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The economic consequences of democratic backsliding: Evidence from US states

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Abstract

Recent research has demonstrated that US states are bellwethers of national institutional decline, acting as 'laboratories of autocratisation' through voter repression and gerrymandering. We provide the first evidence on the economic consequences of sub-national democratic backsliding in the United States. We find that backsliding episodes during 2000-2023 do not systematically lead to a reduction in per capita income but do cause an increase in inequality and impoverishment. Innovation efforts (business R&D expenditure) and outputs (patenting) contract substantially, undermining the endogenous growth engine of the economy. International exports are unaffected, which suggests that foreign accountability operates through national-level institutions rather than sub-national ones.

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Non-Technical Summary

Democracy is in retreat globally. Over the past decade, more countries have shifted from democracy to autocracy than the reverse, and by 2023, the average global citizen experienced levels of democracy comparable to those in 1985. This reversal is not limited to developing nations. Emerging economies like Hungary, Turkey, and Brazil are frequently cited as examples of democratic backsliding—where democratic institutions erode gradually rather than collapse outright. Alarmingly, recent developments in the United States suggest that even mature democracies are not immune.

Since the inauguration of the Second Trump Administration in January 2025, observers have noted a marked increase in executive actions that undermine democratic institutions—targeting courts, universities, and civil rights protections. However, the health of US democracy is arguably shaped much more by the actions of individual state governments. In the US federal system, states hold significant authority over elections, districting, and policing. Practices such as gerrymandering, voter suppression, and felon disenfranchisement have led some scholars to describe states as "laboratories of authoritarianism."

Recent research by Jacob Grumbach has made it possible to systematically track democratic institutions at the US state level. His State Democracy Index (SDI) covers all 50 states from 2000 to 2023, measuring electoral integrity, suffrage, and gerrymandering. Grumbach's findings reveal that state governments have often led the way in democratic backsliding, driven largely by national partisan dynamics: Republican control of both chambers in a state legislature is the strongest predictor of institutional decline.

Against this backdrop, our study asks: **What are the economic consequences of democratic backsliding in US states?** While the Trump administration claims strong economic performance, isolating the effects of federal policy is difficult. Instead, we leverage the variation in democratic quality across states over time to assess how institutional deterioration affects economic outcomes in the US context.

Using Grumbach's SDI, we identify episodes of democratic backsliding in each state, following a methodology similar to the V-Dem Episodes of Regime Transformation dataset. We focus on sustained periods of decline rather than short-term fluctuations, which may reflect normal political dynamics. Our empirical strategy adopts a treatment effects model, using binary 'treatment' indicators to mark backsliding episodes.

We examine five key dimensions of economic performance:

1. Average income: GDP per capita and GDP per worker.

2. Income distribution: Median household income and within-state top income share.

3. Poverty and deprivation: Poverty rates, headcounts, and health insurance coverage.

4. Innovation: Business R&D spending and patent grants.

5. International exports: State-level export flows to foreign markets.

Our findings show a consistent negative relationship between the duration of backsliding episodes and state GDP per capita, though this result is sensitive to alternative model specifications. More robustly, we find that backsliding increases income inequality and poverty: while average income may not decline significantly, the number of people living in poverty rises, and economic disparities widen.

Our results for innovation outcomes are particularly concerning. States experiencing democratic backsliding see significant reductions in private R&D investment and patenting activity. These trends suggest that institutional decline undermines the drivers of long-term economic growth, such as technological advancement and entrepreneurship.

Interestingly, we find no evidence that sub-national backsliding affects international export flows. This contrasts with country-level studies, which show that democratic erosion can lead to trade penalties from democratic partners. This suggests that foreign trading partners primarily assess risk through national-level institutions such as federal courts and overarching rule-of-law guarantees, effectively insulating backsliding US states from external economic penalties.

Our study makes two key contributions. First, we introduce a new dataset of democratic backsliding episodes across all 50 US states, enabling systematic subnational analysis. Second, we provide the first empirical evidence of the economic costs of democratic backsliding within a federal democracy. These findings underscore that institutional decline is not just a normative concern – it carries tangible economic costs.

