression allows us to simultaneously smooth on *multiple* independent variables. This form of presentation has a number of advantages: (i) we do not average

[17] Results for the ATET from Single and Double PCDID models are presented in Appendix Table B-1.

[18] We also point to the recent insights regarding the decomposition of a 'pooled' DID ATET estimate in the context of variation in treatment timing (Goodman-Bacon, 2021). Heterogeneous estimators do not face similar ambiguities of interpretation (weighting) and our running line regressions put 'treatment length' (early vs late treatment) at the heart of the results.

across different countries with dozens or just a few years in democracy; (ii) we can account for differential sample observations and for multiple regime changes in each country;[19] and, for the Double PCDID, (iii) we can condition on the novel two-stage setup advocated here, by controlling for the number of episodes, the years spent in these episodes, and the magnitude of the episode effect $\hat{d}_i^A$.

In analogy to a standard Mean Group estimator, the ATET in the Chan and Kwok (2022) PCDID is simply the average across all treated units, $\hat{d}^{MG} = N^{-1} \sum_i \hat{d}_i$, with a nonparametric variance estimator following Pesaran (2006): $\widehat{\text{var}}(\hat{d}^{MG}) = [N(N-1)]^{-1} \sum_{i=1}^{N} (\hat{d}_i - \hat{d}^{MG})^2$. We view running line regressions as 'local ATET', where 'local' refers to a similar number of years spent in democracy, and simply adopt the standard errors from this methodology.[20]

4 Empirical Results

Visual Presentation of Results We estimate the both PCDID models country by country. Thus, we obtain individual coefficients for each country (ITET) rather than a single treatment effect for all countries.[21] Regardless of whether we think of democratisation as a one-step or two-step process, individual countries enter our sample at different times, spend different periods of time in democracy, and may or may not experience (temporary) reversion to autocracy. These aspects matter for assessing the effect of democracy on growth and simply displaying

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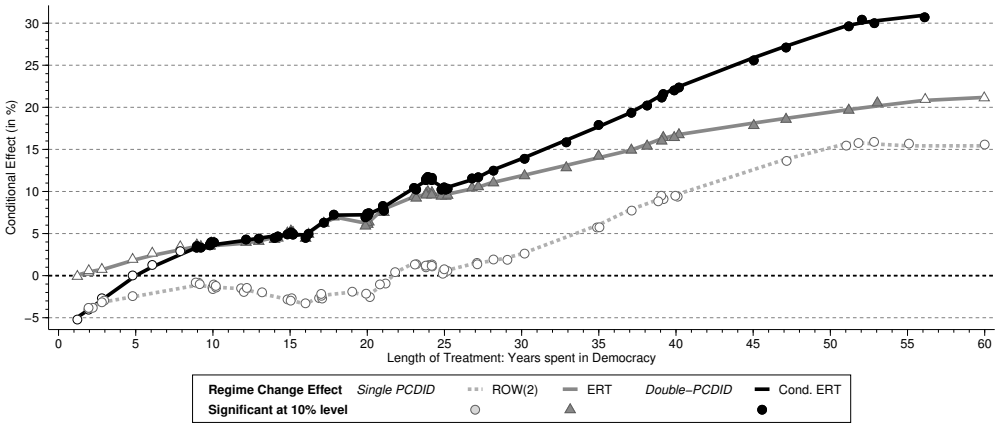
[19] Most of the existing literature on democracy and growth models democratisation as a one-off event, ignoring the empirical reality that some countries flip back and forth between regimes. Exceptions include Przeworski et al. (2000), Papaioannou and Siourounis (2008) and Eberhardt (2022).

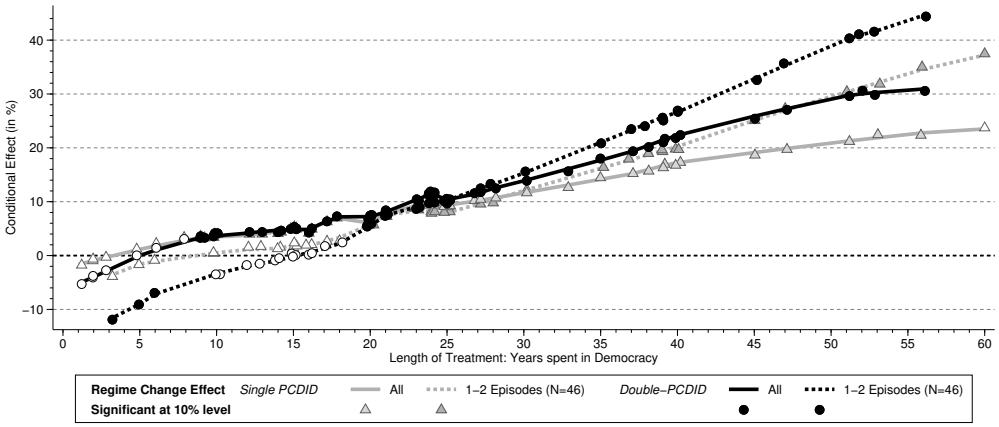
[20] Since there may be concerns that these standard errors do not fully account for the correlation amongst the regressors we employ bootstrap methods to show that using bias-corrected confidence intervals (Appendix Figure B-2) the patterns of statistical significance are similar to those in the uncorrected results.

