

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 not a discrete event but a two-stage process: autocracies first enter into 'episodes' of political liberalization; in the second stage, these episodes either culminate in regime change or not. Failure to account for this chronology and the implicit counterfactual groups risks biased estimates due to selection effects. Adopting a repeated-treatment difference-in-difference implementation, which captures non-parallel trends and selection into treatment, we find that a single event approach substantially underestimates the economic dividends from regime change and, crucially, obscures the permanent growth effect of democratisation.

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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 boycotts, strikes and demonstrations. 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.

Such a drawn-out liberalisation process eventually culminating in democratic regime change is far from uncommon: in the 62 countries in our sample (1950-2014) which experienced democratic regime change the median length of time spent undergoing such a liberalisation process (henceforth ‘democratisation episode’) is four years — we elaborate on definitions and data sources below — yet existing studies on the growth effects of democracy do not account for this chronology.

The first contribution of this paper is to consider democratisation as a process rather than a discrete event, and to accommodate this chronology in the empirical analysis of the democracy-growth nexus: countries select into democratisation episodes, and some select out of these episodes into democratic regime change. Our approach is hence situated between those studies which favour binary democracy indicators (e.g. [Giavazzi & Tabellini 2005](#), [Rodrik & Wacziarg 2005](#), [Papaioannou & Siourounis 2008](#), [Acemoglu et al. 2019](#)) and those which favour continuous measures in analysing the economic implications of democratic change (e.g. [Knutsen 2013](#), [Murtin & Wacziarg 2014](#), [Madsen et al. 2015](#)).

Our second contribution is that we include the countries with failed attempts at democratic regime change as a separate control group in our empirical analysis. Not all democratic struggles culminate in successful regime change: around 40% of the countries in our sample experienced democratisation episodes yet never became democracies. What is the appropriate control group when studying the economic implications of democratic regime change? Modelling regime change as a two-stage process identifies distinct groups: some autocracies select into liberalisation, whereas others (the first control group) do not; subsequently, some liberalising autocracies select into regime change (the treated group), whereas others (the second control group) do not. Thus, we distinguish between those nations which attempted liberalisation 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. 2021](#)).

The third contribution of this paper is methodological: we extend previous causal inference in a heterogeneous Difference-in-Difference framework to our proposed two-stage setup. 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. One motivation for our approach is that length of treatment (years 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. Focusing on average effects further obscures the qualitative nature of the democratic dividend, since it imposes the interpretation of a one-off levels

effect on the treatment, ignoring the potential for a perpetual growth effect. Instead, using running line regressions we show the central tendencies in estimated country treatment effects relative to the length of time spent in democracy, while accounting 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. Furthermore, by conditioning on the frequency of democratisation episodes, the years spent in episodes, and their estimated effect on development this approach allows us to account for the two-stage nature of democratic change we advocate.

The distinction between democratisation episode and democratic 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).¹ Democratic regime change is based on V-Dem's Regimes of the World (ROW) measure (Lührmann et al. 2018) for democracy but further requires a 'founding election' to take place — simple abolition of an autocracy is not sufficient to constitute democracy. Our sample includes data from 1950-2014, covering a total of 227 episodes and 70 regime changes in 105 countries.²

Our empirical approach builds on previous studies in this literature adopting difference-in-difference specifications (e.g. Giavazzi & Tabellini 2005, Persson & Tabellini 2006, Papaioannou & Siourounis 2008). Recent work on the causal analysis of treatment effects expresses serious reservations about the use of the two-way fixed effects estimator within the difference-in-difference framework when treatment effects are likely to be heterogeneous (Athey & Imbens 2021, De Chaisemartin & d'Haultfœuille 2020)³ The novel empirical implementation by Chan & Kwok (2021) we adopt and extend in our analysis estimates the *country-specific* treatment effects and allows for non-parallel pre-treatment trends as well as endogenous selection into treatment. This follows a recent tradition which introduces the interactive fixed effects structure popular in the panel time series literature (Pesaran 2006, Bai 2009, Chudik & Pesaran 2013) to the empirics of policy evaluation (e.g. Gobillon & Magnac 2016, Xu 2017). Our implementations employ estimates of unobserved common factors as proxies to capture the endogeneity arising from selection into treatment and non-parallel trends. The standard single-treatment model (henceforth Single PCDID) includes one treatment dummy (regime change) and estimated factors from one control group; in our extension to a repeated treatment setup (henceforth Double PCDID) we employ two treatment dummies (episode, regime change) and estimated factors from two control groups: (i) countries which never experienced a democratisation episode, and (ii) countries which experienced an episode but not regime change.

We find that failing to account for episodes and selection into regime change by adopting the appropriate counterfactual groups substantially *underestimates* the economic benefits of democratisation: first, positive economic effects emerge substantially earlier in our Double PCDID results than in a standard model considering only regime change; second, the magnitude of these benefits from democratisation substantially diverge in that they continue to accrue with increasing number of years in democracy in our preferred Double PCDID model but plateau fairly soon after a small burst at around 25 years in the Single PCDID model: hence, our chronology of episode and regime change

¹The ERT dataset further captures democratisation episodes within democratic regimes, which are ignored in our analysis: we focus on political liberalisation in autocracies which did or did not result in democratic regime change.

²Our treated sample comprises 62 countries experiencing 141 episodes. The 43 control group countries experienced 86 episodes, the median rate of 2 episodes per country is identical across samples. Appendix Section A provides details.

³Existing research on democracy and growth has unanimously assumed a common democracy-growth effect across countries. See Eberhardt (2021) for a detailed motivation of the heterogeneous democracy-growth nexus.

suggests a perpetual growth effect from democratisation, whereas the conventional approach implies a one-off levels effect (e.g. [Acemoglu et al. 2019](#), [Eberhardt 2021](#)).

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 of the patterns of democratisation episodes and regime change over the sample period. Section 3 covers the common factor model setup and the empirical implementations in greater detail. Main results and robustness checks are presented in Section 4, Section 5 concludes.

2 Regime Change as a Two-Stage Process

In this section we provide a conceptual motivation for democratisation as a two-stage process. We then introduce the data used and offer some descriptive analysis.

2.1 Conceptual Development

Our empirical implementation captures two elements of democratisation we seek to motivate: 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 an appropriate counterfactual to regime change).

Empirical studies of democratisation are commonly focused on the analysis of electoral autocracies, so-called ‘hybrid regimes’, which have occupied the political science literature for at least the past two decades ([Diamond 2002](#), [Brownlee 2009](#), [Levitsky & Way 2010](#), [Geddes et al. 2014](#)). These authors appear to tacitly agree that democratisation is an event, a single moment of “dramatic upheaval” ([Gunitsky 2014](#), 561) in the fashion of [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](#)). Many formal models of nondemocratic politics can be argued to 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 events of ‘dramatic upheaval’ leading to democratic regime change undoubtedly do occur, these arguments suggest that establishing the political institutions of democracy, in all their multifaceted complexity, frequently does not happen over night.

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”. The very presence of hybrid regimes and the uncertainty over their ‘direction of travel’ creates awkward questions for the empirical literature on the democracy-growth nexus employing binary representations of democratic regime change (e.g. [Giavazzi & Tabellini 2005](#), [Papaioannou & Siourounis 2008](#), [Acemoglu et al. 2019](#)): this practice requires that within-category subjects are homogeneous ([Wilson et al. 2021](#)) and hence all ‘negative’ cases of transition are lumped together.⁴ This is problematic as 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 latter group, 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 democracy data from the V-Dem 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 democracy data from [Marshall et al. \(2017](#), PolityIV), where a positive polity2 variable indicates democracy, and from V-Dem’s Regimes of the World ([Lührmann et al. 2018](#), ROW).

We log-transform real per capita GDP and multiply this by 100: results are estimates of the percentage change in per capita income as a result of regime change. We add population growth and the export/trade ratio, aggregated from bilateral export and import flows, as additional controls. Population growth as covariate 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 (e.g. [Papaioannou & Siourounis 2008](#), Table 3 [5]; [Acemoglu et al. 2019](#), Table 6 [6]).

We adopt the democratic regime transformation dummy from ERT (`reg_type`) alongside the democratisation episode dummy (`dem_ep`). An episode requires (i) a small increase (0.01) in the V-Dem polyarchy index⁵ for a country classified as ‘closed’ or ‘electoral democracy’ (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. Polyarchy represents the minimal definition of democracy favoured in political science ([Teorell et al. 2019](#), [Boese 2019](#)). Since autocracies have low index levels this cumulative growth amounts to a substantial proportional increase.⁶ 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. The democratic regime change dummy builds on the ROW categorisation of democracy but further requires a founding democratic election to occur. Our analysis focuses on episodes of democratisation originating in autocracies.⁷

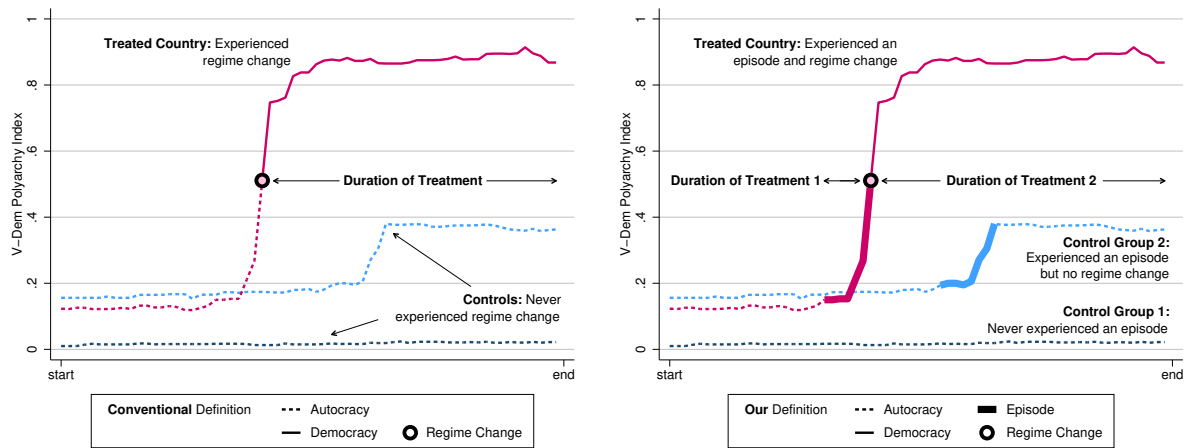
⁴In a separate literature which adopts *continuous* measures for democracy (e.g. [Knutsen 2013](#), [Murtin & Wacziarg 2014](#), [Madsen et al. 2015](#)) failed liberalisations remain similarly undistinguished.