The Economic Consequences of Democratic Backsliding: Evidence from U.S. States*

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Abstract

Recent research has demonstrated that U.S. states are bellwethers of national institutional decline, acting as 'laboratories of autocratisation' through voter repression and gerrymandering. We provide first evidence on the economic consequences of subnational democratic backsliding in the United States. We find that backsliding episodes during 2000-2023 do not systematically lead to a reduction in per capita income, but do cause an increase in inequality and impoverishment. Innovation efforts (business R&D expenditure) and outputs (patenting) contract substantially, undermining the endogenous growth engine of the economy. International exports are unaffected, suggesting that foreign accountability operates through national-level institutions rather than subnational ones.

Keywords: democratic backsliding, difference-in-differences, heterogeneity, interactive fixed effects, trade gravity model

JEL codes: P16, F13, F14, C23

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1 Introduction

"My recent election is a mandate to completely and totally reverse a horrible betrayal, and all of these many betrayals that have taken place and to give the people back their faith, their wealth, their democracy, and indeed their freedom."
President Donald J. Trump, Inaugural Presidential Speech, January 21, 2025

"Congratulations America! The Polity Project has downgraded its score for the USA from +8... to 0."
Monty G. Marshall, Center for Systemic Peace, February 12, 2025 (posted on LinkedIn)

"Americans trust in President Trump's America First economic agenda that continues to prove the so-called 'experts' wrong. President Trump has reduced America's reliance on foreign products, boosted investment in the US, and created thousands of jobs — delivering on his promise to Make America Wealthy Again."
News Release (excerpt), Press Secretary Karoline Leavitt, The White House, July 30, 2025

Democracy is in retreat around the world. Over the past decade, the number of countries reverting from democracy to autocracy has outpaced the number of democratising nations, and the level of democracy experienced by the average citizen of the world in 2023 has reverted to that of 1985, before the collapse of the Eastern Bloc and the ensuing 'Third Wave of Democratisation' (Nord et al., 2024). Many democratic countries are also said to have experienced persistent deterioration of their democratic institutions, a process referred to as 'democratic backsliding' (Edgell et al., 2020).[1] While democratic collapse in the twenty-first century is still limited to developing countries, a number of emerging economies (such as Hungary, Turkey, and Brazil) are regularly used as case studies for backsliding 'episodes'. Most recently, democratic backsliding has also become a concern in the world's largest economy: since the inauguration of the Second Trump Administration, the authoritarian (ab)use of presidential executive orders to attack the courts, threaten law firms and universities, end birthright citizenship, and undermine government agencies and democratic institutions more generally (see the Trump Administration Tracker, `https://cohen.house.gov`), have made the United States the new poster child for democratic backsliding.

Yet, the health of democracy in the U.S. is arguably shaped to a much lesser extent by the actions of the President and their administration, than those of the individual state executives: in the decentralized institutional system of U.S. federalism, it is the individual state governments which hold "the authority to administer elections, draw electoral districts, and exert police power" (Grumbach, 2023, 967). Hence, individual states decide who can and who cannot participate in American politics. Gerrymandering, voter repression, felon disenfranchisement, and the authoritarian use of police powers, among other practices, have been argued to render states into 'laboratories of authoritarianism' (Levitsky and Ziblatt, 2019). Importantly, it is now possible to track the quality of U.S. democracy at the sub-national

[1] Bermeo (2016, 5) defines backsliding as a "state-led debilitation or elimination of any of the political institutions that sustain an existing democracy".

level, thanks to the recent work by Grumbach (2023), who uses a large range of measures of electoral democratic quality covering inclusive suffrage and gerrymandering to create an index of U.S. state-level democracy for the 2000-2023 period.[2] His subsequent analysis reveals that "state governments have been leaders in democratic backsliding" (ibid: 967). But state powers do not operate in isolation, and should instead be viewed in the context of the power-struggle between the Democratic and Republican parties: national partisan dynamics are the most significant driver of democratic change in the United States and Republican 'capture' of a state, i.e. holding both state senate and house, is the dominant factor in explaining democratic backsliding (Grumbach, 2023).[3]

