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. Our approach is

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

situated between studies which favour binary democracy indicators (e.g. Giavazzi & Tabellini 2005, Rodrik & Wacziarg 2005, Papaioannou & Siourounis 2008, Acemoglu et al. 2019) and others which favour continuous measures (e.g. Knutsen 2013, Murtin & 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. 2022). 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 & 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 & Smith 1995) estimate would implicitly or explicitly average across *some* countries which experienced decades and *others* which only experienced a few years of democracy. 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 70 regime changes in 105 countries.[3]

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

[3]Our treated (control) sample comprises 62 (43) countries experiencing 141 (86) episodes, the median rate

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).

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

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of 2 episodes per country is identical across samples. Appendix A provides details.

"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 & Robinson 2006, Brownlee 2007, Graham & Quiroga 2012). 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 & Way 2010, Frantz & 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 & Drazen 1991), status-quo bias in the implementation of economic reforms (Fernandez & Rodrik 1991), and the advantage of gradual economic reform under uncertainty (Dewatripont & 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 & 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. 2022) 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 & 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 & van Zanden (2020, the 'Maddison data'), and exports and imports from Fouquin & 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 (Papaioannou & Siourounis 2008, Table 3 [5]; Acemoglu et al. 2019, Table 6 [6]).[6]

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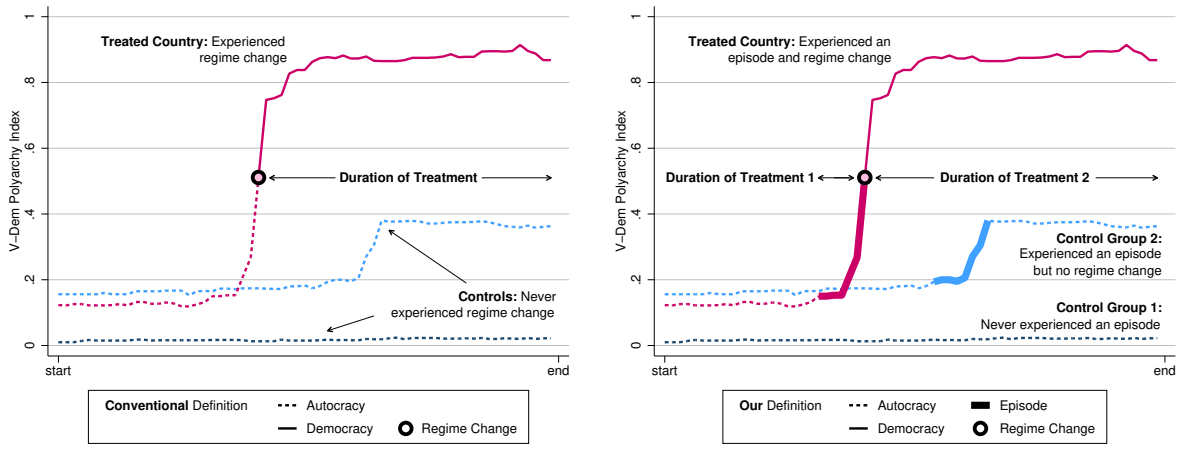
[4] In the literature using *continuous* democracy measures (e.g. Knutsen 2013, Murtin & Wacziarg 2014, Madsen et al. 2015) failed liberalisations are likewise undistinguished.

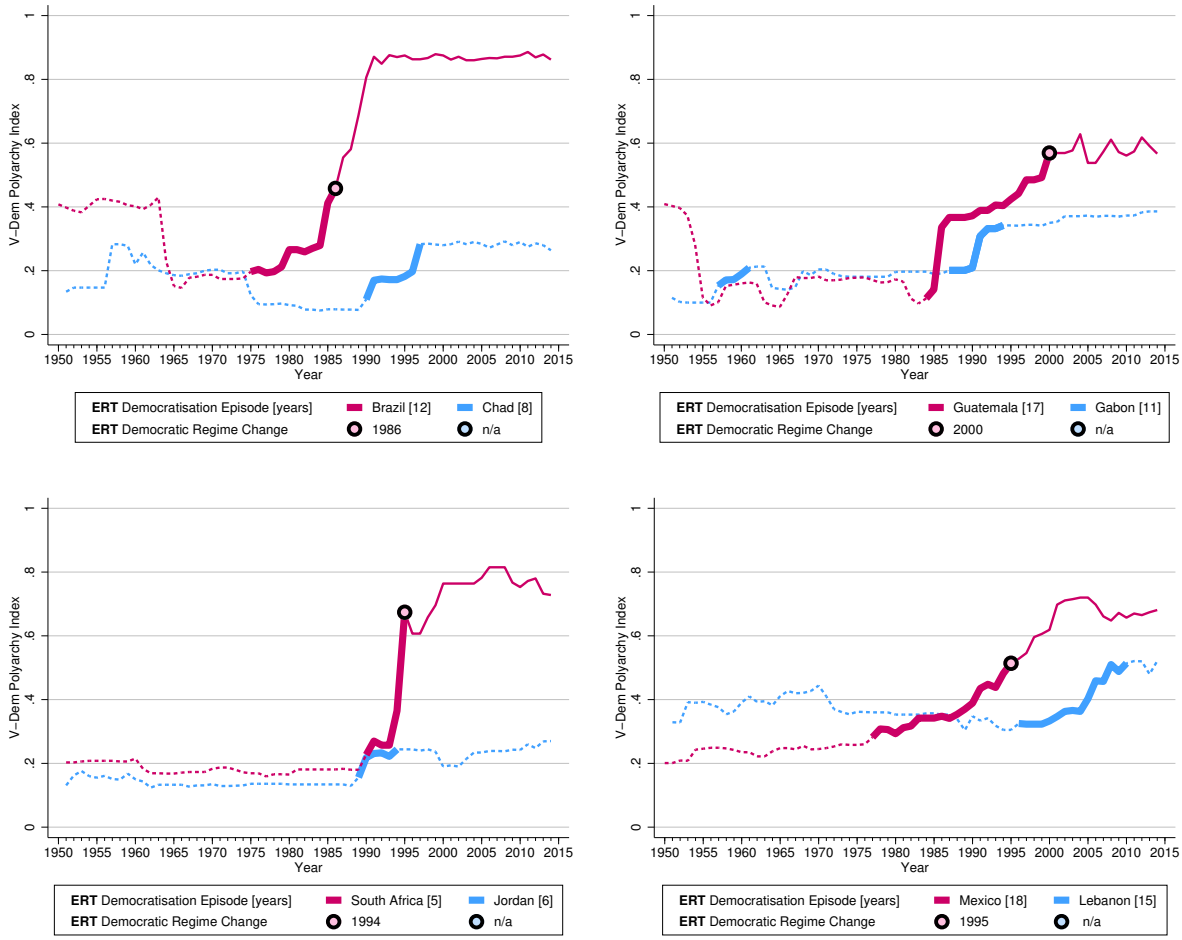
[5] Both, the ERT data and the ROW measure capture electoral democracy, i.e. free and fair elections, freedom of association and expression (Boese 2019).

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

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 dark pink experienced regime change and the country in light blue 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 pink 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.

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 (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

[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. 2022).

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), 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).[9] The median episode length in treated countries is four years (stdev. 3.3), and six years (stdev. 3.4) 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 & Kwok (2022) Principal Component Difference-in-Difference estimator (Single PCDID) and our extension (Double PCDID) for these respective 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

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

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 & Magnac 2016, Xu 2017, Chan & 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 & Kwok's (2022) PCDID estimator by-passes these problems by adding proxies (estimated factors) for the time-varying unobservables as additional controls in heterogeneous 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.

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[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 these 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 & 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.

[13] The basic intuition for the PCDID follows that of the control function approach in microeconometric analysis of production functions (Olley & 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 & Teal 2011).

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 $E(\widetilde{\Delta}_{it}|t > T_{0i}) = 0 \ \forall i \in E$ 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

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

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.

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[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.

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 of treatment group countries are augmented with these factor proxies as additional regressors.

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

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

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

[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.

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 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

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

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 (AB) are estimated from the residuals of the following regression in the second control group: $y_{it} = b_{0i} + d_i^A \mathbf{1}^A_{\{t>T_{0i}\}} + a_{1i}^{\text{A}\prime} \hat{f}_t^{\text{A}} + b_{1i}' x_{it} + e_{it}$, where the presence of the episode dummy and the $\hat{f}_t^{\text{A}}$ 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 & Rogoff (2009), Cotet & Tsui (2013), Laeven & Valencia (2020)

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

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 & 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 & 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.

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 (INET) 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*, politi-

[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.

cians, 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 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 & 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]

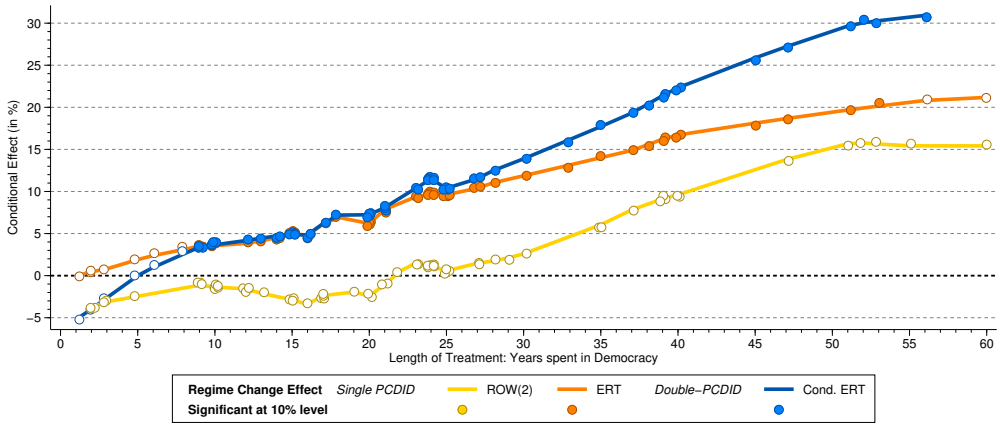
4 Empirical Results

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

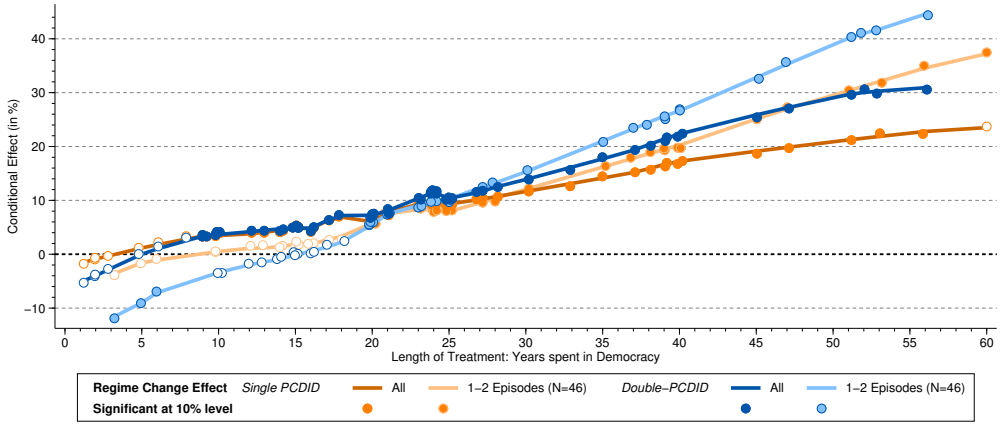
[19] Most of the existing literature on democracy and growth models democratisation as a one-off event, ignoring the empirical reality that some countries flip back and forth between regimes. Exceptions include Przeworski et al. (2000), Papaioannou & Siourounis (2008) and Eberhardt (2022).