⁵This increment may seem small, 1% of the range of the entire 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. 2021](#)).

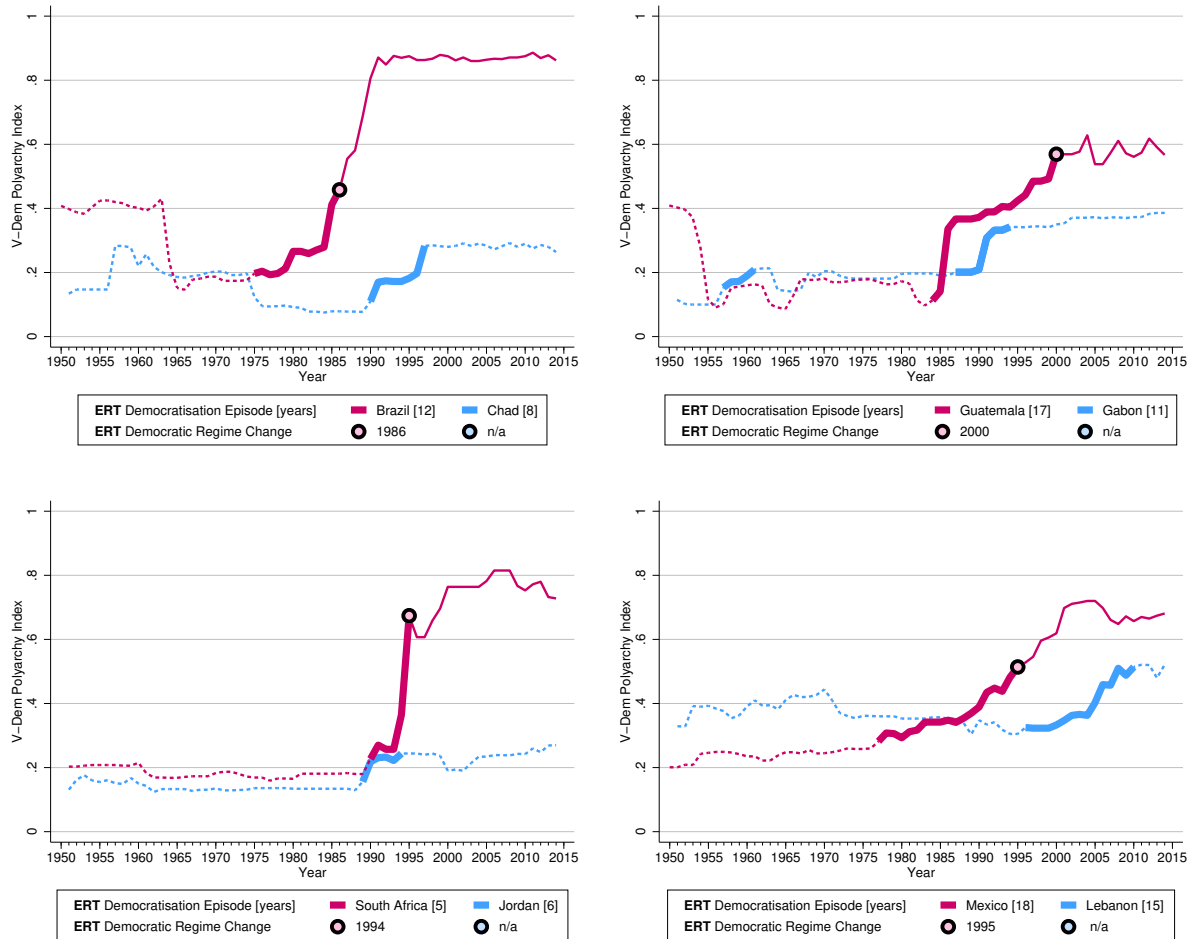
⁶However, no democratisation episode can leave a country in the ROW ‘closed autocracy’ category.

⁷In order to obtain separate treatment effect estimates for episodes and regime changes we exclude episodes of

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).

Panel (a) of Figure 1 contrasts the single treatment approach (left diagram), including the conflation of heterogeneous control groups, with the two-stage treatment approach advocated 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.

All variables are available from 1901 to 2014, but here 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 empirical approach forces us to consider the relative sample sizes of treated and various control groups — we regard this as a core strength of this implementation. Our 1950-2014 sample covers 62 ‘treated’ countries which experienced episodes and regime change ($n=3,724$), 43 autocratic countries which only experienced democratisation episodes ($n=2,515$; control group 2), and 15 autocratic countries which never experienced episodes ($n=646$; control group 1).⁸

2.3 Descriptive Analysis: Democratisation Episodes and Regime Change

Panel (b) of Figure 1 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). These graphs show that (i) episodes often span several years, and that (ii) the outcome of this liberalisation process is uncertain: country pairs presented start out with near-identical polyarchy scores in the 1950s, but at times end up at opposite ends of the scale in 2014.

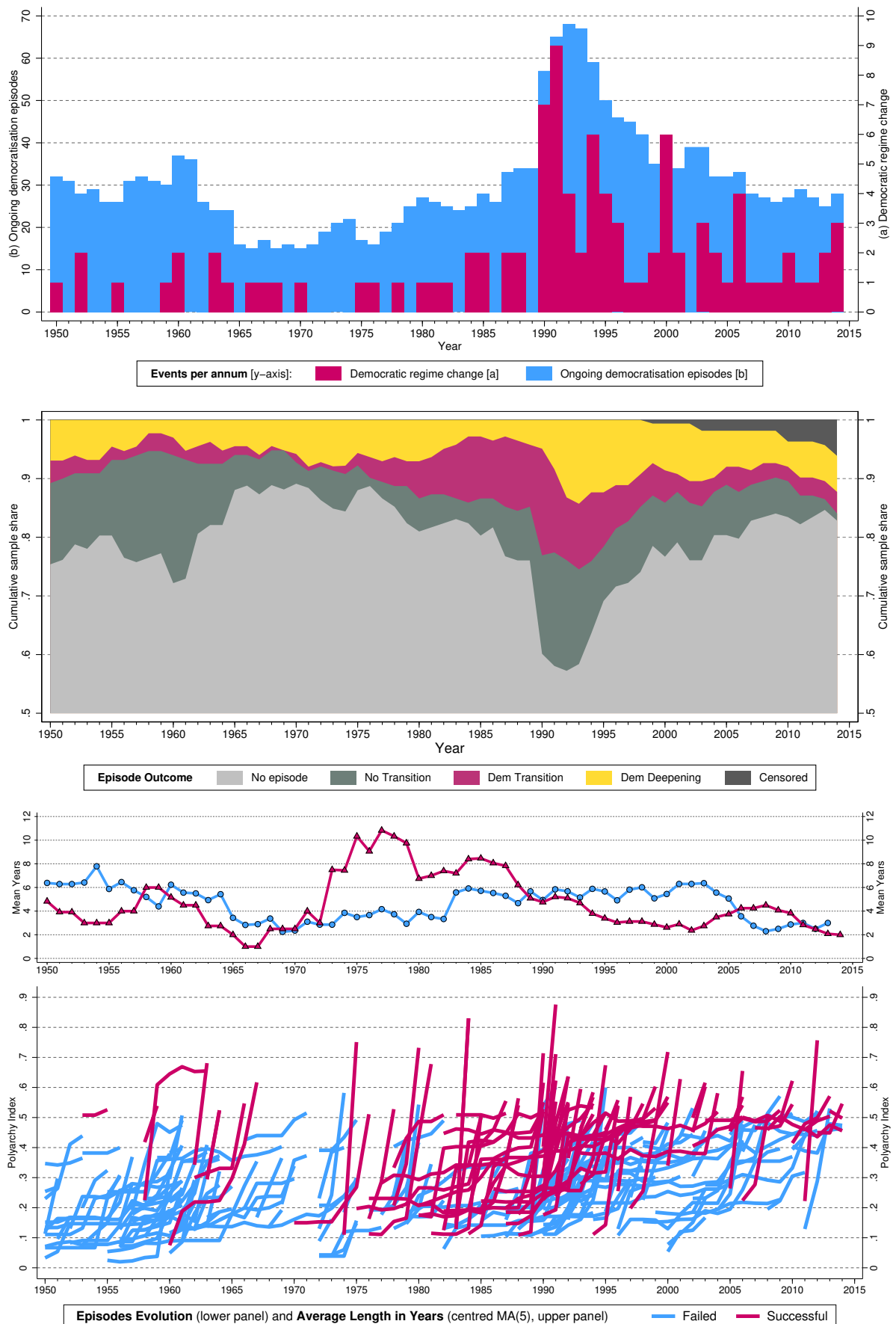
These observations apply to the vast majority of episodes. Figure 2 provides an overview of the distribution of episodes and regime changes in our sample. In the top panel the histogram in light blue highlights two peaks of democratisation episodes in the late 1950s/early 1960s, and in the 1990s, coinciding 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 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) — the bottom panel in Figure 2 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 (short episodes), yet it would be misleading to claim that these trajectories *dominate* the treatment sample.

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.

⁸We cannot use all 71 countries with successful regime change since nine of them have no pre-episodal observations which prevents separate identification of episode and regime change effects — see Appendix Table A-3.