[21] We present *average* effects (ATET) in Appendix Table B-1.

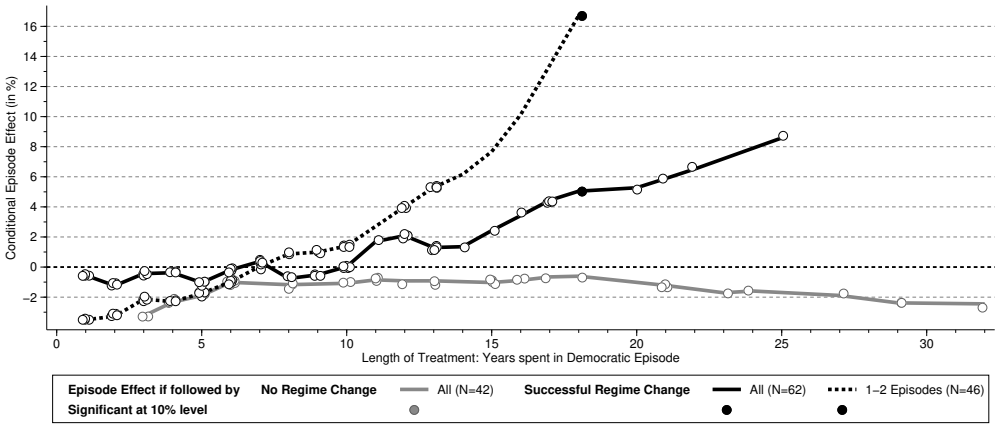
Figure 2: Regime Change, Episodes and Economic Growth — Single and Double PCDID



(a) Comparison of Single and Double PCDID Results for Regime Change



(b) Dto., with Focus on Countries with at most two Episodes



(c) Comparing Episode Effects in Countries with and without Regime Change

Notes: These plots present the causal effect of time spent in democracy (or, in Panel (c), in an episode) on income per capita. These are predictions from multivariate running line regressions of country-specific democracy or episodes effects (y-axis) on years spent in democracy or episodes (x-axis) and additional controls (see main text). The sample matches that of the Double PCDID estimates for ERT (62 treated countries, unless indicated), all results are for PCDID models augmented with 3 common factors for each control group — this is the preferred model on the basis of Chan and Kwok (2022) Alpha tests. Panel (a) presents Single PCDID alongside Double PCDID (in black) results. Panel (b) contrasts Single and Double PCDID results for all 62 countries with those for 46 which experienced at most 2 liberalisation episodes. In Panel (c) we report the estimates for episodes in 62 countries with regime change and those in 43 without. In the former, we further distinguish countries with at most 2 episodes like in Panel (b).

the individual coefficients without accounting for them would be misleading. Instead, we employ predictions from running line regressions to condition on these country-specific differences and to display our findings.

Panel (a) of Figure 2 presents the results from both approaches: (i) the findings from the Single PCDID model are displayed for two democracy measures: a dummy for the ROW measure (dashed line), and a dummy for the ERT regime type dummy (gray solid line) and (ii) the results from the Double PCDID model (black solid line). In all cases the democracy effect (in percent, y-axis) is smoothed over the years the country spent in democracy (x-axis) using multivariate running line regression. In the Single PCDID we control for (i) the start year of the country series, and (ii) the number of times a country moved into or out of democracy; in the Double PCDID we additionally control for (iii) the number of democratisation episodes, (iv) the years spent in episodes, and (v) the coefficient estimate on the episodes dummy, $\hat{d}_i^A$.

The interpretation of these graphs is that the years spent in democracy indicated on the x-axis cause the percentage increase in income per capita indicated on the y-axis. Filled (white) markers indicate statistical (in)significance at the 10% level.[22]

Democratic Regime Change The treatment effects and their relationship with time spent in democracy are very similar for the two democracy indicators using Single PCDID (dashed and gray solid lines): effects are moderately positive and statistically insignificant for the first 15 years, whereupon additional years spent in democracy lead to a rise in income up until a peak around 40 years of 'treatment', which is associated with 18-20% higher per capita GDP. Thereafter, the effect plateaus.

When accounting for the episodic nature of democratisation in the Double PCDID (the black solid line) regime change implies a more substantial long-run effect on development: in the early years these estimates are very similar to those when episodes are ignored, but from around thirty years onwards the effect continues to increase to reach around 30% higher

[22]The sample size is limited to the same 62 'treated' countries in the Double PCDID analysis.

income after 50 years in democracy.[23] Standard ATET estimates (Appendix Table B-1) fail to provide this insight offered by our running line predictions.

Our Double PCDID approach yields identical results if we exclude countries with very short episodes (≤ 2 years) — see Panel (a) of Appendix Figure B-1. Indeed, additional analysis in Panel (b) of the same Appendix Figure indicates that *length of time* spent in episodes is not linked to subsequent growth performance in democracy. However, the *number* of episodes experienced plays an important role: Panel (b) of Figure 2 suggests that countries with at most two episodes (dashed black line) have considerably higher long-run income effects of around 40% after 50 years in democracy compared with the full sample (solid black line).