[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.

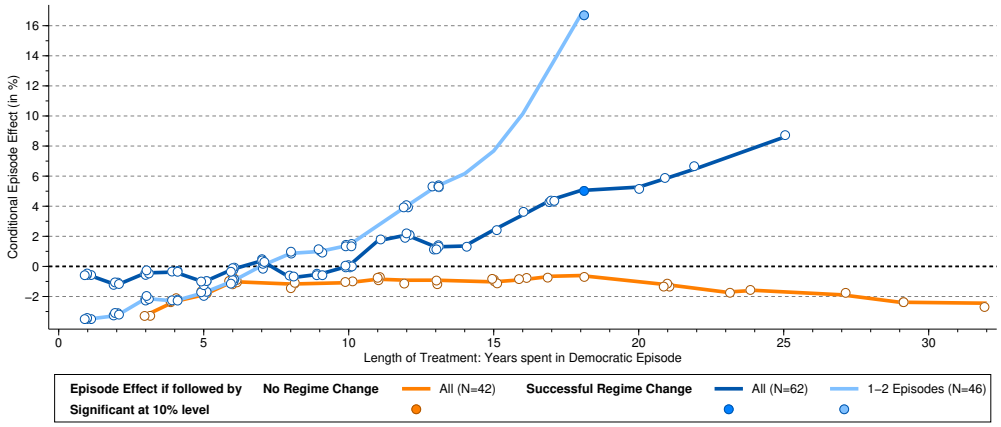
Figure 2: Regime Change, Episodes and Economic Growth — Single and Double PCDID



(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 maintext). 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 & Kwok (2022) Alpha tests. Panel (a) presents Single PCDID alongside Double PCDID (in blue) 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).

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 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 (yellow line), and a dummy for the ERT regime type dummy (orange line) and (ii) the results from the Double PCDID model (dark blue 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 (orange and yellow 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.

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

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

When accounting for the episodic nature of democratisation in the Double PCDID (the dark blue 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 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 (light blue line) have considerably higher long-run income effects of around 40% after 50 years in democracy compared with the full sample (dark blue 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 (dark and light blue lines for full sample and countries with one or two episodes, respectively) or did not (orange 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

[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.

on financial crises (Eberhardt & 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.

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 dark blue 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 & 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 (b) 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 & Siourounis 2008, Acemoglu et al. 2019) and that there is substantial heterogeneity in the growth performance across democratizers (Cervellati & 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 significant 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 & Vollrath 2018, Johnson & Vollrath 2020); second, political institutions foster financial development (Rajan & Zingales 2003, Degryse et al. 2018), but 'poor' geography limits investment opportunities in countries lacking market access (Malik & Temple 2009) and/or with a narrow range of (primary) exports. We seek to investigate these factors in future research.

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Appendix — Not Intended For Publication

A Data Appendix

A.1 Sources and Sample Makeup

Our choice of data sources (Maddison, TRADHIST) enables analysis from 1950-2014, but excludes a number of countries which are available in ERT from inclusion in the treatment or control groups: ten small treated economies (Bhutan, Fiji, Guyana, Kosovo, Maldives, PNG, Solomon Islands, Suriname, Timor-Leste, Vanuatu); five small (historical) economies with failed episodes: Zanzibar, Somaliland, Somalia, Republic of (South) Vietnam, GDR; and three autocratic economies with no episodes: South Yemen, Gaza/Palestine, Eritrea.

Our 1950-2014 sample covers 62 'treated' countries which experienced episodes and regime change (n=3,724 observations — see for Table A-3 sample makeup), 43 autocratic countries which only experienced democratisation episodes (n=2,515; control group 2 — Table A-2), and 15 autocratic countries which never experienced episodes (n=646; control group 1 — Table A-1). Four democracies reverted to autocracy and subsequently had unsuccessful democratisation episodes (n=75 observations); 9 countries had episodes and regime change but no pre-episode data (n=399) — both sets of observations are excluded from the analysis. The balance to arrive at 161 countries in the full available sample (n=8,770) is made up by 28 countries which were democracies throughout the sample period, which are also excluded. In practice the minimum number of time series observations for inclusion in our analysis is n=21. This is in line with the practice in Giavazzi & Tabellini (2005), Persson & Tabellini (2006) and Papaioannou & Siourounis (2008). Of the 62 'treated' countries, 12 reverted to autocracy before the end of the sample period — in additional analysis available on request we confirmed that the growth experience of these 12 countries during democracy closely matches that of the 50 remaining countries which did not revert to autocracy.

Figure A-1 provides an overview of the distribution of episodes and regime changes in our sample. In the top panel the histogram in light blue highlights two peaks of democratisation

episodes in the late 1950s/early 1960s, and in the 1990s, coinciding with the second and third waves of democratisation (Huntington 1993). The lowest rate of ongoing democratisation episodes is in the mid-1960s and 1970s. The regime change events, in dark pink, clearly match these patterns for the second peak in the 1990s, but less so for the earlier period. The middle panel supports this notion of differential rates of episodes and their outcomes over time: the share of failed episodes (in teal) is particularly strong in the 1950s and early 1960s, and again in the 1990s. Episodes culminating in regime change (in dark pink) are only substantial in the late 1970s to early 1990s and are otherwise dominated by the former group.

The bottom panel in Figure A-1 charts the mean episode length over time and the evolution of each episode in our sample. It shows substantial variation in episode length over time as well as temporal clusters of episodes with and without regime change. The graphs for successful episodes are frequently very steep (implying short episodes), yet it would be misleading to claim that these trajectories *dominate* the treatment sample.

Our analysis includes some data for countries prior to their independence — the data coverage is very good so that sample selection is not a concern. Nevertheless, in a robustness check (available on request) we discarded pre-independence country-years and find the qualitative conclusion from our analysis, that accounting for democratisation episodes yields even higher economic growth in the long-run, is unchanged.

Table A-1: Sample Makeup: Control Group 1 (never experienced a democratisation episode)

Country	ISO	Total obs	Country	ISO	Total obs
United Arab Emirates	ARE	21	North Korea	PRK	35
Azerbaijan	AZE	21	Qatar	QAT	40
China	CHN	64	Saudi Arabia	SAU	64
Cuba	CUB	65	Tajikistan	TJK	21
Djibouti	DJI	64	Turkmenistan	TKM	21
Iran	IRN	64	Uzbekistan	UZB	21
Kazakhstan	KAZ	21	Viet Nam	VNM	60
Mozambique	MOZ	64			

Notes: This table provides details on the sample-makeup of the first control group sample, made up of the 15 countries which never experienced a democratisation episode (and of course also no regime change).

Table A-2: Sample Makeup: Control Group 2 (never democratised)

			Episodes (all failed)									Autocracy	
Country	ISO	Total obs	Years in ep	Share	Avg length	Count	1st	2nd	3rd	4th	5th	Years in auto	Share
Afghanistan	AFG	59	5	8%	5.0	1	2002					54	92%
Angola	AGO	39	4	10%	4.0	1	2008					35	90%
Burundi	BDI	55	17	31%	5.7	3	1982	1992	1999			38	69%
Bahrain	BHR	44	6	14%	3.0	2	1972	2000				38	86%
Central African Republic	CAF	64	21	33%	5.3	4	1956	1987	2005	2014		43	67%
Cameroon	CMR	52	4	8%	4.0	1	1990					48	92%
DR of Congo	COD	64	18	28%	9.0	2	1955	1998				46	72%
Congo	COG	64	11	17%	3.7	3	1957	1990	2002			53	83%
Algeria	DZA	44	6	14%	2.0	3	1977	1990	1995			38	86%
Egypt	EGY	64	10	16%	10.0	1	1956					54	84%
Ethiopia	ETH	64	6	9%	6.0	1	1987					58	91%
Gabon	GAB	64	13	20%	6.5	2	1957	1987				51	80%
Guinea	GIN	64	24	38%	8.0	3	1957	1985	2010			40	63%
Gambia	GMB	64	13	20%	3.3	4	1960	1966	1996	2014		51	80%
Guinea-Bissau	GNB	64	21	33%	5.3	4	1973	1990	2005	2014		43	67%
Equatorial Guinea	GNQ	55	15	27%	7.5	2	1968	1982				40	73%
China, Hong Kong	HKG	64	8	13%	8.0	1	1985					56	88%
Haiti	HTI	65	12	18%	2.4	5	1951	1987	1991	1993	2006	53	82%
Iraq	IRQ	64	8	13%	8.0	1	2004					56	88%
Jordan	JOR	64	6	9%	6.0	1	1989					58	91%
Kenya	KEN	64	29	45%	9.7	3	1956	1990	2010			35	55%
Kyrgyzstan	KGZ	23	11	48%	11.0	1	2003					12	52%
Cambodia	KHM	60	11	18%	11.0	1	1990					49	82%
Kuwait	KWT	40	16	40%	8.0	2	1981	1991				24	60%
Lao PDR	LAO	60	4	7%	4.0	1	1955					56	93%
Lebanon	LBN	64	15	23%	15.0	1	1996					49	77%
Libya	LBY	62	3	5%	3.0	1	2011					59	95%
Morocco	MAR	64	15	23%	7.5	2	1963	1993				49	77%
Myanmar	MMR	64	8	13%	8.0	1	2010					56	88%
Mauritania	MRT	55	10	18%	3.3	3	1987	2007	2010			45	82%
Malaysia	MYS	65	27	42%	13.5	2	1972	1999				38	58%
Oman	OMN	57	4	7%	4.0	1	2000					53	93%
Pakistan	PAK	64	32	50%	10.7	3	1962	1985	2002			32	50%
Rwanda	RWA	55	21	38%	7.0	3	1979	1991	2003			34	62%
Sudan	SDN	64	23	36%	7.7	3	1965	1986	1996			41	64%
Singapore	SGP	55	1	2%	1.0	1	1960					54	98%
Swaziland	SWZ	55	6	11%	6.0	1	1964					49	89%
Seychelles	SYC	55	29	53%	9.7	3	1963	1979	1991			26	47%
Syrian Arab Rep.	SYR	64	5	8%	2.5	2	1953	1961				59	92%
Chad	TCD	64	8	13%	8.0	1	1990					56	88%
Uganda	UGA	64	16	25%	5.3	3	1953	1981	1989			48	75%
Yemen	YEM	52	6	12%	6.0	1	1988					46	88%
Zimbabwe	ZWE	64	3	5%	3.0	1	1979					61	95%

Notes: This table provides details on the sample-makeup of the second control group sample, made up of the 43 countries which experienced at least one democratisation episode but never realised democratic regime change.