Figure 2: 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.

In Appendix Section A, we list the countries and further details of the three samples under analysis: (i) the 62 treated countries which experienced episodes and regime change. These spend on average 16% (st.dev. 9%) of their observations in episodes, 38% (22%) in democracy and 46% (19%) in autocracy; (ii) the first control group of 15 autocratic countries which never experienced a democratisation episode; prominent members include China, Viet Nam, and a number of (oil-rich) Middle Eastern autocracies; and (iii) the second control group of 43 autocracies which experienced failed democratisation episodes; many of these are in Africa and on average around 20% (st.dev. 14%) of their observations are in episodes.

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 as a single or a repeated ‘treatment’. We discuss the [Chan & Kwok \(2021\)](#) Principal Component Difference-in-Difference estimator (‘Single’ PCDID) and our extension, the ‘Double’ PCDID, for these respective cases. The final part of this section introduces our novel presentation of heterogeneous treatment effects using predictions from running line regressions.

3.1 Single PCDID

The ‘single treatment’ case is simply an endogenous selection into democratic regime change where pre-treatment trends between treated and non-treated (never-regime changing) countries are potentially non-parallel. Our setup captures the possibility of a correlation between time-varying observed as well as unobserved determinants of economic development (absorptive capacity, culture, etc) and democratic regime change and hence of selection into treatment. The treatment is defined by a binary variable, such as a positive value in the polity2 variable of PolityIV, the ROW measure, or the V-Dem ERT categorisation of regime change. The control group is the set of countries which remained autocratic throughout the sample period. As motivated above we adopt export/trade and population growth as controls.⁹

The basic intuition for [Chan & Kwok’s \(2021\)](#) PCDID estimator follows that of the control function approach in microeconomic analysis of production functions ([Olley & Pakes 1996](#), [Levinsohn & Petrin 2003](#)) with factors taking on the role of the control functions: common factors are estimated from the control sample via Principal Component Analysis (PCA) and added to the country-specific estimation equation for the treated units as additional covariates.

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

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

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

⁹The principal components are estimated from the residuals of the country-specific regressions of income per capita on export/trade, population growth and an intercept.

where the first and second 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 is a vector of observed covariates with associated country-specific parameters β_i ,¹⁰ $\mu'_i f_t$ represents a set of unobserved common factors f_t with country-specific factor loadings μ_i , and $\tilde{\epsilon}_{it}$ is the error term.

The treatment effect is assumed to decompose into $\Delta_{it} = \bar{\Delta}_i + \tilde{\Delta}_{it}$, with $E(\tilde{\Delta}_{it}|t > T_{0i}) = 0$ $\forall i \in E$ since $\tilde{\Delta}_{it}$ is the demeaned, time-varying idiosyncratic component of Δ_{it} ; we refer to $\bar{\Delta}_i$ as ITET, the treatment effect of unit i averaged over the post-intervention period — this is our key parameter of interest. The reduced form model is

$$y_{it} = \bar{\Delta}_i \mathbf{1}_{\{i \in E\}} \mathbf{1}_{\{t > T_{0i}\}} + \varsigma_i + \beta'_i x_{it} + \mu'_i f_t + \epsilon_{it} \quad \text{with} \quad \epsilon_{it} = \tilde{\epsilon}_{it} + \tilde{\Delta}_{it} \mathbf{1}_{\{i \in E\}} \mathbf{1}_{\{t > T_{0i}\}}, \quad (3)$$

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

The factor structure has a long tradition in the panel time series literature to capture strong cross-section dependence (e.g. [Pesaran 2006](#), [Bai 2009](#)), a form of unobserved, time-varying heterogeneity.¹¹ Strong correlation across panel members is distinct from weaker forms of dependence, such as spatial correlation, and if ignored can lead to serious (omitted variable) bias in the estimated coefficients on observable variables ([Phillips & Sul 2003](#), [Andrews 2005](#)). Here, 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 further 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 .

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, $\mu'_i f_t$ ('interactive effects'), as in [Athey et al. \(2021\)](#) and the panel time series literature cited above, and that ϵ is orthogonal to all conditioning components in equation (4): all aspects of treatment endogeneity and nonparallel trends are assumed to be captured by the low-dimensional factors, the controls, and the deterministic term as well as their combinations/correlation with the treatment variable. Since factor proxies are measured with error, the idiosyncratic errors $\tilde{\epsilon}_{it}$ of treated and non-treated units may be correlated — this asymptotic bias is removed with a condition that $\sqrt{T}/N_C \rightarrow 0$, where T is the time series dimension of the treated sample and N_C is the number of units in the control sample.

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

¹⁰As common in the literature ([Pesaran 2006](#)) we assume $\beta_i = \bar{\beta} + \tilde{\beta}_i$ where $E(\tilde{\beta}_i) = 0$. Covariates x and factors f can be orthogonal or correlated.

¹¹[Eberhardt & Teal \(2011\)](#) discuss these models with reference to cross-country growth empirics.

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.

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 experienced 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 transitioned into democracy. We postulate that the most relevant control group for countries successfully transitioning into democracy are not all countries which never experienced regime change, but a strict subset of those which engaged in democratisation episodes: countries that tried and failed, rather than a combination of those that tried and failed and those that never tried.

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 unobserved common factors can be correlated with treatments or observed covariates. Correcting for repeated treatment/selection requires the use of estimated common factors from two distinct control groups.¹²

Setup We extend the PCDID to a repeated-treatment Difference-in-Difference specification:

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

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 'Implementation' below for notation). The setup can be thought of as a double-selection process where selected and 'discarded' units are possibly on different trajectories and selection itself may be correlated with observables and/or unobservables; or as a repeated-threshold regression model where treated units are those which

¹²The 'repeated treatment' setup can also speak to the assumption of unanticipated treatment (e.g. Callaway & Sant'Anna 2021, Sun & Abraham 2021).

overcome both thresholds.¹³ We now assume two sets of multi-factor error terms: one for each counterfactual group. The reduced form is now

$$y_{it} = \bar{\Delta}_i^A \mathbf{1}_{\{i \in E^*\}} \mathbf{1}_{\{t > T_{0i}\}} + \bar{\Delta}_i^B \mathbf{1}_{\{i \in E^*\}} \mathbf{1}_{\{t > T_{1i} > T_{0i}\}} + \varsigma_i + \beta'_i x_{it} + \mu_i^{A'} f_t^A + \mu_i^{A' B'} f_t^{A' B} + \epsilon_{it} \quad (6)$$

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

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

$$y_{it} = b_{0i} + d_i^A \mathbf{1}_{\{t > T_0\}}^A + d_i^B \mathbf{1}_{\{t > T_1 > T_0\}}^B + a_{1i}^{A'} \hat{f}_t^A + a_{2i}^{A' B'} \hat{f}_t^{A' B} + b'_{1i} x_{it} + e_{it}, \quad (7)$$

where the \hat{f} with superscript A (A B) 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 (second) control group, and d_i^A and d_i^B are the country-specific treatment parameters for episodes and regime change. 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 or prevent them from realising 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 (the repercussions of) a financial crisis or natural disaster. We know that oil exploration is guided by global prices and follows (not leads) institutional change (Cust & Harding 2020), while financial crises have sizeable international dimensions (Cesa-Bianchi et al. 2019, Arellano et al. 2017) — all arguments in favour of our low-dimensional factor structure. In Appendix C we run separate event analyses for GDP pc 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) and EM-Dat. These suggest no systematic differences between the effects apparent in the two groups. We also study the parallel trend assumption in the Single versus Double PCDID models adopting the parallel trend test under factor structure developed by Chan & Kwok (2021). This test cannot reject the null of common trends for the Double PCDID regime change effect.

3.3 Heterogeneous Treatment Effects and Inference

Given the decomposition of the treatment effect Δ_{it} a typically useful estimate would be the ATET, which in our setup would be $\bar{\Delta} = E(\bar{\Delta}_i)$, the average of the ITET across treated units $i \in E$ or E^* . This makes sense in the context of a level effect of treatment which manifests itself after a

¹³The methodology extends to the analysis of many treatments, provided that respective treatment and control samples are sufficiently large.

small number of years, as one would expect in the context of many medical interventions.¹⁴ In the context of the democracy-growth nexus we instead argue for an alternative means of presentation, namely conditional running line plots of the estimated ITET for democratic regime change, \hat{d}_i^B , in relation to the years of treatment. This has the following 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; (iii) we can account for multiple regime changes within a country; (iv) 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 , and (v) we do not impose the restriction that democracy only has a one-off levels effect on income as implied by an ATET approach.

A running line regression smooths the dependent variable against the independent variable by using subsets of nearest neighbours in local linear regressions. In our presentation we can rely on simultaneous smoothing on multiple independent variables: we are able to add additional controls to address the sample concerns as well as the idiosyncracies of countries' democratic histories with regard to episodes and regime changes.