In this paper, we exploit Grumbach's (2023) State Democracy Index (SDI) to ask about the *economic consequences of democratic backsliding in U.S. states.* The present Trump administration loudly postulates its economic prowess (see above quote), yet the quantification of real economy effects *caused* by U.S. national policies and executive orders is fraught with difficulty, not least because the myriad of government 'actions' makes it difficult to get a handle on what could be seen as 'treatment' or 'intervention' in an empirical model. Grumbach's efforts offer us a wealth of heterogeneous democratic dynamics in fifty states over a quarter-century, which combined with state-level data on income, inequality, impoverishment, innovation, and international exports available from the U.S. BEA, Census Bureau, and NSF, among others, allows us to address our research question.

Conceptual development The theoretical mechanism linking democratic quality to economic performance operates through electoral integrity and its effect on government accountability. Global evidence suggests that the integrity of elections constitutes a central institutional determinant of long-run economic outcomes (Acemoglu et al., 2019; Boese-Schlosser and Eberhardt, 2025). When elections are free and competitive, incumbents face credible threats of removal, which incentivizes them to provide broad-based public goods, maintain sound fiscal policy, and foster and invest in long-term productivity-enhancing areas such as innovation, human capital, and infrastructure (Besley and Kudamatsu, 2006; Dahlum and Knutsen, 2017; Knutsen, 2021). In contrast, when electoral integrity is eroded—through manipulation, suppression, or systemic bias—the accountability mechanism weakens (Schedler, 2002; Gehlbach and Simpser, 2015; Sato et al., 2022).

As accountability declines, governments face fewer constraints on rent-seeking and have diminishing incentives to pursue policies with diffuse, delayed benefits (Persson and Tabellini, 2002). Instead, policymaking and public spending become increasingly targeted toward narrow constituencies or coercive apparatuses that sustain political control first and consider economic

[2]We provide a detailed overview of what this index includes and how it can be interpreted in Appendix A.

[3]Our backsliding episodes correlate with Republican state capture (correlation coefficients of around 0.3; episode onset and Republican state capture have correlation coefficients of around 0.05.) but the economic patterns we reveal are not primarily driven by who is in power in a state. We document these patterns but focus our formal empirical analysis on the economic effects of backsliding, rather than political ideology.

prosperity second. This shift in focus and spending composition yet further undermines investment in human capital and public goods that underpin growth.[4] In short, electoral erosion sets off a causal chain in which weakened accountability drives cuts to public investment and policymaking, losing sight of the economic welfare of the average citizen, ultimately resulting in measurable, long-term economic decline.

Democratic backsliding can also interact with trade, both as a potential transmission channel and as an external accountability mechanism. Democratic institutions provide the legal and political stability necessary for sustaining trade relationships by ensuring predictable enforcement of contracts, transparency in regulation, and protection against arbitrary policy shifts (Levchenko, 2007; Nunn, 2007; Yu, 2010; Araujo et al., 2016; Sheng and Yang, 2016; Zissimos, 2017). When U.S. states experience backsliding, they may become less attractive to international trading partners due to weakened rule of law, increased corruption, or politicized economic policymaking. These shifts can hinder economic openness and reduce export flows.

Economic Consequences We approach our central research question by examining the effects of democratic backsliding on measures of economic prosperity and the broader implications for long-term economic potential. We investigate five dimensions of economic consequences at the state level: (i) average income; (ii) income distribution; (iii) deprivation (iv) innovative capacity; and (v) international export flows.

Measures of per capita or per worker income are natural candidates for our analysis, while the investigation of income inequality and impoverishment seeks to go beyond the *average* to understand whether certain parts of society are likely to be more affected by backsliding than others. This line of reasoning would fit the analysis of any economy, regardless of its level of development. However, the mechanisms bringing about economic prosperity differs between developing and advanced economies: the latter are characterised by an endogenous growth process, which focuses our attention on proxies for innovation efforts (R&D) and outputs (patents). Finally, the study of state international exports can offer insights into whether the external accountability mechanism detectable in changing trade flow patterns between *national* economies following democratic 'retreat' (as documented in Boese-Schlosser et al., 2025) also operates at the *sub-national* level.