Successful and Failed Episodes In Panel (c) of Figure 2 we compare the effect of *episodes* (not regime change like in the above panels) on income in countries which did (solid and dashed black lines for full sample and countries with one or two episodes, respectively) or did not (gray line) experience regime change.[24] While none of the three line plots are statistically significant, the contrast is still indicative: not only deprived from a boost to income *in democracy*, countries which did not experience regime change also failed to benefit economically from their time in episodes. It is not an episode *per se*, but its successful completion that matters for growth.

Given the significance we put on successful versus failed episodes, we provide additional insights into the dominant determinants of 'episodal failure'. In supplementary analyses available upon request, we develop an empirical Early Warning System inspired by the literature on financial crises (Eberhardt and Presbitero, 2021). Across a range of specifications we find that oil booms (rather than coup attempts or natural disasters, among others) are associated with large and significant increases in the propensity for an episode to end without democratic regime change.

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[23] Panel (a) of Appendix Figure B-2 presents 90% confidence intervals for the ERT estimates using Single and Double PCDID, which overlap.

[24] The latter is derived from regressions in the control sample of 43 countries.

Robustness All of the above estimates are constructed from PCDID models where we include three common factors estimated from each control group — diagnostic tests presented in Appendix D provide favourable results for this specification choice. We could be concerned that this choice fails to capture all the unobserved heterogeneity. In Panel (a) of Appendix Figure B-2 we show the regime change estimate for the augmentation with three common factors (from each control group) in black alongside alternative specifications with 1 to 6 common factors (dto). Augmented with only one or two factors the estimate for the democracy-growth nexus is attenuated but still reaches 20% higher per capita GDP. Including three or more common factors leads to qualitatively very similar results, as predicted by theory (Moon and Weidner, 2015).

The running line predictions based on local linear regression presented above do not account for all the correlation between the underlying variables (here: estimates) and we therefore use the bootstrap to address this concern. Panel (c) of Appendix Figure B-2 suggests that patterns of statistical significance are similar to those in our main results.

All of our results above include export/trade and population growth as additional covariates, raising concerns that these may represent *outcomes* of democratic regime change. A version of the Single and Double PCDID excluding these produces identical *relative* patterns of results (see Appendix Figure B-3).

We rely in our definition of episodes on the parameters spelled out in Section 2.2. In Appendix E we present results for a wide range of alternative parameterisations (see Table note for details), which yield qualitatively very similar results.

Finally, in Appendix F we devise an alternative implementation capturing dynamic treatment effects for which results closely match those from our running line predictions in Figure 2.

5 Conclusion

Recent efforts in the analysis of the democracy-growth nexus have emphasised that great care needs to be taken in defining democratic regime change (Papaioannou and Siourounis, 2008; Acemoglu et al., 2019) and that there is substantial heterogeneity in the growth performance

across democratizers (Cervellati and Sunde, 2014; Eberhardt, 2022). Building on this literature, our paper motivates and empirically implements democratisation as a two-stage process, made up of a liberalisation episode and regime change. This chronology enables us to provide a more nuanced analysis of the long-term growth implications of democratisation.

Our results suggest that modeling democracy as a two-stage process yields even higher economic growth in the long-run. Repeated failed episodes prior to a successful democratic transition diminish subsequent growth in democracy, but the length of episodes does not. Countries that fail to successfully complete an episode appear to derive no growth benefits, which suggests that growth dividends hinge on the successful completion of an episode, not on experiencing an episode *per se*. Avoiding episode failure is clearly important. We identify a version of the natural resource curse as the most likely culprit for episode failure.

We report standard ATET for episodes and regime change (see Appendix Table B-1), but caution that these obscure the differences between results for democracy 'over night' versus a two-stage process. Our main insights derive from running line regressions which predict the trajectory of economic growth over the years spent in democracy and additionally account for the idiosyncracies of individual countries' data availability, their episode and regime change dynamics, as well as the implications of the episode and regime change chronology.

Our analysis highlights the importance of episode completion and, more generally, the heterogeneity of the democratic growth dividend. Why is it then that some countries experience repeated failed episodes whereas others just need one 'attempt'? What drives the differential patterns of growth under democracy? It stands to reason that factors related to the 'deep determinants of comparative development' may play an important role in answering these questions. We can think of several ways in which the 'unequal favours' of geography (Landes, 1999) may influence the magnitude of the democracy-growth effect: first, democracy fosters structural change (Acemoglu et al., 2015), yet geography (climate, crop type, disease environment) can lead to differential *speeds* of structural transformation and hence development (Vollrath, 2011; Eberhardt and Vollrath, 2018; Johnson and Vollrath, 2020); second, political institutions foster financial development (Rajan and Zingales, 2003; Degryse et al., 2018), but 'poor' geography limits investment opportunities in countries lacking market

access (Malik and Temple, 2009) and/or with a narrow range of (primary) exports. We seek to investigate these factors in future research.

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