Since in analogy to a standard Mean Group estimator the regular ATET in the [Chan & Kwok \(2021\)](#) PCDID is simply the average across all treated units i , with a nonparametric variance estimator following [Pesaran \(2006\)](#), we adopt running line regressions as 'local ATET', where 'local' refers to a similar number of years spent in democracy, and adopt the standard errors from this methodology.¹⁵

4 Empirical Results

Single PCDID Panel (a) of Figure 3 presents the results from Single PCDID models for three democracy indicators: a dummy for a positive polity2 score (PolityIV), a dummy for the V-Dem ROW measure ($ROW \geq 2$), and the ERT regime type dummy. In all cases the democracy effect (in percent, y -axis) is smoothed over the years the country spent in democracy (x -axis) using multiple running line regression. Here, and in all other results graphs below, we further control for (i) the start year of the country series, and (ii) the number of times a country moved into or out of democracy. Filled (white) markers indicate statistically (in)significance at the 10% level.¹⁶

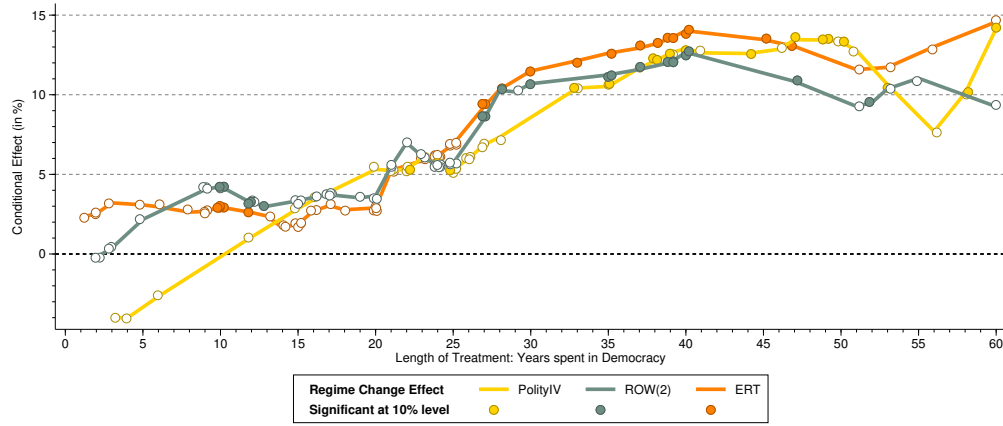
The treatment effects and their relationship with length of time spent in democracy are largely identical across results for these three democratic regime type indicators: effects are moderately positive and statistically insignificant for the first 25 years, whereupon additional years spent in democracy are associated with a rise in income up until around 40 years of 'treatment', which is associated with a 12-14% higher per capita GDP. Thereafter the effect plateaus. Why are the effects for some indicators, here PolityIV but others in the following analysis, negative (albeit insignificant) in the initial years? There are at least two plausible explanation: first, we estimate static models,

¹⁴We 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 the 'treatment length' (early vs late treatment) at the heart of the presentation of results.

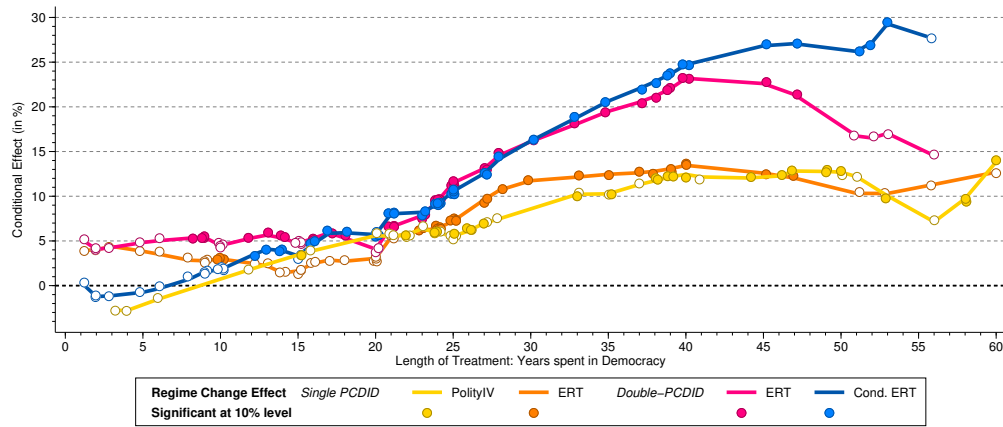
¹⁵Since these standard errors do not fully account for the correlation amongst the regressors we employ bootstrap methods with 1,000 replications to show that using bias-corrected confidence intervals the patterns of statistical significance are very similar to those in the uncorrected results — see Appendix B for detailed presentation.

¹⁶The sample size is limited to the 62 'treated' countries in the Double PCDID analysis below.

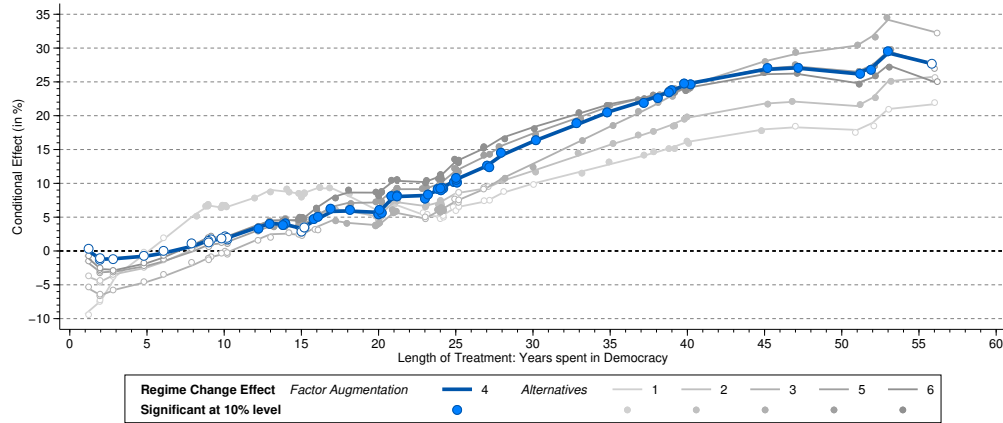
Figure 3: Democracy and Economic Development — Single and Double PCDID, Robustness



(a) Single PCDID Results



(b) Comparison of Single and Double PCDID Results



(c) Alternative Factor Augmentations: Conditional ERT

Notes: We present results from running line regressions of country-specific democracy coefficients on years spent in democracy, conditioning on the start year of the country series and the number of democratisations a country experienced. The sample matches that of the Double PCDID estimates for ERT presented in panel (b). All results are for PCDID models augmented with 4 common factors for each control group except in panel (c). In panel (a) the democracy estimates are derived from Single PCDID estimates adopting the positive polity2 variable (PolityIV), the V-Dem Regimes of the World definition (ROW2), and the V-Dem ERT dummy. Panel (b) presents the results for Single PCDID alongside those for Double PCDID estimates — the latter control for selection into democratisation episodes and separately into democratic regime change. We present the results for the ERT dummy, with the running line model labelled ‘Cond. ERT’ further conditioning on the country estimates for a democratisation episode (\hat{d}_i^A), the number of and years spent in democratisation episodes. In Panel (c) we report ‘Cond. ERT’ results from Double PCDID models augmented with 1 to 6 factors from each of the respective control groups.

hence we would expect these represent the dip in per capita income commonly found around the time of regime change (Acemoglu et al. 2019, Cervellati & Sunde 2014); second, like the literature we use crude indicators for regime change, which are subject to measurement error at the margin (i.e. in the first few years).

Double PCDID In Panel (b) of Figure 3 we combine results from Single PCDID models with those from the Double PCDID: the orange and yellow lines are the running line estimates for the effect of democracy based on the PolityIV and the ERT definition of democracy using a Single PCDID model — these are the same as the results in panel (a). The pink line presents the running line estimate for the ERT definition of democratic regime change in a Double PCDID model, while the blue line presents the result for the same definition of democracy but here the running line regression further accounts for country information on (i) the number of democratisation episodes, (ii) the years spent in democratisation episodes, and (iii) the coefficient estimate on the episodes dummy, \hat{d}_i^A . The blue line hence is the preferred specification since it accounts for the double-selection process inherent in democratic regime change.

When accounting for the episodic nature of democratisation, we can reveal an important *qualitative* difference to existing analyses: a transition to democracy has a permanent effect on economic growth, not merely a one-off levels effect on per capita GDP. Comparing the blue and orange lines it is obvious that the ERT definition of democracy implies a much more substantial long-run effect on development *if we account for democratisation episodes*: in the early years of democracy this yields a statistically significant positive effect from around 12 years onwards (10% higher per capita GDP), rising linearly for further years spent in democracy and reaching around 30% higher per capita GDP after 50 years in democracy. In contrast, as established above, the Single PCDID estimates indicate an insignificant effect of democracy up to 25 years and a flattening out beyond that at a maximum of 12-14% higher per capita GDP.

Robustness All of the above estimates are constructed from PCDID models where we include four common factors estimated from each control group to account for non-parallel trends and selection.¹⁷ In panel (c) of Figure 3 we show the conditional ERT estimate for the augmentation with four common factors (from each control group) in blue alongside alternative specifications with 1 to 6 common factors (dto). Augmented with only one or two common 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 identical results. This outcome is not surprising since research by Moon & Weidner (2015) suggests that including more factors than necessary does not bias the results for the parameter of interest.

In additional robustness checks we explore the empirical reality that even countries which successfully transitioned to democracy often needed several attempts (episodes): merely 25 of the 62 treated countries had only one democratisation episode, 21 had two, and 16 between three and five. We demonstrate that the strong positive effect in our main results in Figure 3 is driven by countries which experienced one or two democratisation episodes. Nevertheless, the divergence between Single and Double PCDID results remains in *all* groups (see Appendix Figure B-2).

¹⁷In the Double PCDID this means four factors constructed from the control group which never experienced an episode and four factors from the control group which experienced democratisation episodes but not regime change.

A 'plain vanilla' version of the Single and Double PCDID excluding covariates x produces identical relative patterns in running line regressions (see Appendix Figures B-3 and B-4).

Democratisation episodes and regime changes are not uniformly distributed over time, we therefore explore alternative start (1950-1970) and end years (1995-2014). The significant deviation between the economic effects of modelling a simple model and our two-stage model of regime change remains, regardless of the start year or end year adopted (see Appendix Figures B-5 and B-6).