Backsliding Episodes We dichotomise the Grumbach SDI into three variants of 'episodes' of democratic backsliding, following the practices used in the construction of the V-Dem *Episodes of Regime Transformation* dataset (Edgell et al., 2020). Our episodes cover democratic backsliding in all fifty U.S. states (excluding the District of Columbia) over the 2000-2023 period. To demonstrate the robustness of our findings, our three alternative variants of backsliding

[4]The Republican war against 'woke' and 'extreme-left' DEI (diversity, equity, and integration) practices in public institutions and beyond is a point in case: existing research provides strong empirical evidence of the significant benefits of diversity for innovation (e.g. Moser et al., 2014; Nielsen et al., 2017; Moser et al., 2025).

episodes differ by the magnitude of institutional decline required to classify an episode as backsliding: we adopt a *broad* variant that includes comparatively small declines, an *intermediate* variant requiring larger declines, and a *narrow* variant capturing only the severe cases. The resulting measures identify backsliding in 33, 22, and 18 states, respectively.

There are two primary motivations for our modelling choice of backsliding episodes: first, democratic change is rarely binary or immediate; instead, it unfolds gradually and is often interspersed with minor year-to-year fluctuations, some of which may reflect a healthy, dynamic political process rather than decline. An episode-based approach can pinpoint periods of substantial and sustained democratic backsliding from short-term variations. This mirrors recent practice in the study of regime transformation in comparative political science, where processes of democratization and autocratization are conceptualized as clustered, path-dependent episodes rather than isolated events (e.g., Boese et al., 2023; Edgell et al., 2022; Maerz et al., 2024; Wilson et al., 2023).[5] Applying this logic to subnational democratic backsliding allows us to more precisely assess its trajectory and consequences within the United States. Second, our mode of empirical inference follows the recent tradition in the literature on democracy and growth (Papaioannou and Siourounis, 2008; Acemoglu et al., 2019; Boese-Schlosser and Eberhardt, 2024) by adopting a *treatment effects* model (Chan and Kwok, 2022), which uses binary 'treatment' variables.

Empirical implementation We employ the Principal Component Difference-in-Differences (PCDID) estimator developed by Chan and Kwok (2022) to capture the economic consequences of backsliding. The PCDID accounts for heterogeneous treatment effects and non-parallel pre-trends by using common factor proxies across states to capture time-varying unobserved heterogeneity and estimating the factor-augmented treatment regression separately in each state. The common factors are extracted from control states, i.e. those which never experienced any backsliding episodes, and the inclusion of these proxies in the state-specific treatment regressions enables us, under reasonable and testable assumptions, to identify the causal impact of democratic backsliding on economic outcomes.[6] This approach further enables us to map state-specific treatment effects to the number of years spent in a backsliding episode, while accounting for the number of episodes experienced (following the practice introduced in Boese-Schlosser and Eberhardt, 2024). In robustness checks we account for the distorting impact of the Covid pandemic and further devise a specification that can capture the economic effect of backsliding episodes while allowing for more flexibility in post-episode trends (e.g. a 'bounce-back' effect or a temporal spillover of the episode).

[5] Recent research in economics (Boese-Schlosser and Eberhardt, 2024) has also shown that treating democratization as the successful culmination of an episode rather than a single point-in-time transition reveals stronger and more enduring effects on economic growth.

[6] Recent studies have adopted this implementation to study the effect of democratic regime change or collapse on economic prosperity (Eberhardt, 2022; Boese-Schlosser and Eberhardt, 2024, 2025) and the economic implications of 'excessive' financial deepening (Cho et al., 2025).