Table A-3: Sample Makeup: Treated Countries

			Episodes (successful or failed)											Regime change to democracy							Autocracy	
Country	ISO	Total obs	Years in ep	Share	Avg length	Count	1st	2nd	3rd	4th	5th	Count Failed	Avg length Failed	Years in dem	Share	Count	1st	Ep Length	2nd	Ep Length	Years in auto	Share
Albania	ALB	60	10	17%	3.3	3	1991	1998	2005			2	5.0	10	17%	1	2005	0			40	67%
Argentina	ARG	65	10	15%	2.5	4	1957	1963	1972	1983		2	4.0	33	51%	2	1964	1	1984	1	22	34%
Armenia	ARM†	21	8	38%	4.0	2	1998	2010				2	4.0	2	10%	0					11	52%
Benin	BEN	64	15	23%	7.5	2	1952	1990				1	13.0	23	36%	1	1992	2			26	41%
Burkina Faso	BFA	55	14	25%	4.7	3	1960	1978	1990			2	2.0	15	27%	1	2000	10			26	47%
Bangladesh	BGD	42	17	40%	4.3	4	1973	1977	1984	2009		3	3.0	10	24%	1	1992	8			15	36%
Bulgaria	BGR	64	1	2%	1.0	1	1990					0		24	38%	1	1991	1			39	61%
Bosnia & Herzeg.	BIH[a]	19	1	5%	1.0	1	1996					0		18	95%	1	1997	1			0	0%
Belarus	BLR†	21	0	0%		0						0		3	14%	0					18	86%
Bolivia	BOL	65	13	20%	6.5	2	1952	1983				1	11.0	30	46%	1	1985	2			22	34%
Brazil	BRA	65	12	18%	12.0	1	1975					0		28	43%	1	1987	12			25	38%
Barbados	BRB[b]	64	9	14%	9.0	1	1951					0		55	86%	1	1960	9			0	0%
Botswana	BWA[c]	55	7	13%	7.0	1	1960					0		48	87%	1	1967	7			0	0%
Chile	CHL	65	3	5%	1.5	2	1958	1988				0		39	60%	2	1959	1	1990	2	23	35%
Côte d'Ivoire	CIV	64	17	27%	4.3	4	1990	1995	2001			3	3.3	2	3%	1	2008	7			45	70%
Colombia	COL	65	25	38%	8.3	3	1958	1972	1982			2	8.0	24	37%	1	1991	9			16	25%
Comoros	COM	55	7	13%	2.3	3	1990	1997	2002			2	1.5	9	16%	1	2006	4			39	71%
Cabo Verde	CPV	57	7	12%	2.3	3	1972	1980	1990			2	3.0	24	42%	1	1991	1			26	46%
Cyprus	CYP	64	2	3%	2.0	1	1960					0		45	70%	1	1960	0			17	27%
Dominican Rep.	DOM	64	13	20%	3.3	4	1961	1966	1978	1995		2	4.0	27	42%	2	1982	4	1996	1	24	38%
Ecuador	ECU	65	9	14%	3.0	3	1950	1967	1978			2	3.5	35	54%	1	1980	2			21	32%
Spain	ESP	65	2	3%	2.0	1	1976					0		37	57%	1	1978	2			26	40%
Georgia	GEO[b]	21	10	48%	10.0	1	1994					0		11	52%	1	2004	10			0	0%
Ghana	GHA	64	7	11%	1.8	4	1951	1969	1979	1993		3	2.0	21	33%	1	1994	1			36	56%
Greece	GRC	65	5	8%	2.5	2	1950	1974				1	4.0	40	62%	1	1975	1			20	31%
Guatemala	GTM	65	16	25%	16.0	1	1984					0		15	23%	1	2000	16			34	52%
Honduras	HND	65	13	20%	4.3	3	1950	1971	1980			2	1.5	18	28%	1	1990	10			34	52%
Croatia	HRV[d]	22	7	32%	7.0	1	1993					0		15	68%	1	2000	7			0	0%
Hungary	HUN	65	2	3%	2.0	1	1988					0		25	38%	1	1990	2			38	58%
Indonesia	IDN	65	10	15%	5.0	2	1950	1997				1	7.0	15	23%	1	2000	3			40	62%

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Table A-3: Sample Makeup: Treated Countries (continued)

			Episodes (successful or failed)											Regime change to democracy							Autocracy	
Country	ISO	Total obs	Years in ep	Share	Avg length	Count	1st	2nd	3rd	4th	5th	Count Failed	Avg length Failed	Years in dem	Share	Count	1st	Ep Length	2nd	Ep Length	Years in auto	Share
India	IND[e]	65	2	3%	2.0	1	1950					0		63	97%	1	1952	2			0	0%
Jamaica	JAM	64	2	3%	2.0	1	1953					0		56	88%	1	1955	2			6	9%
Japan	JPN[e]	65	2	3%	2.0	1	1950					0		63	97%	1	1952	2			0	0%
South Korea	KOR	64	13	20%	6.5	2	1964	1976				1	1.0	27	42%	1	1988	12			24	38%
Liberia	LBR	64	7	11%	2.3	3	1985	1997	2005			2	3.0	9	14%	1	2006	1			48	75%
Sri Lanka	LKA	65	8	12%	4.0	2	1983	2011				1	4.0	51	78%	1	1987	4			6	9%
Lesotho	LSO	55	12	22%	4.0	3	1960	1992	2002			2	5.5	12	22%	1	2003	1			31	56%
Moldova	MDA	23	4	17%	4.0	1	2006					0		16	70%	1	2010	4			3	13%
Madagascar	MDG	64	20	31%	5.0	4	1956	1985	2003	2013		2	4.0	10	16%	2	1994	9	2006	3	34	53%
Mexico	MEX	65	18	28%	18.0	1	1977					0		20	31%	1	1995	18			27	42%
Macedonia	MKD	22	6	27%	6.0	1	1993					0		14	64%	1	1999	6			2	9%
Mali	MLI	55	3	5%	1.5	2	1960	1992				1	2.0	20	36%	1	1993	1			32	58%
Malta	MLT	57	1	2%	1.0	1	1962					0		52	91%	1	1963	1			4	7%
Montenegro	MNE†	10	0	0%		0						0		3	30%	0					7	70%
Mongolia	MNG[a]	24	1	4%	1.0	1	1990					0		23	96%	1	1992	2			0	0%
Mauritius	MUS	64	3	5%	1.5	2	1959	1968				1	3.0	47	73%	1	1968	0			14	22%
Malawi	MWI	58	13	22%	6.5	2	1992	2005				1	9.0	6	10%	1	2009	4			39	67%
Namibia	NAM	35	3	9%	1.5	2	1989	1995				1	3.0	20	57%	1	1995	0			12	34%
Niger	NER	64	12	19%	3.0	4	1957	1988	1993	1997		3	3.7	15	23%	1	1994	1			37	58%
Nigeria	NGA	64	11	17%	3.7	3	1976	1998	2010			2	4.0	2	3%	1	2013	3			51	80%
Nicaragua	NIC	65	10	15%	10.0	1	1980					0		17	26%	1	1990	10			38	58%
Nepal	NPL	52	5	10%	2.5	2	1990	2006				1	3.0	5	10%	1	2008	2			42	81%
Panama	PAN	65	6	9%	2.0	3	1950	1953	1990			2	2.5	24	37%	1	1991	1			35	54%
Peru	PER	65	17	26%	4.3	4	1950	1964	1976	1994		3	4.0	25	38%	1	1981	5			23	35%
Philippines	PHL	65	9	14%	4.5	2	1982	2007				0		21	32%	2	1988	6	2010	3	35	54%
Poland	POL	64	10	16%	10.0	1	1980					0		25	39%	1	1990	10			29	45%
Portugal	PRT	65	6	9%	6.0	1	1970					0		39	60%	1	1976	6			20	31%
Paraguay	PRY	65	4	6%	4.0	1	1990					0		21	32%	1	1994	4			40	62%
Romania	ROU	60	1	2%	1.0	1	1990					0		24	40%	1	1991	1			35	58%
Russia	RUS†	23	0	0%		0						0		2	9%	0					21	91%

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Table A-3: Sample Makeup: Treated Countries (continued)

			Episodes (successful or failed)											Regime change to democracy							Autocracy	
Country	ISO	Total obs	Years in ep	Share	Avg length	Count	1st	2nd	3rd	4th	5th	Count Failed	Avg length Failed	Years in dem	Share	Count	1st	Ep Length	2nd	Ep Length	Years in auto	Share
Senegal	SEN	64	7	11%	2.3	3	1960	1978	1990			1	7.0	25	39%	2	1960	0	1990	0	32	50%
Sierra Leone	SLE	64	13	20%	3.3	4	1958	1994	2002	2013		3	4.0	9	14%	1	2003	1			42	66%
El Salvador	SLV	65	12	18%	6.0	2	1982	1991				1	4.0	16	25%	1	1999	8			37	57%
São Tomé & Principe	STP	55	9	16%	4.5	2	1972	1987				1	4.0	23	42%	1	1992	5			23	42%
Togo	TGO	64	21	33%	5.3	4	1956	1991	2005	2012		3	6.3	1	2%	1	2014	2			42	66%
Thailand	THA	64	22	34%	4.4	5	1974	1978	1992	2008	2010	4	4.0	8	13%	1	1998	6			34	53%
Trinidad & Tobago	TTO[f]	64	9	14%	9.0	1	1951					0		55	86%	1	1960	9			0	0%
Tunisia	TUN	64	6	9%	3.0	2	1956	2011				1	5.0	3	5%	1	2012	1			55	86%
Turkey	TUR	65	13	20%	4.3	3	1950	1962	1983			1	2.0	38	58%	2	1966	4	1990	7	14	22%
Tanzania	TZA	64	17	27%	8.5	2	1958	1986				1	7.0	13	20%	1	1996	10			34	53%
Ukraine	UKR	21	1	5%	1.0	1	2005					0		10	48%	1	2006	1			10	48%
Uruguay	URY	65	4	6%	4.0	1	1981					0		53	82%	1	1985	4			8	12%
Venezuela	VEN	65	5	8%	5.0	1	1958					0		40	62%	1	1963	5			20	31%
South Africa	ZAF	64	5	8%	5.0	1	1990					0		20	31%	1	1995	5			39	61%
Zambia	ZMB	58	8	14%	2.7	3	1961	1990	2000			1	8.0	14	24%	2	1961	0	2000	0	36	62%