5 Conclusion

This paper speaks to recent efforts in the analysis of the democracy-growth nexus which emphasise that greater care needs to be taken in defining democratic regime change events (Papaioannou & Siourounis 2008, Acemoglu et al. 2019). In contrast to these studies which employ single regime change indicators we motivate and empirically implement democratisation as a two-stage process, made up of a liberalisation episode and regime change. Our main concern is the selection bias implicit in modelling democratisation as a single event. We propose an alternative approach which uses all those countries which embarked on an ultimately unsuccessful liberalisation episode as a counterfactual to the group of countries which became democracies. We use a repeated-treatment Difference-in-Difference estimator adapted from Chan & Kwok (2021) which allows for heterogeneous treatment effects, and compare the outcomes using the implementation which assumes a single treatment. Focusing on the distributional features of the individual treatment effects, our findings suggest that ignoring the two-stage nature of democratisation substantially *underestimates* the economic effect of democracy on economic development. The conventional single regime change approach implies a one-off levels effect on the order of 12-14% higher per capita GDP as the long-run implication of democratisation, whereas our two-stage approach suggests that democracy can exert a perpetual growth effect — a democratic dividend that keeps on giving.

Our approach identifies the economic significance of the democratic dividend but we cannot point to the specific democratic institutions driving this result (e.g. rule of law, including property rights protection; constraints on the executive). The answer to the important question 'which institutions rule' is beyond the scope of this study and left for future research.

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Appendix — Not Intended For Publication

A Data Appendix

We provide detailed information of the makeup of three samples: in Table A-3 we study the ‘treated’ sample, the countries which experienced democratisation episodes and democratic regime change. In Table A-2 we report details on the countries which experienced democratisation episodes but never realised regime change, while Table A-1 covers all those countries which remained autocracies throughout their sample period and never had any democratisation episodes.

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), 43 autocratic countries which only experienced democratisation episodes ($n=2,515$; control group 2), and 15 autocratic countries which never experienced episodes ($n=646$; control group 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 \(2008\)](#). Note that [Eberhardt \(2019\)](#) demonstrates the fragility of results in [Acemoglu et al. \(2019\)](#) when countries with ‘small N’ are excluded from analysis, highlighting the overall lack of robustness in these authors’ findings.

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)

Country	ISO	Total obs	Episodes (all failed)								Autocracy		
			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 Repu	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

Country	ISO	Total obs	Episodes (successful or failed)										Regime change to democracy					Autocracy				
			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)

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

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

Country	ISO	Total obs	Episodes (successful or failed)										Regime change to democracy					Autocracy				
			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 & Príncipe	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%

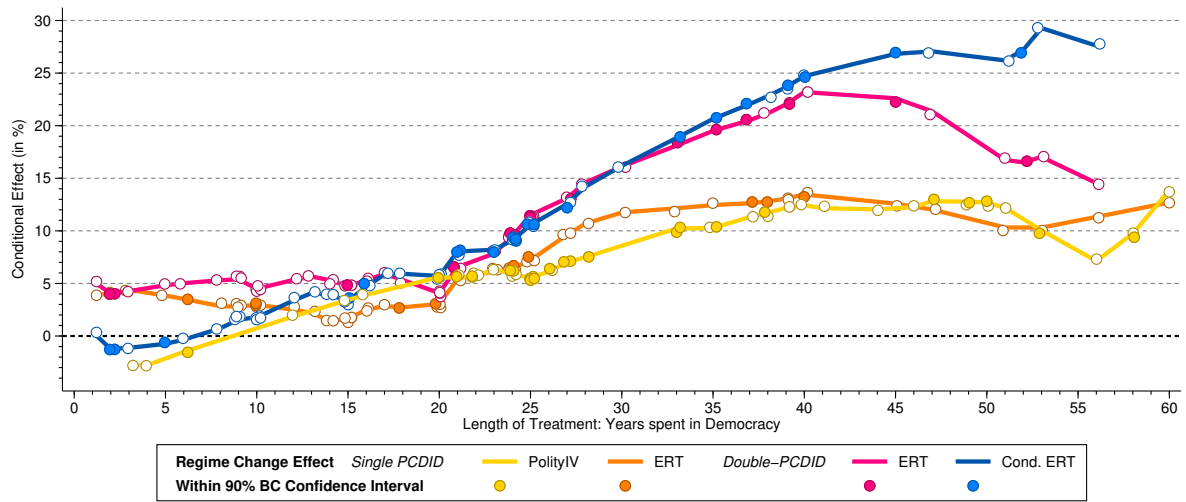
Notes: This table provides details on the sample-makeup of the 'treated' sample, i.e. the set of countries which experienced at least one democratisation episode followed by a regime change. There are 75 countries in this table, but only 62 of them have estimates for both the episode dummy and the democratic 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 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)

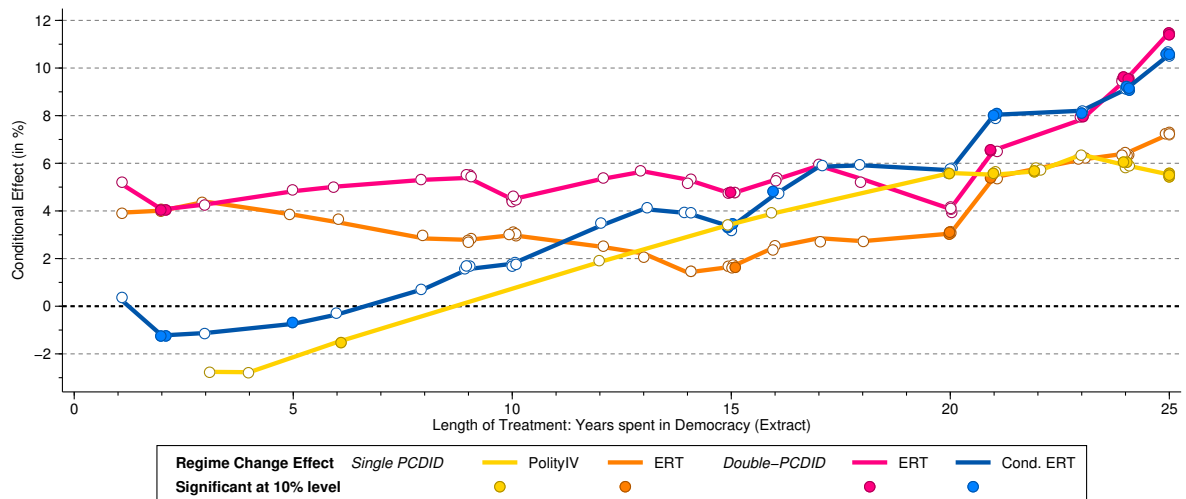
The first set of columns after the country name, ISO code and total observation count refers to information on the total number of episodes, their average length, and timing as well as the count and average length for failed episodes. The next set of columns refers to successful regime changes, how long 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).

B Additional Figures and Tables

Figure B-1: Democracy and Economic Development — Bootstrap Inference for Single and Double PCDD



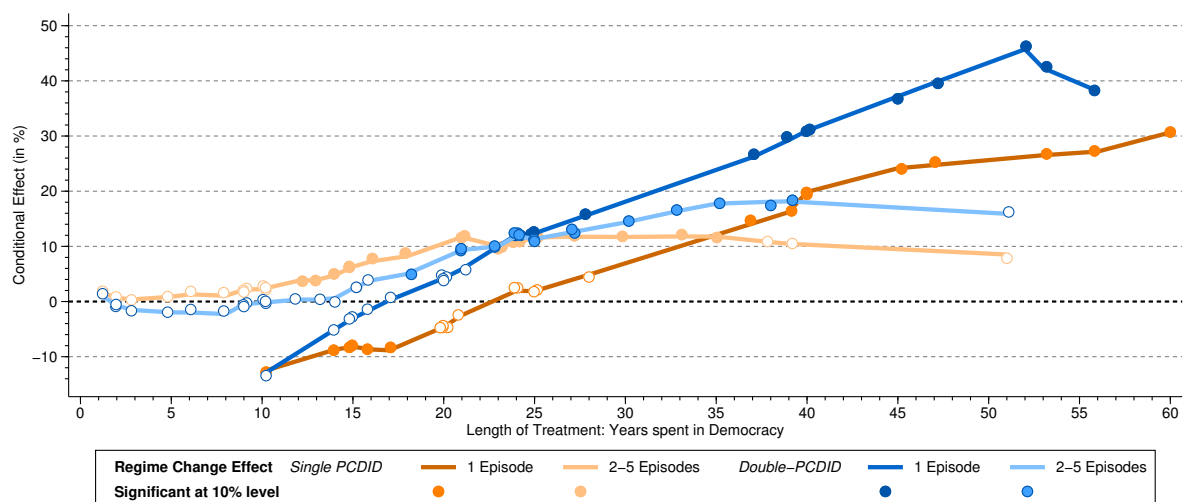
(a) Full Results



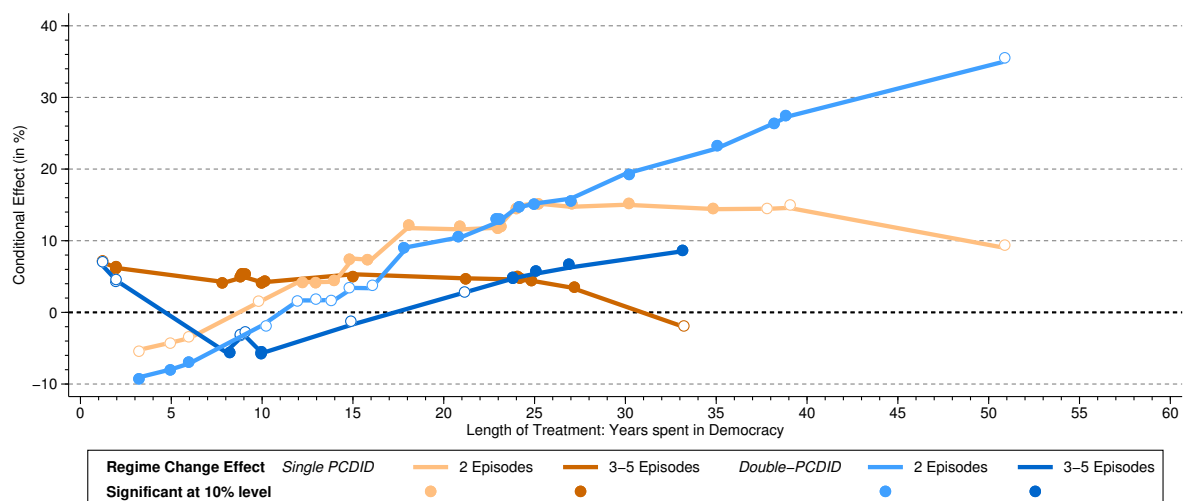
(b) Zooming in on the first 25 years in Democracy

Notes: These plots presents the results from running line regressions of country-specific coefficients on the democracy (ERT) dummy, derived from Single and Double PCDD estimates. These are the results using bias-corrected 90% confidence intervals (via 1,000 bootstrap replications) for inference. Panel (a) presents the results for all treatment lengths, Panel (b) focuses on fewer than 25 years in democracy.