Main Findings Our empirical results suggest a greater economic loss in terms of state GDP per capita with an increasing number of years spent in a backsliding episode, with stronger statistical evidence as we move towards more restrictive (narrower) definitions of backsliding. In contrast to our other main findings, the per capita income effect is not robust to the flexible post-episode treatment we consider in a robustness check. Backsliding, however, robustly increases income inequality within states, measured using top-income shares, as well as leading to a higher incidence of poverty. Hence, the average American may not directly suffer economically from democratic backsliding in their state during the time frame studied; however we can detect diverging incomes and a rise in the number of impoverished people.

A second set of outcomes related to innovation investment and outputs provides strong evidence that private firms in treated states substantially reduce their R&D expenditure, while patenting counts of treated states decline as well. These results indicate that democratic backsliding undermines the endogenous growth engine of the United States and hence jeopardizes future economic progress.

Finally, shifting to a heterogeneous gravity model of export flows from U.S. states to international destination countries, we find no statistical evidence for a detrimental effect of backsliding. Contrasted with ongoing research analysing democratic backsliding in the context of export flows between countries (Boese-Schlosser et al., 2025), this implies that *sub-national* backsliding does not result in the 'punishment' effect by democratic trading partners observed in the country-level analysis.

Related Literature and Contributions While the positive economic effects of democratic regime change are well established (Madsen et al., 2015; Acemoglu et al., 2019; Knutsen, 2021; Eberhardt, 2022; Boese-Schlosser and Eberhardt, 2024) research on the consequences of democratic decline is relatively sparse. Recent research primarily covering developing countries finds evidence for an 'autocratic loss' (Boese-Schlosser and Eberhardt, 2025), i.e. a measurable decline in a country's income per capita after democratic breakdown, during the current wave of autocratization. Work by Blattman et al. (2025) suggests that an 'autocratic penalty' is concentrated in countries with 'personalist' rule, where power is captured by an individual or a small elite.

The above studies adopt a strict distinction between democracy and autocracy which is too crude when considering the political economy of many advanced economies, virtually all of which have been (and remain) democratic since the 1990s, yet collectively experienced a noticeable deterioration in democratic institutional quality since the late 2000s (Nord et al., 2024). A large literature in political science is devoted to defining these 'democratic backsliding' episodes in cross-country data (e.g. Edgell et al., 2020) and studying the characteristics and determinants of such episodes of institutional deterioration (e.g. Bermeo, 2016; Waldner and Lust, 2018; Levitsky and Ziblatt, 2019; Lührmann and Lindberg, 2019; Grillo and Prato, 2023; Grillo et al., 2024; Riedl et al., 2024). This strand of research is not without controversy, including regarding the very *existence* of backsliding episodes, with some suggesting quantitative evidence for episodic autocratisation is due to the biased subjective measurement of democratic institutions and their temporal dynamics (see Little and Meng 2024a; 2024b and Knutsen et al., 2024).

A related literature in economics and political science deals with the rise of populism (see the recent survey by Guriev and Papaioannou, 2022). One example of the emerging research on the economic consequences of populism is the work by Funke et al. (2023), who use data for sixty major economies and the synthetic control methodology to show that populism slows economic progress considerably (while leaving economic inequality unchanged). The analysis by Born et al. (2019) uses the same methodology to find no differential effect of the first Trump administration on the U.S. economy. Other studies linking populism in a single country (Marzetti et al., 2022; Woo-Mora, 2025) or a small number of countries (Absher et al., 2020) to economic outcomes using synthetic control find detrimental effects of populist leadership.

While the literature in political science offers a large number of country case studies, there is comparatively limited quantitative analysis of *sub-national* democracy and its dynamics. Recent work by Michel (2024) studies sub-national elections and opposition control in 371 states of 18 democratic Latin American countries, finding a positive effect for the 'health' of national democracy when sub-national control of the opposition increases. The study by Grumbach (2023) on U.S. states, which is central to our work, in contrast labels states as 'laboratories for democratic backsliding' and finds a strong link between state capture by the Republican party and a deterioration in the state democracy index he develops. To the best of our knowledge, there is only one empirical study linking sub-national democracy to economic prosperity. Iddawela et al. (2021) construct sub-national measures of government quality from survey data in 22 African countries and find a positive causal link between regional democratic change and regional economic prosperity, even when accounting for national democracy levels.