Notes: This table provides details on the sample-makeup of the 'treated' sample, i.e. the set of countries which experienced at least one democratisation episode followed by a regime change. There are 75 countries in this table, but only 62 of them have estimates for both the episode dummy and the democratic regime change dummy — these countries have their country names highlighted in bold. The remaining 13 countries have the following characteristics: four countries (marked in the 'ISO' column with †) experienced a reversal to autocracy, in one case followed by an unsuccessful democratisation episode. Nine countries (marked in the 'ISO' column with superscripts a-f) do not have any pre-episode data (and in some cases additionally experienced episodes lasting only one or two years), hence the regime change or the episode dummy is unidentified. In some more detail:

a) No pre-episode data, one-year episode before democratic regime change (BIH,MNG)

b) No pre-episode data, ten-year episode before democratic regime change, no regime change estimate (BRB, GEO)

c) No pre-episode data, seven-year episode before democratic regime change, no episode estimate (BWA)

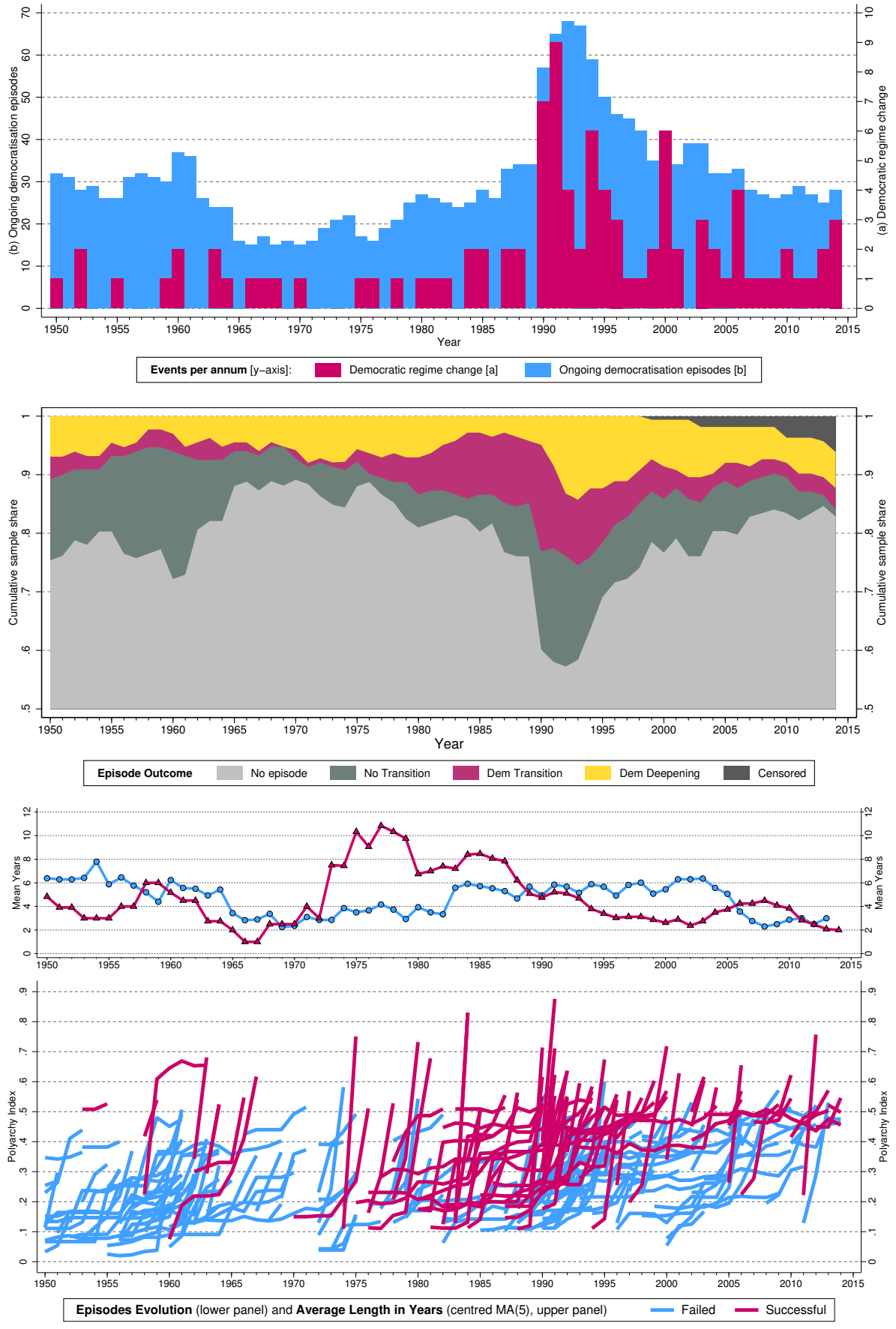
d) No pre-episode data, seven-year episode before democratic regime change, no regime change estimate (HRV)

e) No pre-episode data, two-year episode before democratic regime change, no regime change estimate (IND, JPN)

f) No pre-episode data, nine-year episode before democratic regime change, no regime change estimate (TTO)

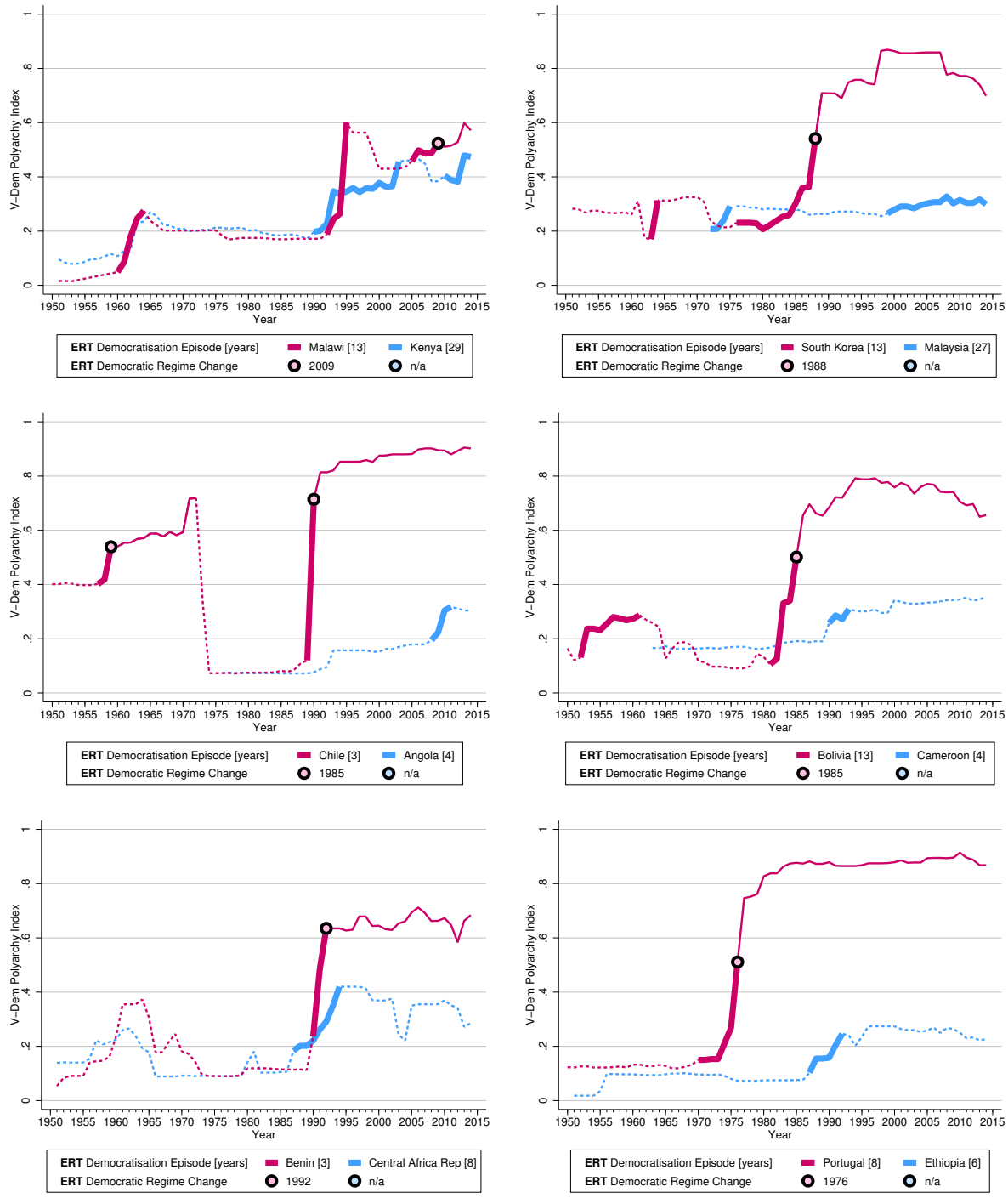
The first set of columns after the country name, ISO code and total observation count refers to information on the total number of episodes, their average length, and timing as well as the count and average length for failed episodes. The next set of columns refers to successful regime changes, how long countries spent in democracy (the 'years of treatment') and the length of the associated democratisation episodes (in years). The final two columns report the information on the pre-episode data. For years in episodes, democracy, and autocracy we report the share of total years, which adds up to 100% (even though of course episodes are *nominally* within the autocratic regime but we separate them out here).

Figure A-1: Episodes and Regime Change (1950-2014)



Notes: We present the distribution of democratisation episodes and regime changes in the top panel, the share of episode type in the middle panel, and the individual evolution of each episode in the lower plot along with the smoothed annual mean episode length (computed for episode start years) in the bottom panel.

Figure A-2: More Examples of Episodes and Democratic Regime Change



Notes: We present the V-Dem polyarchy index evolution for country pairs, where the country in dark pink experienced regime change and the country in light blue 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 pink 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'.