Figure B-2: Democracy and Economic Development — Single vs Multiple Episodes Groups



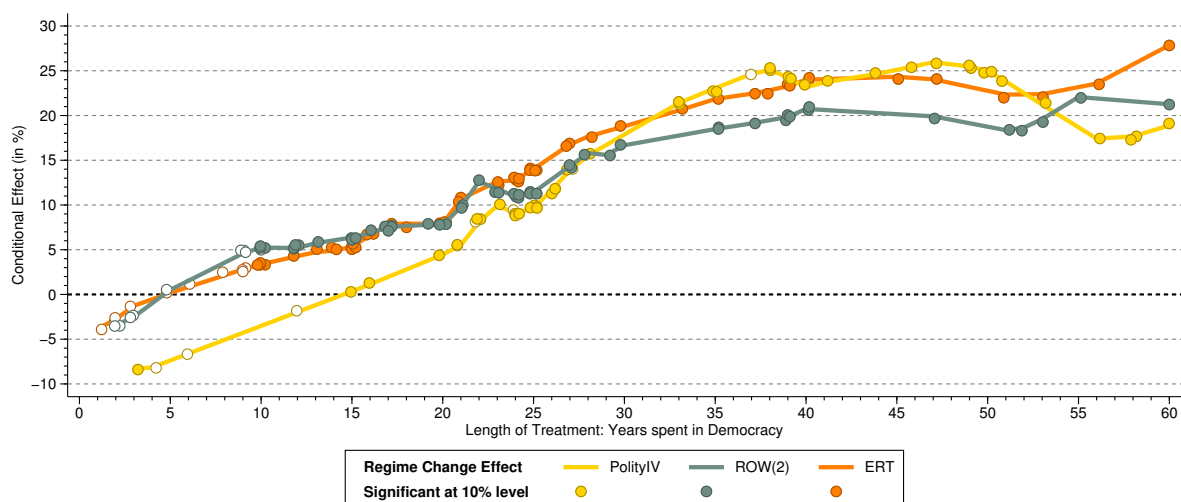
(a) Single vs Multiple Episodes



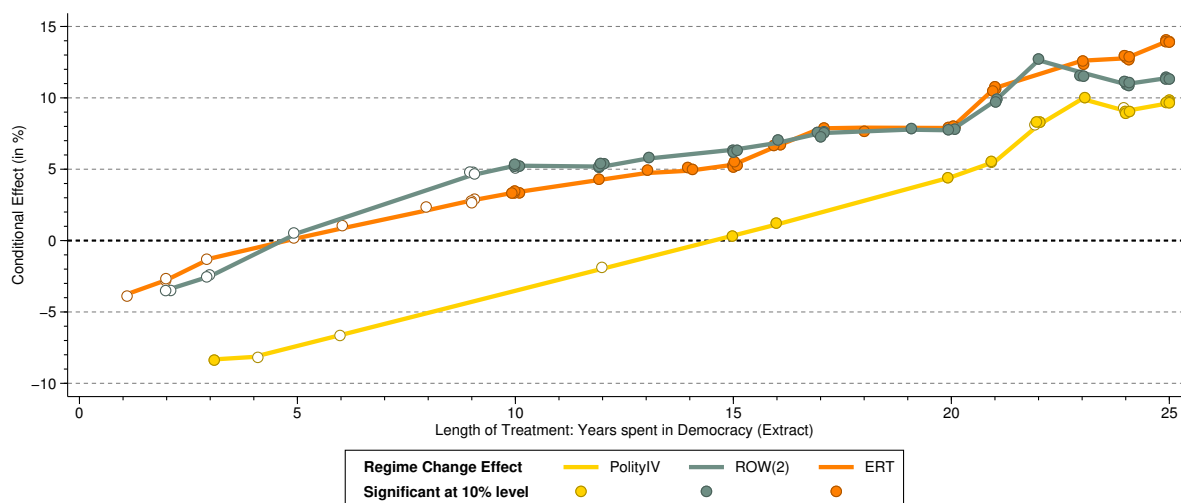
(b) Two vs More Episodes

Notes: These plots present the results from running line regressions of country-specific coefficients on the democracy dummy, derived from Single and Double PCDID regressions (these are the 'Conditional ERT' estimates for the latter). In the upper panel we split the sample into those countries which only experienced one democratisation episode ($N = 25$) and those which experienced several ($N = 37$), in the lower panel we further split the latter into those with 2 ($N = 21$) and with 3-5 episodes ($N = 16$), respectively. The Double PCDID estimates are still conditioned on the number episodes experienced for the latter group.

Figure B-3: Democracy and Economic Development — Plain Vanilla Single PCDID



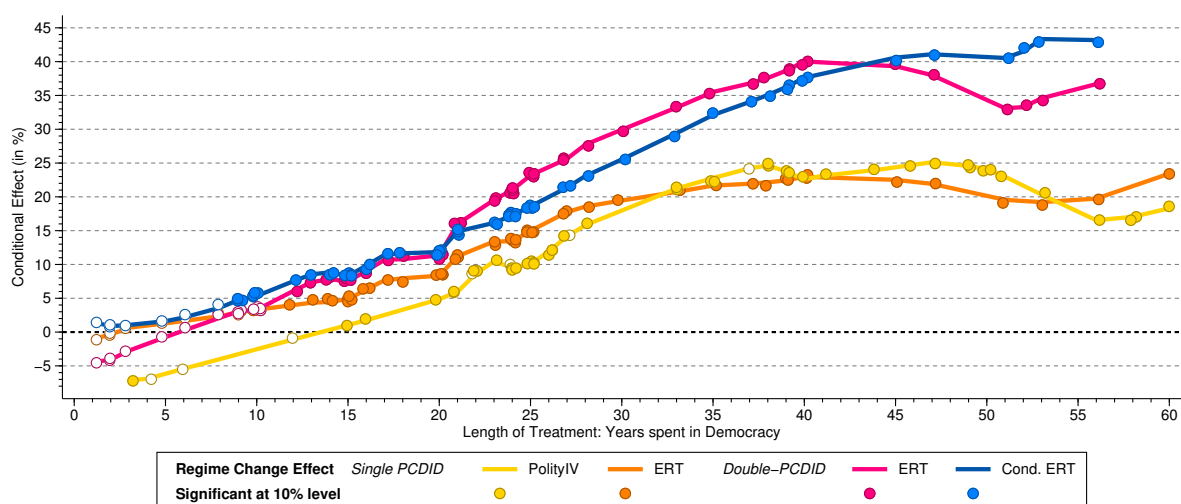
(a) Full Results



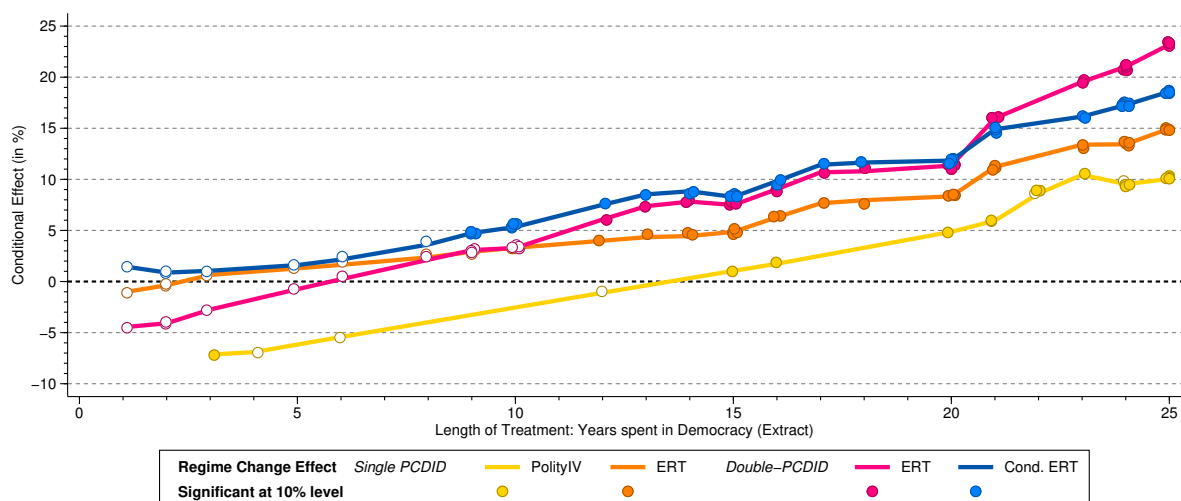
(b) Zooming in on the first 25 years in Democracy

Notes: The plots present the results from running line regressions of country-specific coefficients on the democracy (ERT) dummy, derived from Single PCDID regressions which do not include additional covariates (population growth, export-trade-ratio). These regressions condition on (i) the country series start year, and (ii) the number of times a country experienced regime change. In Panel (a) we present the full results, in Panel (b) we focus on the first 25 years of countries in democracy.