This paper makes two important contributions to the study of democratic backsliding and its consequences: first, we introduce a novel dataset of backsliding episodes across all fifty U.S. states from 2000 to 2023, derived from the Grumbach (2023) SDI. By offering three variants of backsliding episodes, our dataset offers a built-in robustness check to counter concerns over definitional arbitrariness. Second, we provide the first empirical assessment of the economic effects of subnational democratic backsliding. Our findings suggest that democratic backsliding is not only normatively concerning but also economically costly, with consistent evidence of increasing deprivation, widening inequality, and reduced innovation.

The remainder of this paper is structured as follows. In Section 2 we introduce the data, devise the backsliding episodes, and provide descriptive analysis. Our empirical methodology is developed in Section 3, followed by the discussion of the results in Section 4. Section 5 concludes.

2 Data and Transformations

2.1 State-level Democratic Backsliding

Data Source We use the updated state-level democracy data from Grumbach (2023, State Democracy Index, v. 2.0), covering all states (but not the District of Columbia) over 2000-2023.[7] The SDI is constructed from 51 measures and indicators (covering electoral democratic quality related to information on polling day wait times, voter registration, felon disenfranchisment and proxies for gerrymandering, among others) using Bayesian factor analysis, intended to capture a latent measure of democratic performance. Conceptually, this brings the state-level indices close to the electoral democracy index of the Varieties of Democracy (V-Dem) Project (building on the theories of Dahl (1971, 2000) in political science and Schumpeter (1942) in economics), focusing most attention on free and fair elections, and inclusive suffrage. See Appendix A for some thoughts on the purpose and applicability of the SDI compared with cross-national indices such as those compiled by the V-Dem project.

State Democracy Index: Transformations and Descriptives We rescale the Grumbach index to lie between 0 and 1, mimicking the practice of V-Dem indices.[8] Panel (a) of Figure 1 presents the annual average (rescaled) Grumbach index alongside the V-Dem polyarchy index (Coppedge et al., 2025, v15) for the United States: the overall patterns speak to one of Grumbach's (2023, 967) main conclusions, namely that states have been "leaders in democratic backsliding in the U.S. in recent years", especially following the "Republican gains in state legislatures and governorships in the 2010 election" (968).

Panel (b) of the same Figure illustrates how the share of states with the 'worst' democratic institutions has expanded over time: we determine the threshold SDI values for the four quartiles of the entire distribution of the rescaled Grumbach index in 2000 (values reported on the right of the plot) and shade the quartiles in different colours. The distributional significance of the top quartile is largely unchanged, and the 3rd quartile has been squeezed marginally, in that in 2023 it captures only one fifth instead of a quarter of all states. The 2nd quartile has been squeezed the most, from one quarter in 2000 to around one tenth in 2023. These changes have all been to the 'benefit' of the bottom quartile: the share of states with low levels of institutional quality (below SDI 0.62) has expanded from 0.25 to over 0.40!

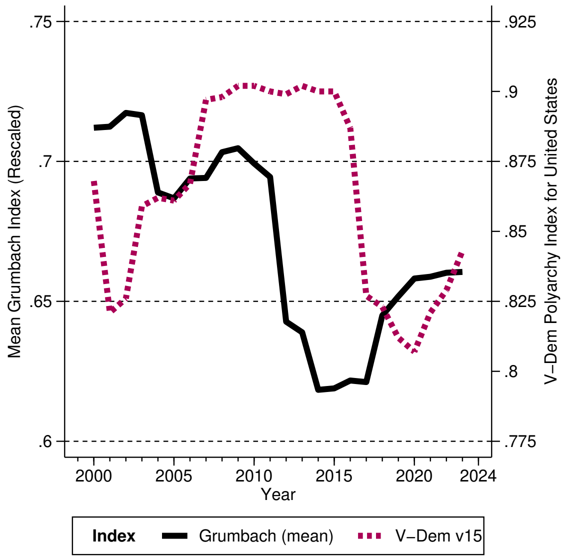
Panel (c) offers a geographical illustration of the rise and fall in state-level democratic institutions: we compute the total change in the rescaled Grumbach SDI between 2000 and 2023 and plot states in green shades if this change is positive and in red shades if it is