B Additional Figures and Tables

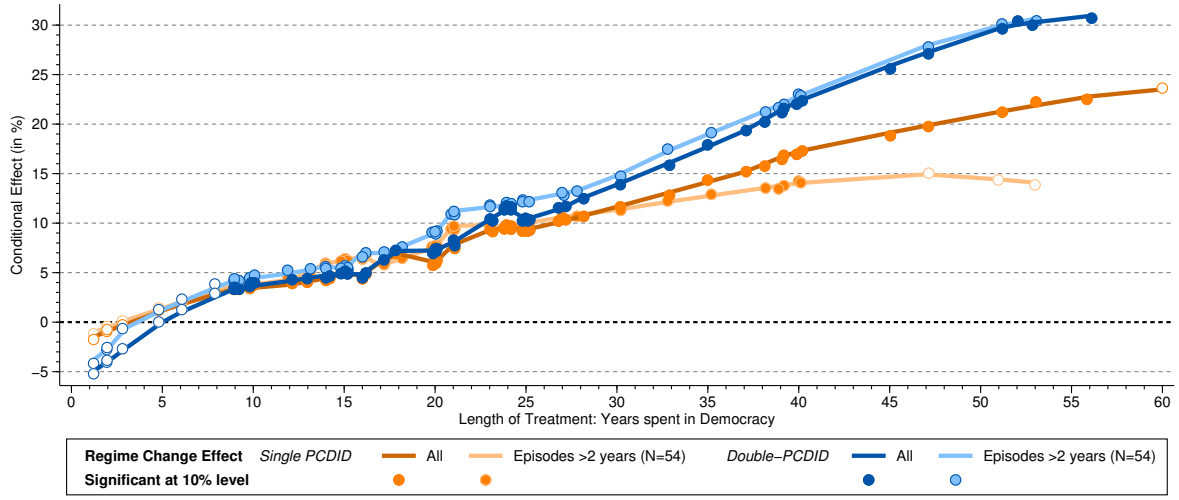
Table B-1: ATET Estimates: Single and Double PCDID

Panel (a) Double PCDID Results	(1)	(2)	(3)	(4)	(5)	(6)
Factors included	1×2	2×2	3×2	4×2	5×2	6×2
Democratic Episode	-2.832	-1.170	0.959	0.351	0.548	-0.310
	[2.582]	[2.003]	[2.077]	[2.136]	[1.977]	[1.992]
Democratic Regime Change	3.157	5.497*	10.165***	6.845**	6.645**	6.785**
	[3.952]	[3.341]	[3.927]	[3.321]	[3.359]	[3.311]
Export/Trade Ratio	-0.212	-0.213	-0.172	-0.224**	-0.166**	-0.180**
(in percent)	[0.147]	[0.134]	[0.116]	[0.091]	[0.084]	[0.088]
Population Growth Rate	-5.000**	-7.775***	-7.540***	-6.206***	-7.054***	-7.844***
(in percent)	[1.991]	[2.100]	[1.883]	[1.593]	[1.694]	[1.994]
Treated Countries	62	62	62	62	62	62
Observations	3660	3660	3660	3660	3660	3660
Control Countries 1	15	15	15	15	15	15
Observations	631	631	631	631	631	631
Control Countries 2	43	43	43	43	43	43
Observations	2472	2472	2472	2472	2472	2472

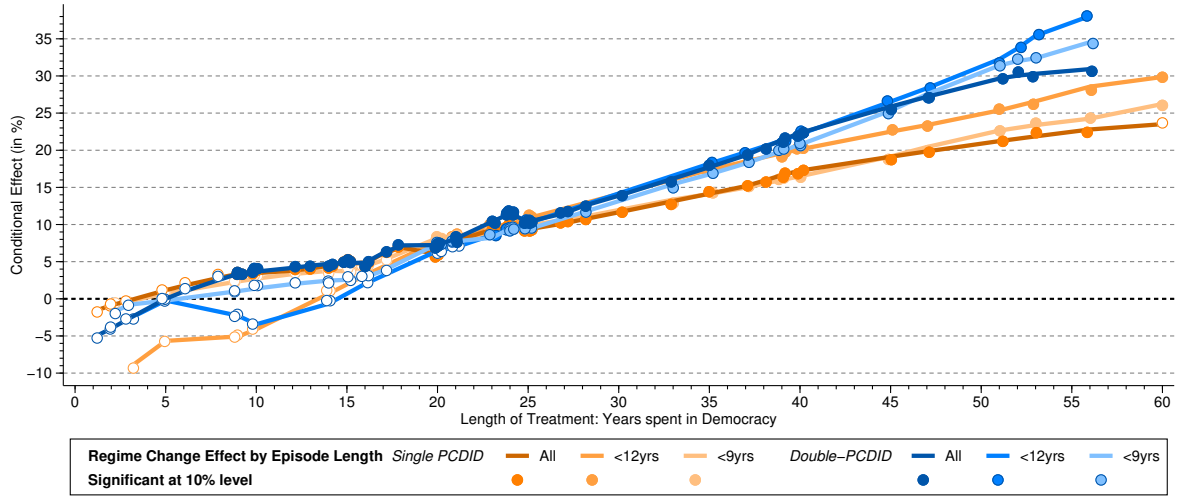
Panel (b) Single PCDID Results	(1)	(2)	(3)	(4)	(5)	(6)
Factors included	1	2	3	4	5	6
Democratic Regime Change	5.914*	3.877	8.601***	6.247***	6.710**	7.738***
(ERT definition)	[3.595]	[3.455]	[2.816]	[2.286]	[2.744]	[2.769]
Export/Trade Ratio	-0.304*	-0.369**	-0.363**	-0.179	-0.076	-0.092
(in percent)	[0.175]	[0.150]	[0.157]	[0.121]	[0.107]	[0.101]
Population Growth Rate	-6.721**	-6.709***	-7.059***	-5.564***	-6.445***	-6.200***
(in percent)	[2.891]	[2.584]	[2.661]	[2.025]	[2.083]	[2.090]
Treated Countries	62	62	62	62	62	62
Observations	3724	3724	3724	3724	3724	3724
Control Countries	58	58	58	58	58	58
Observations	3161	3161	3161	3161	3161	3161

Notes: The table presents the Mean Group estimates from the Double and Single PCDID treatment regressions in Panels (a) and (b), respectively. The regime change effects can be interpreted as ATET. There are six different models for augmentation with 1 to 6 common factors — for the Double PCDID in Panel (a) there are separate factors from each of the two control samples, hence the number of factors is double that included in the Single PCDID models in Panel (b). Statistical significance is indicated using * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B-1: Episodes and their Implications for the Regime Change Effect



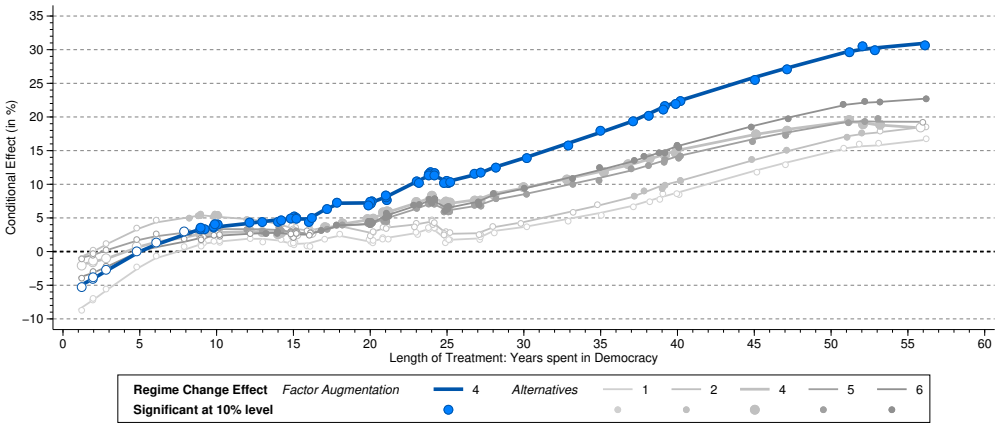
(a) Robustness: Exclude 'short' episodes ($\leq$2yrs)



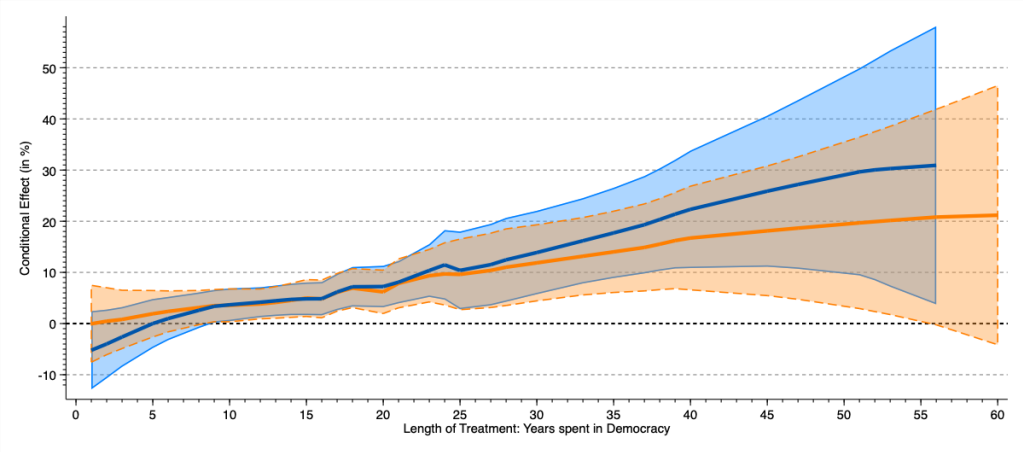
(b) Robustness: Exclude 'longer' episodes ($>$9 or $>$12yrs)

Notes: These plots present the results from running line regressions of country-specific coefficients on the democracy (ERT) dummy, derived from Single and Double PCDID estimates. In Panel (a) we compare Single (orange lines) and Double PCDID results (blue lines) for ERT in the full sample with those where countries with just one or two years spent in episodes are dropped. In Panel (b) we distinguish countries which had episodes lasting up to 9 years ($N = 33$) or up to 12 years ($N = 43$), respectively the median and 70th percentile, and find qualitatively no difference to the full sample ($N = 62$) results.