Figure B-4: Democracy and Economic Development — Plain Vanilla Double PCDID



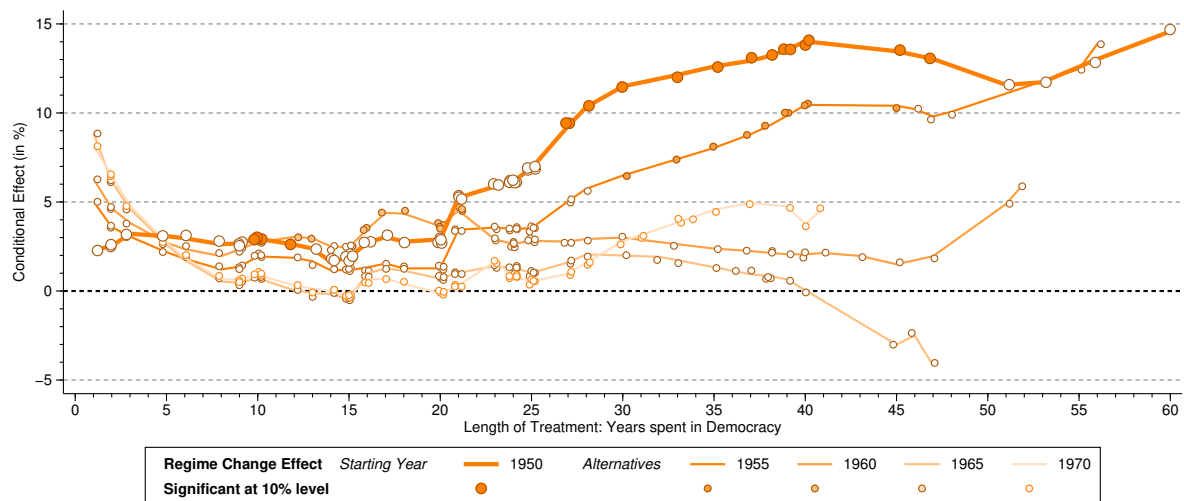
(a) Full Results



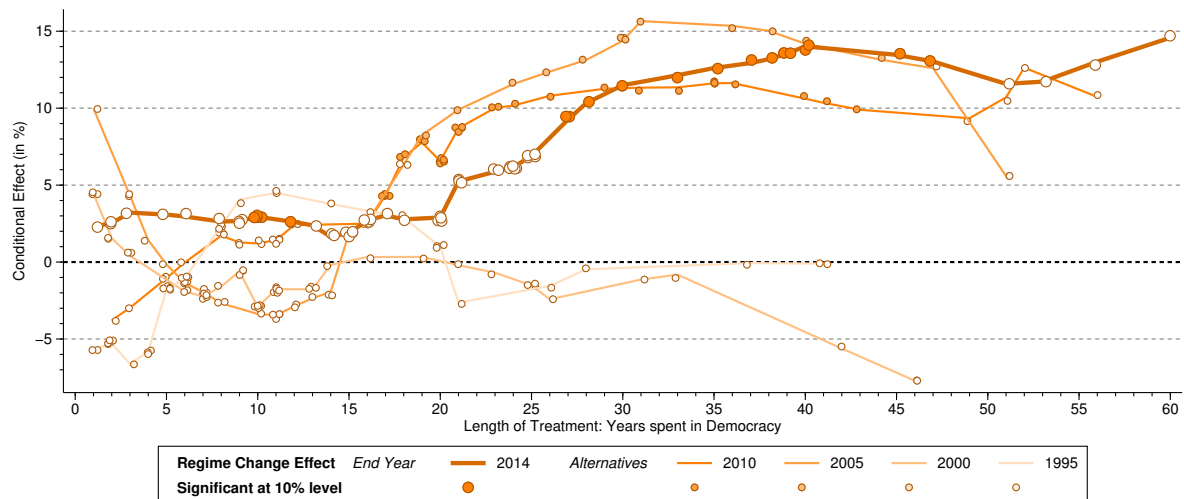
(b) Zooming in on the first 25 years in Democracy

Notes: The plots present the results from running line regressions of country-specific coefficients on the democracy (ERT) dummy, derived from Double PCDID regressions which do not include additional covariates (population growth, export/trade ratio). These regressions condition on (i) the country series start year, and (ii) the number of times a country experienced regime change. In Panel (a) we present the full results, in Panel (b) we focus on the first 25 years of countries in democracy.

Figure B-5: Democracy and Economic Development — Robustness Check for Single PCDID



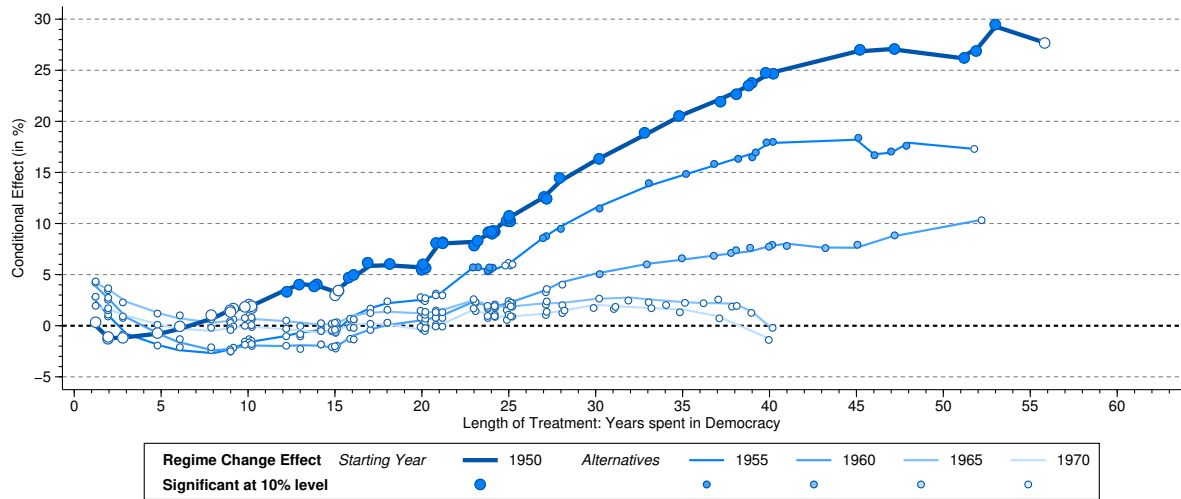
(a) Reducing the sample: different start years



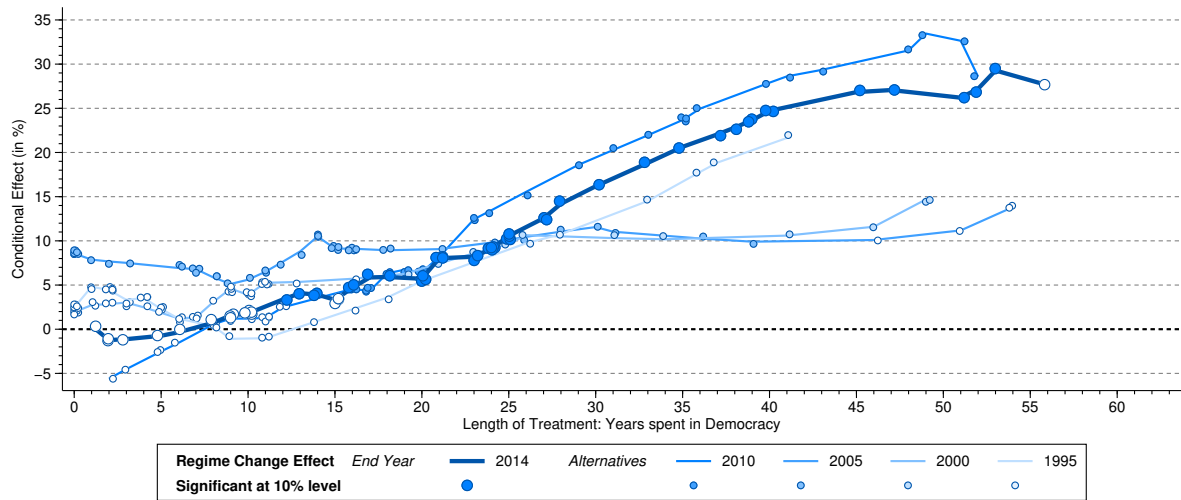
(b) Reducing the sample: different end years

Notes: The plots present the results from running line regressions of country-specific coefficients on the democracy (ERT) dummy, derived from Single PCDID estimates. These regressions condition on (i) the country series start year, and (ii) the number of times a country experienced regime change. In Panel (a) we vary the start year, in Panel (b) the end year of the full 1950-2014 panel.

Figure B-6: Democracy and Economic Development — Robustness Check for Double PCDDID



(a) Reducing the sample: different start years



(b) Reducing the sample: different end years

Notes: The plots present the results from running line regressions of country-specific coefficients on the democracy (ERT) dummy, derived from Double PCDDID estimates which account for selection into a democratisation episode and separately into democratic change, on the number of years spent in democracy. The running line regressions condition on (i) the country series start year, (ii) the number of times a country experienced regime change, (iii) the country estimate for a democratisation episode, (iv) the years spent in democratisation episodes, and (v) the number of democratisation episodes. In Panel (a) we vary the start year, in Panel (b) the end year of the sample. Note that the sample reductions substantially curtail the treated and control sample sizes. For the start years ('control 1' refers to the episodes counterfactual, 'control 2' to the regime change counterfactual): 1955 -6% treated, -4% control 1; 1960 -12% treated, -10% control 2; 1965 -20% treated, -17% control 2; and 1970 -27% treated, -24% control 2. For the end years: 2010 -7% treated, -9% control 1; 2005 -15% treated, -21% control 1; 2000 -23% treated, -33% control 1; and 1995 -32% treated, -44% control 1.

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 the construction of the episodes and regime change data we adopt in the main part of the paper) up to five years before and after the event/crisis.