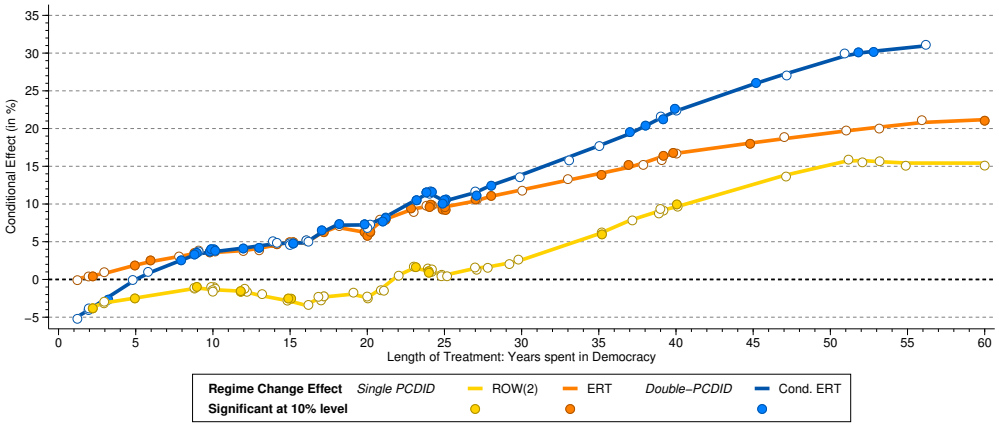
Figure B-2: Alternative Factor Augmentations, Confidence Intervals and Bootstrapped CI



(a) Alternative Factor Augmentations



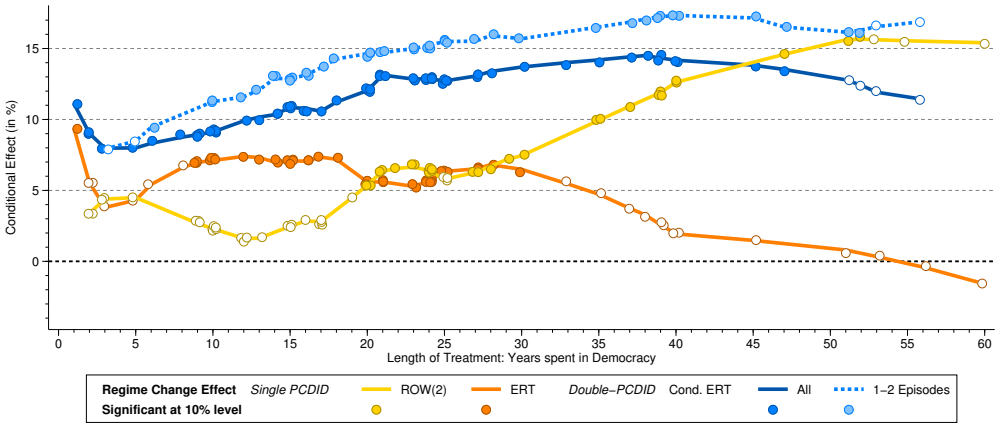
(b) Comparison Single and Double PCDID (with 90% CI)



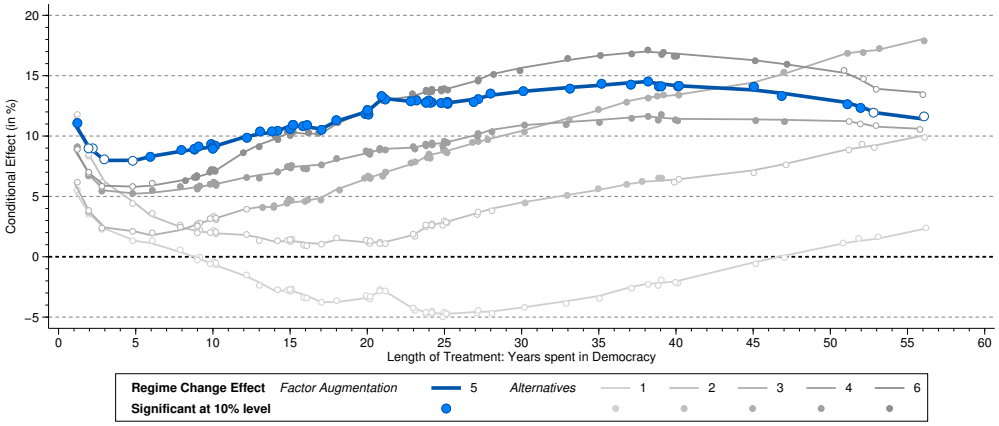
(c) Statistical Significance Based on Bootstrap

Notes: These plots presents the results from running line regressions of country-specific coefficients on the democracy (ERT) dummy, derived from Single and Double PCDID estimates. In Panel (a) we present the conditional ERT results from Double PCDID models augmented with 1 to 6 factors from each of the respective control groups: the blue line is for the model augmented with 3 estimated factors (from each respective control sample), grey lines present alternative augmentations using one to six factors (dto.). In Panel (b) we report the full sample results for ERT (Single and Double PCDID) but plot the 90% confidence intervals for each running line regression. In Panel (c) we signal statistically significant difference from zero in the running line regression adopting the bootstrap 90% confidence interval (250 replications). A hollow (filled) marker indicates that the bootstrapped 90% confidence interval does (not) include zero.

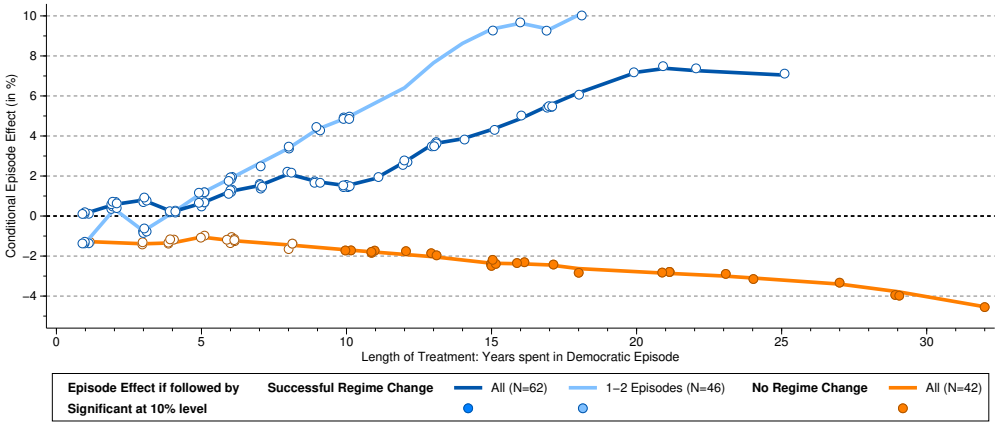
Figure B-3: Single and Double PCDID — Excluding Covariates



(a) Comparison of Single and Double PCDID Results



(b) Alternative Factor Augmentations: Conditional ERT



(c) Episodes effect in the Double PCDID

Notes: The results presented in this figure do not include any controls in the PCDID regressions. We present results from running line regressions of country-specific democracy coefficients on years spent in democracy or years spent in episodes, respectively. Additional controls in these running line regressions are the same as those in the analysis in the main text. The full sample matches that of the Double PCDID estimates for ERT (62 treated countries unless indicated), and with the exception of Panel (b) all results are for PCDID models augmented with 5 common factors for each control group — this is the preferred model on the basis of Chan & Kwok (2022) Alpha tests. Panel (a) presents the results for Single PCDID alongside those for Double PCDID estimates — for the latter we contrast results for all countries with those which experienced only 1 or 2 liberalisation episodes (dark and light blue, respectively). Panel (b) presents results for the Double PCDID for 1-6 factors per control sample. Panel (c) focuses on the Episode effect, distinguishing countries which eventually experienced regime change (in blue) from those which did not (in orange).

C Event Analysis

In this section we study the potential for idiosyncratic events, such as natural resource discoveries, natural disasters, or financial crises, exerting undue influence/bias on our PCDID estimates. Adopting dummies for each of the aforementioned events we employ event analysis to investigate the evolution of GDP per capita growth and the change in the V-Dem Polyarchy measure (the index underlying our episode and regime change data) up to five years before and after the event/crisis: we estimate country fixed effects models separately for each variable k (growth, polyarchy) and event type:

$$y_{it}^{k} = \alpha_{i}^{k} + \sum_{s=-5}^{5} \beta_{\tau+s}^{k} \delta_{i,\tau+s} + \varepsilon_{it}^{k}, \tag{8}$$

where $\delta_{i,\tau+s}$ is a dummy equal to one if country i is s years away from the event at time τ, t indexes the years between 1950 and 2014, α_i is the country fixed effect and ε is the error term. s varies from -5 to $+5$, such that we evaluate each variable in the lead-up and aftermath of the event *relative to* the observations outside this 11-year window, with the latter interpreted as 'normal' times. Importantly, we compare the sample of countries which experienced regime change with the sample which experienced liberalisation episodes but no regime change, presenting results separately. Finally, we do not study crises/events at just any point in time, but focus on those which occur during democratisation episodes: if individual liberalising countries get bumped into or are prevented from realising democracy by a natural resource find, a financial crisis or a natural disaster, then this amounts to the type of idiosyncratic shock which threatens our identification strategy. The number of events in treated and control groups during episodes are tabulated in Table C-1 below. Since the event analysis includes a country fixed effects only countries which experienced a crisis/shock during a democratisation episode are included in the sample.

Although there are ample reasons for spillovers across countries for each event type, our primary reason for selecting these economic events/crises is that they are typically regarded as *country-specific* events, with the respective literatures (at least for the economic crises) seeking to explain their prevalence largely with country-specific determinants.

We adopt data on new oil discoveries from Cotet & Tsui (2013): we define a boom as the point in time when either (i) the 3-year moving average of the growth rate of new oil discoveries (in billion barrels) is at least 100% and the magnitude of the discovery is at least half a billion barrels; or (ii) when the 3-year moving average of the growth rate of new oil discoveries (in billion barrels *per capita*) is at least 100% and the magnitude of the discovery is at least half a million barrels *per 1,000 population*.

Table C-1: Sample Makeup: Event Analysis samples

	Oil boom	Banking Crisis	Currency Crisis	Natural Disaster	Full Sample
Treated N	16	18	27	10	62
observations	947	1104	1674	551	3660
Control N	19	19	18	10	43
observations	1080	602	1085	606	2472

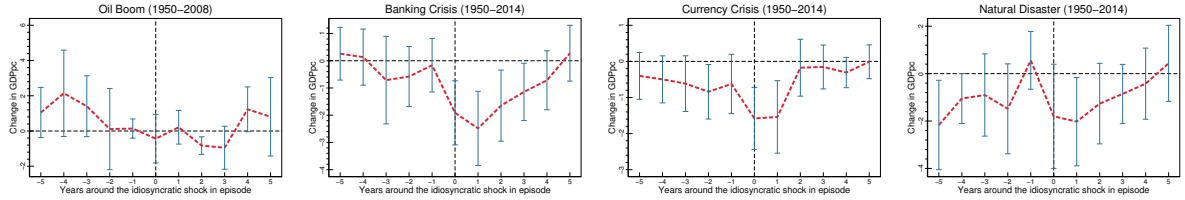
Notes: This table provides details on the crisis/event count in the treated and control groups for the episode-regime change event analysis. The full treated (control) sample (analysed in Figure 2 of the maintext) contains 62 (43) countries and 3,660 (2,472) observations.

For financial crises we augment the data collated by Carmen Reinhart — the expanded Reinhart & Rogoff (2009, RR) database — with information from Laeven & Valencia (2020, LL) — additional search established no further crises in the 1950s and 1960s (LL only starts in 1970). In all cases we mark the crisis start year; for banking crises we do not exclude 'ongoing crisis years' from the event analysis sample, in line with existing practice in the literature.

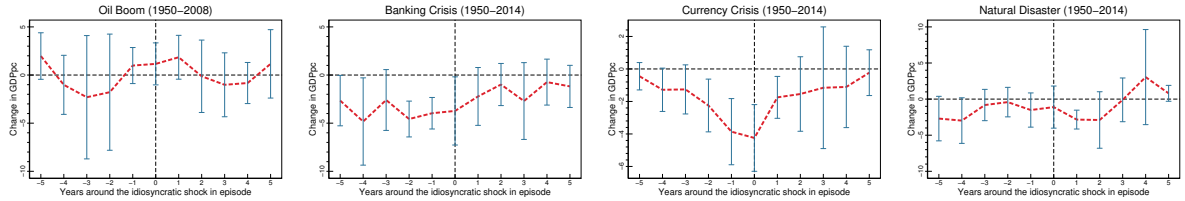
For natural disasters we use the EM-DAT database: EM-DAT, CRED/UCLouvain, Brussels, Belgium – www.emdat.be which covers primarily 'natural' disasters like earth quakes, floods or epidemics, but also large-scale industrial accidents and air/rail/road disasters. We construct a dummy for large-scale disasters by combining the EM-DAT information on associated deaths with Maddison (Bolt & van Zanden 2020) population data and select events with a death rate of 1 in 10,000 population.

The event analysis plots for per capita GDP growth and the annual change in polyarchy are presented in Figure C-1. Timings differ at times minimally, but the patterns of sign and statistical significance of the effects on growth and change in polyarchy between the treated and control samples are in general closely matched.

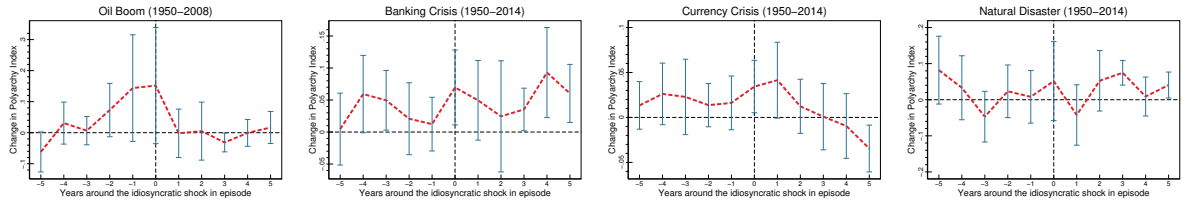
Figure C-1: Idiosyncratic Shocks in their Effect on Growth (a,b) and Polyarchy (c,d)



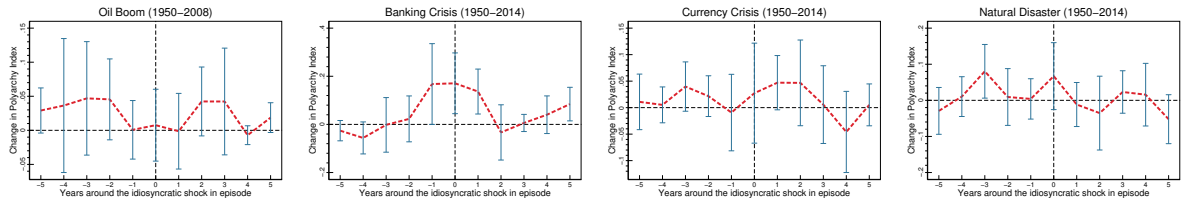
(a) Effect of Shock on *per capita Growth*: Treated Sample



(b) Effect of Shock on *per capita Growth*: Control Sample



(c) Effect of Shock on *Polyarchy*: Treated Sample



(d) Effect of Shock on *Polyarchy*: Control Sample

Notes: We present the results from event analyses for the GDP pc growth or polyarchy variables and the event as indicated. Event plots for growth are presented in panels (a) and (b), those for polyarchy in panels (c) and (d). In each case the first panel is for the treated sample, the second for the control sample. All of these are within-country estimates with standard errors clustered at the country-level. The vertical bars are the 90% confidence intervals.

D Alpha Test of the Weak Parallel Trend Assumption

We carry out tests for the weak parallel trend assumption in the Double PCDID models. The Alpha test is introduced in Chan and Kwok (2022), section 4.4, and works with the residuals from the auxiliary regression in the control sample. In the standard PCDID we estimate the treatment sample regression with factors estimated from $\hat{e}_{it}$ via PCA. In the Alpha Test, we compute the cross-section average of the $\hat{e}_{it}$, say $\bar{e}_t$ and enter this term in the PCDID regression *instead of* the estimated factors: $y_{it} = b_{0i} + d_i \mathbf{1}_{\{t>T_{0i}\}} + a_i' \bar{e}_t + b_{1i}' x_{it} + u_{it}$. We adjust this test to our new empirical setup with two control samples and estimate instead

$$y_{it} = b_{0i} + d_i^A \mathbf{1}^A_{\{t>T_0\}} + d_i^B \mathbf{1}^B_{\{t>T_1>T_0\}} + a_{1i}^{\mathsf{A}} \bar{e}_t^{\mathsf{A}} + a_{2i}^{\mathsf{AB}} \bar{e}_t^{\mathsf{AB}} + b_{1i}' x_{it} + e_{it},$$

where $\bar{e}_t^{\mathsf{A}}$ and $\bar{e}_t^{\mathsf{AB}}$ are the cross-section averages of the residuals from the auxiliary regressions in the control samples (a) for countries which never experienced an episode and (b) for countries which experienced episodes but not regime change, respectively. The null hypothesis is that the respective Mean Group estimates of a_{1i}^{A} (for episodes) and a_{2i}^{AB} (for regime change) are equal to 1, which would constitute 'weak parallel trends'. Considering these hypotheses jointly (Chow test) acts as a test for our Double PCDID. Results suggest that this assumption is satisfied for models up to three factors in the full treated country sample and for all models in the sample of treated countries with only 1 or 2 episodes.

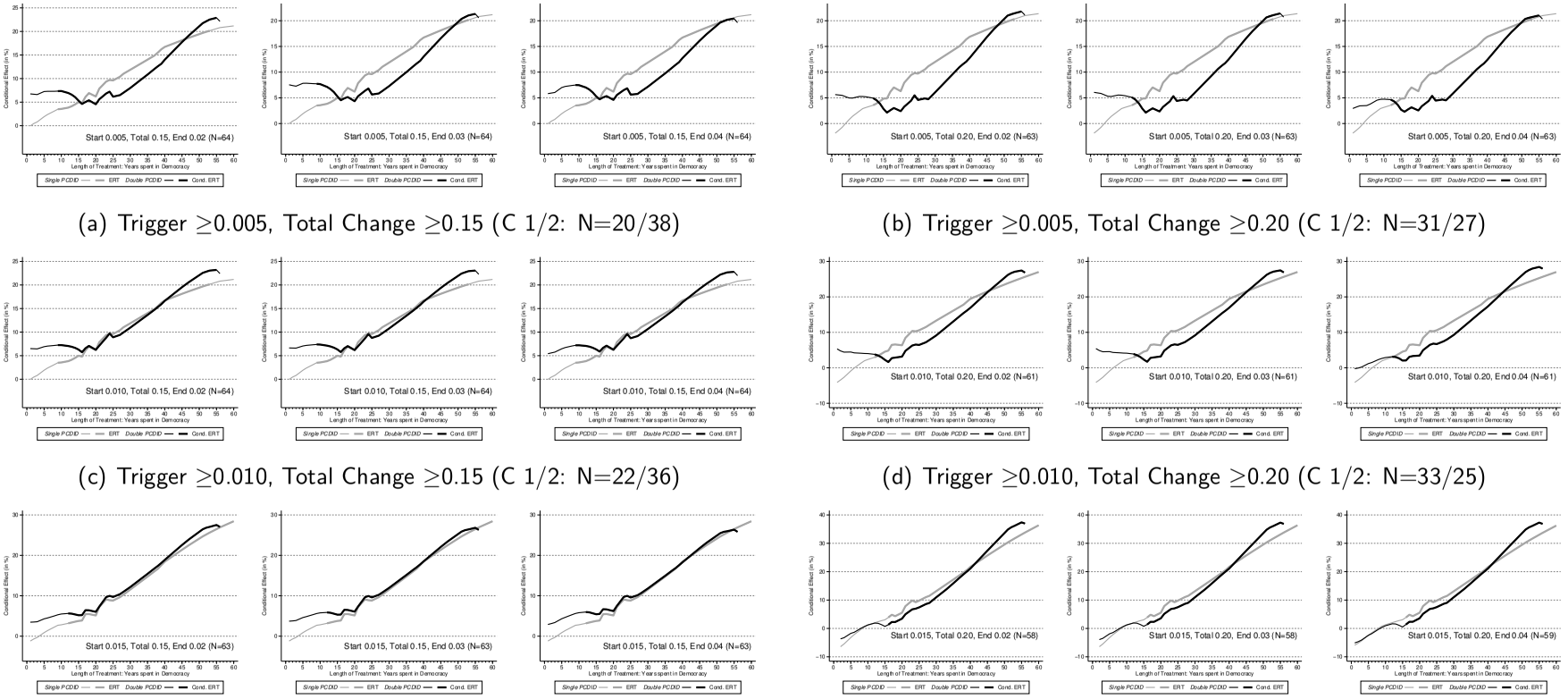
Table D-1: Alpha test for the weak parallel trends assumption

	(A) All Countries			**(B) 1 or 2 episodes**		
Factors	Episode	Regime	Joint	Episode	Regime	Joint
1	0.07	0.41	0.18	0.65	0.58	0.53
2	0.77	0.53	0.43	0.44	0.09	0.19
3	0.61	0.45	0.24	0.54	0.07	0.12
4	0.01	0.10	0.03	0.20	0.78	0.37
5	0.01	0.21	0.02	0.19	0.87	0.25
6	0.00	0.00	0.00	0.07	0.25	0.19

Notes: We report the p values for the Alpha test for weak parallel trends. Panel (A) uses the full treated sample (N=62), Panel (B) the reduced sample for countries which experienced only one or two democratisation episodes (N=46). Factor augmentation for $m = 1, \ldots, 6$ is meant to imply 'm' factors constructed from the episode control sample regressions and an additional 'm' from the regime change control sample regressions.