In more detail, we estimate country fixed effects models separately for each variable k (growth, polyarchy) and event type:

$$y_{it}^k = \alpha_i^k + \sum_{s=-5}^5 \beta_{\tau+s}^k \delta_{i,\tau+s} + \varepsilon_{it}^k, \quad (8)$$

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. The trajectories of the event analyses for these two sets of countries are presented 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 did experience a crisis/oil discovery 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*.

For financial crises (banking crises and currency crises) we start with the data collated by Carmen Reinhart (with her coauthors Ken Rogoff, Christoph Trebesch, and Vincent Reinhart), available at the Behavioral Finance and Financial Stability Project at Harvard Business School — this is the expanded [Reinhart & Rogoff \(2009, RR\)](#) database. We add to this the data collated by [Laeven & Valencia \(2020, LL\)](#) which covers many developing economies not covered by RR — 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 (these years are typically omitted in early warning system models with multiple regressors but not in event analyses).

For natural disasters we use the EM-DAT database: EM-DAT, CRED/UCLouvain, Brussels, Belgium – www.emdat.be (D. Guha-Sapir) 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.

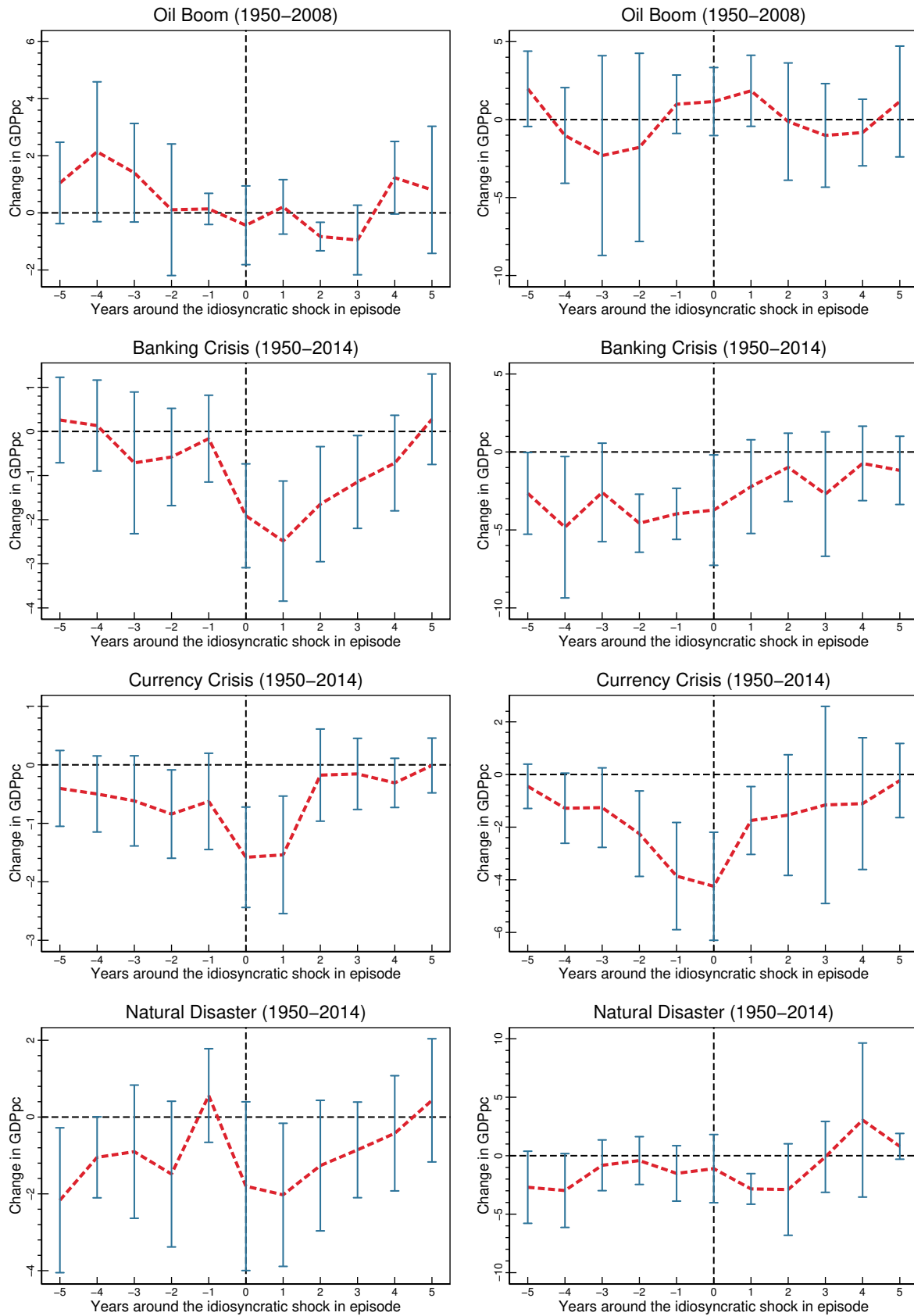
The event analysis plots for per capita GDP growth and the annual change in polyarchy are presented in Figures C-1 and C-2, respectively. Timings differ at times minimally, but the patterns of sign and statistical significance of the effects on growth and change in polyarchy between the treated and control samples are in general closely matched.

Table C-1: Sample Makeup: Event Analysis samples

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

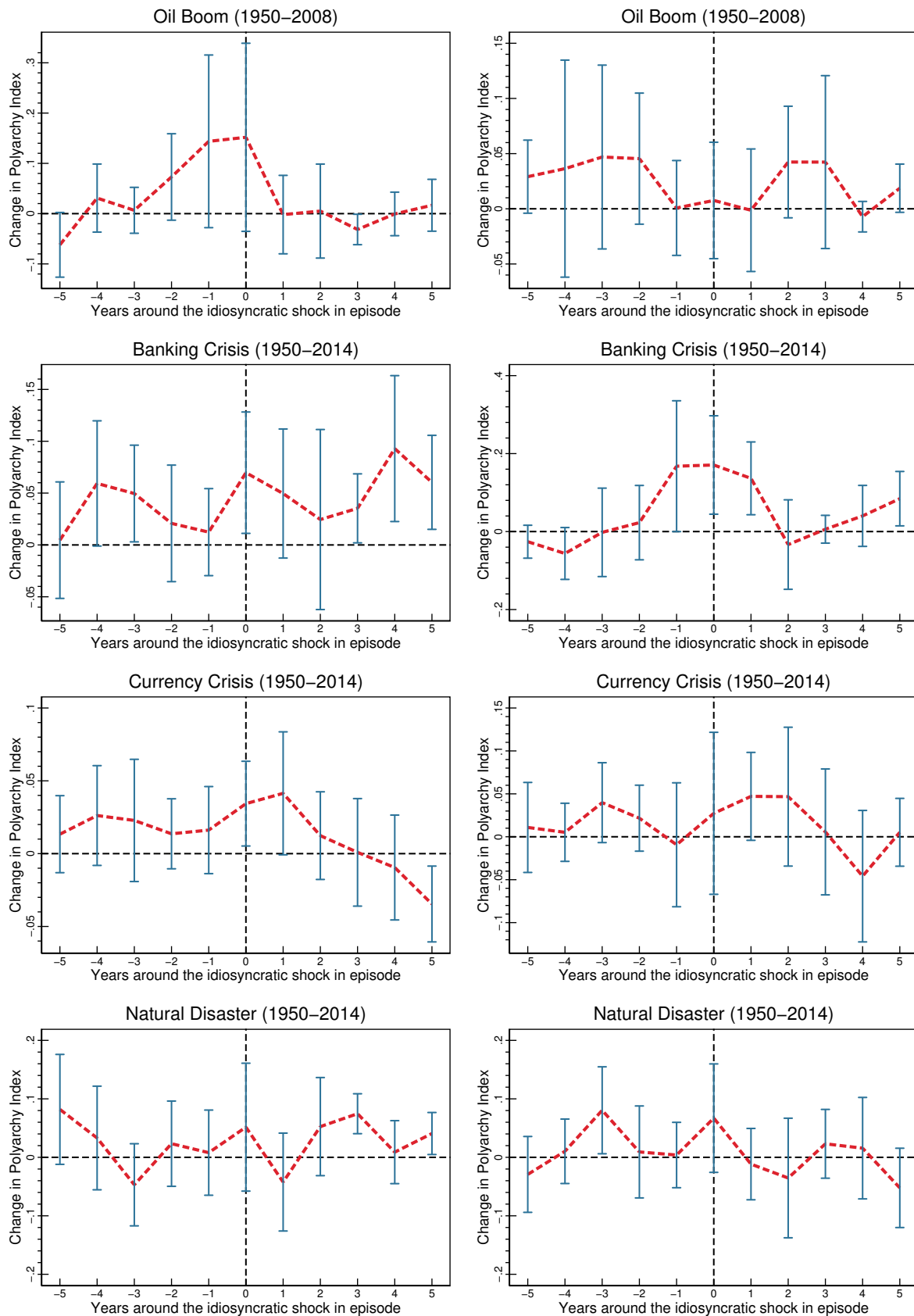
Notes: This table provides details on the crisis/event count in the treated and control groups for the episode-regime change event analysis. Countries are included in the respective sample if they experienced an event/crisis during a democratisation episode. The full treated (control) sample (analysed in Figure 3 of the maintext) contains 62 (43) countries and 3,660 (2,472) observations.

Figure C-1: Idiosyncratic Shocks in Treatment (left) and Control (right) Groups: Growth



Notes: We present the results from event analyses for the GDP pc growth variable and the event as indicated. These are within-country estimates with standard errors clustered at the country-level. The vertical bars are the 90% confidence intervals.

Figure C-2: Idiosyncratic Shocks in Treatment (left) and Control (right) Groups: Change in Polyarchy



Notes: We carry out the same event analysis as in Figure C-1 but adopt the change in the polyarchy index as the dependent variable.