E Alternative Definitions for Episodes

Figure E-1: Alternative Episode Definition



(a) Trigger ≥0.005, Total Change ≥0.15 (C 1/2: N=20/38)

(b) Trigger ≥0.005, Total Change ≥0.20 (C 1/2: N=31/27)

(c) Trigger ≥0.010, Total Change ≥0.15 (C 1/2: N=22/36)

(d) Trigger ≥0.010, Total Change ≥0.20 (C 1/2: N=33/25)

(e) Trigger ≥0.015, Total Change ≥0.15 (C 1/2: N=22/36)

(f) Trigger ≥0.015, Total Change ≥0.20 (C 1/2: N=35/23)

Notes: We present predictions from running line regression results for Single and Double PCDID estimates using different definitions for episodes. Each graph indicates the number of treated countries, each panel header (a) to (f) the number of countries in the two control samples ('C' 1/2). 'Trigger' is for minimal annual change to start an episode (0.02-0.04), 'Total Change' is 0.15 or 0.20. Each panel has three plots for annual change to 'terminate' an episode: 0.02, 0.03, and 0.04 (from left to right). The ERT default parameters were developed by the data authors to best capture actual episodes of political change. Alternative episode definitions consequently deviate from this first best with implications for the validity of the democracy-growth nexus represented in these figures.

F Estimated Evolution of Income Effects

In our analysis in the main text we link each country's regime change estimate (from a Single or Double PCDID regression) to years spent in democracy, adopting running line regressions. This 'ex-post' approach makes no allowances for the *evolution* of the democracy effect over years in treatment *in the estimation equation*. Since PCDID employs country-regressions, we cannot include a dummy for each year in treatment $k = t - T_{1i}$, since this would amount to 56 *additional* regressors (the average country only has 60 observations). Instead, below we include dummies for years $k = 2, \ldots, 15$ ($k = 1$ is set to 0) alongside the regime change dummy to capture the immediate post-regime change effects, while at the same time conditioning on these early years in the estimation of the 'long-term' ($k > 15$) effect:

$$y_{it} = b_{0i} + d_i^A \mathbf{1}^A_{\{t>T_{0i}\}} + d_i^B \mathbf{1}^B_{\{t>T_{1i}>T_{0i}\}} + \sum_{k=2}^{15} d_{ik}^{B'} \mathbf{1}^{B'}_{\{k=t-T_{1i}\}} + a_{1i}^{\mathsf{A}\prime} \hat{f}_t^{\mathsf{A}} + a_{2i}^{\mathsf{AB}\prime} \hat{f}_t^{\mathsf{AB}} + b_{1i}' x_{it} + e_{it}, \quad (9)$$

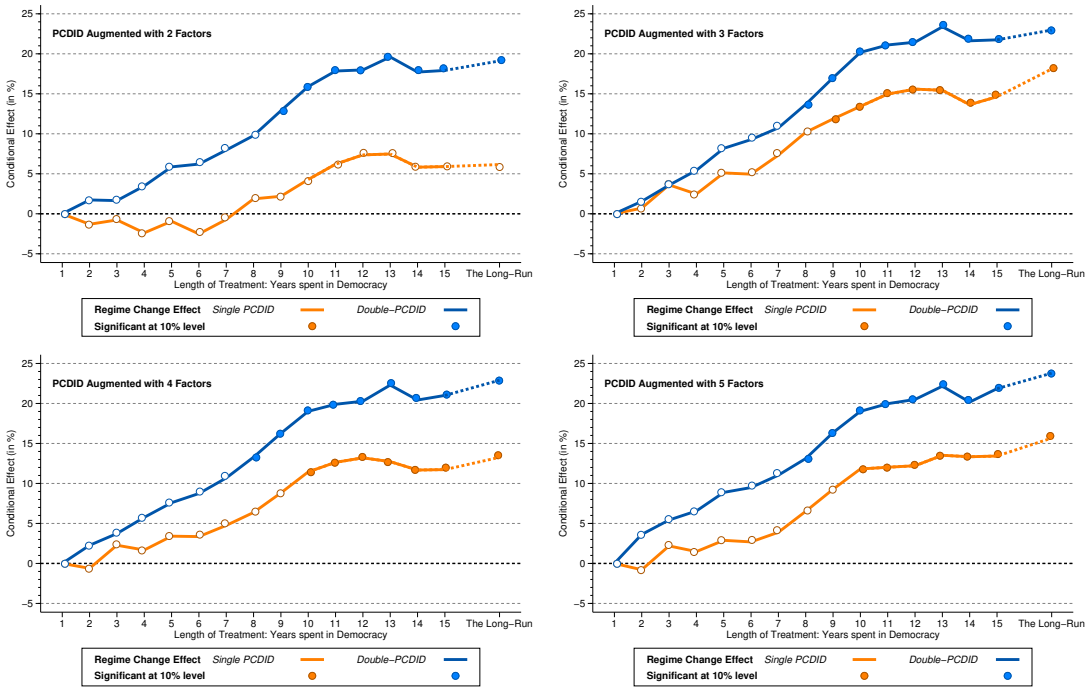
for episode (A) and regime change (B) effects along with the early year effects (B'). Table F-1 reports the p-values for the related weak parallel trend tests, which indicates that the models augmented with 2 and 3 factors are sound when we consider all countries (Panel A). Figure F-1 presents our findings, which are qualitatively identical to those using our alternative methodology (see figure note for details). These results, including Alpha tests, are qualitatively unchanged if we use year dummies only up to $k = 10$ to conserve degrees of freedom.

Table F-1: Alpha test for the weak parallel trends assumption

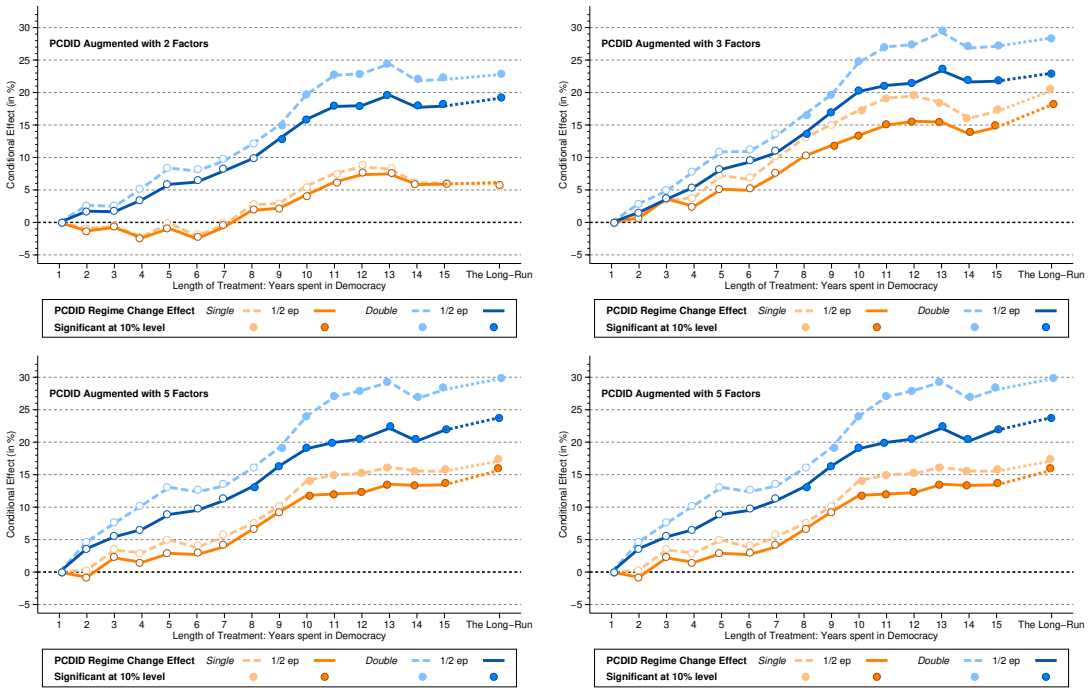
	(A) All countries			**(B) 1 or 2 episodes**		
Factors	Episode	Regime	Joint	Episode	Regime	Joint
1	0.014	0.004	0.016	0.387	0.234	0.400
2	0.298	0.336	0.572	0.763	0.775	0.953
3	0.236	0.498	0.441	0.812	0.525	0.783
4	0.013	0.011	0.029	0.346	0.283	0.551
5	0.004	0.008	0.012	0.208	0.377	0.452
6	0.000	0.002	0.001	0.064	0.289	0.171

Notes: We report the p-values for the Alpha test for weak parallel trends in the full treated sample (N=62) for the model including a dynamic evolution in equation (9) above with year dummies up to $k = 15$. See Appendix Section D for details on the construction of the test. Factor augmentation for $m = 1, \ldots, 6$ is meant to imply 'm' factors constructed from the episode control sample regressions and *an additional* 'm' from the regime change control sample regressions.

Figure F-1: Single and Double PCDID



(a) Estimated Evolution with $k = 2, \ldots, 15$ years in democracy ($k = 1$ set to 0)



(b) dto. highlighting countries with $\leq$2 episodes (lighter shading)

Notes: We present Single and Double PCDID results for specifications including the ERT regime change dummy as well as dummies for each of 2 to 15 years in democracy: in panel (a) for all countries, and in panel (b) additionally for countries with only 1 or 2 episodes (ligher shading). These are not predictions based on running line regressions as in the main text, but the averaged (Mean Group) estimates of $\hat{d}_i^B$ (Long-Run) and $\sum_i \hat{d}_i^B + \sum_i \hat{d}_{ik}^{B'}$ (for years $k = 2, \ldots, 15$ with $k = 1$ set to 0) from equation (9). A hollow (filled) marker indicates that the 90% confidence interval of the average estimate does (not) include zero. Inference is based on the standard errors of the Mean Group estimate of $\hat{d}_i^B$ (following Chan & Kwok 2022), while for the year estimates 2 to 15 it is based on Wald tests for each sum of averaged estimates (i.e. $\sum_i \hat{d}_i^B + \sum_i \hat{d}_{ik}^{B'} = 0$ for $k = 2, \ldots, 15$). Single (Double) PCDID specifications include between 20 and 23 (23 and 29) regressors for models augmented with 2 to 5 factors; average T is 60.