[7] The Grumbach data excludes the District of Columbia (DC), since DC has no voting representation. While it is possible to push the data back to cover earlier periods, the justification provided by Grumbach (2023) to focus on *contemporary* America and hence avoid challenges of data availability and temporal comparability of state variation in democracy, applies in the present study as well.

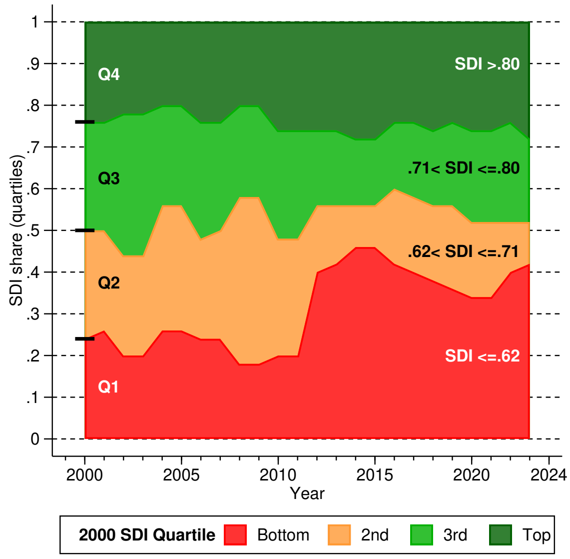
[8] We do so for ease of presentation; absolute *magnitudes* of an entry in the rescaled Grumbach index should not be compared to the V-Dem polyarchy or other indices, though patterns of variation are meaningful.

Figure 1: State Democracy Index (rescaled) — Descriptives

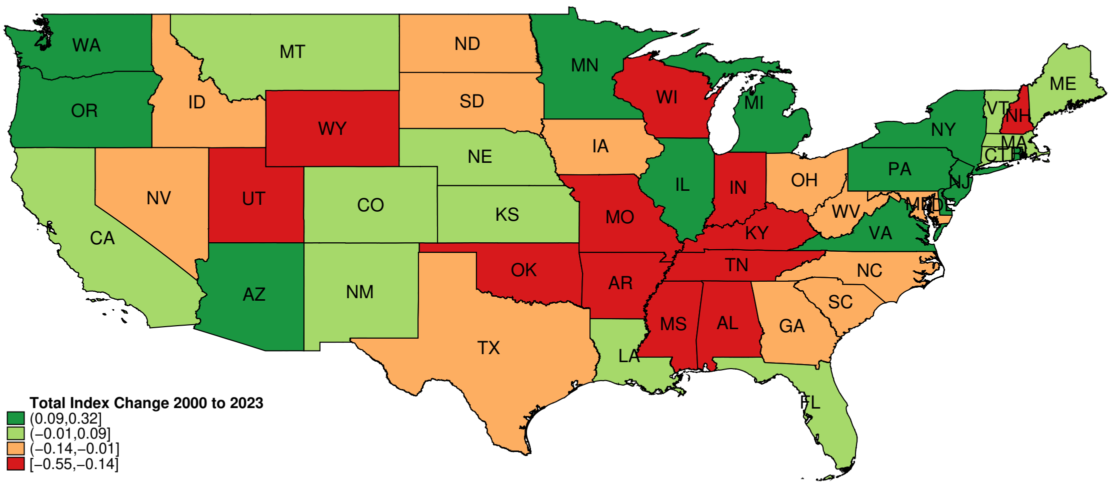
(a) Rescaled SDI (solid), V-Dem USA (dashed)



(b) Evolution of Rescaled SDI from 2000 Quartiles



(c) State-level Change in the Rescaled SDI 2000-2023

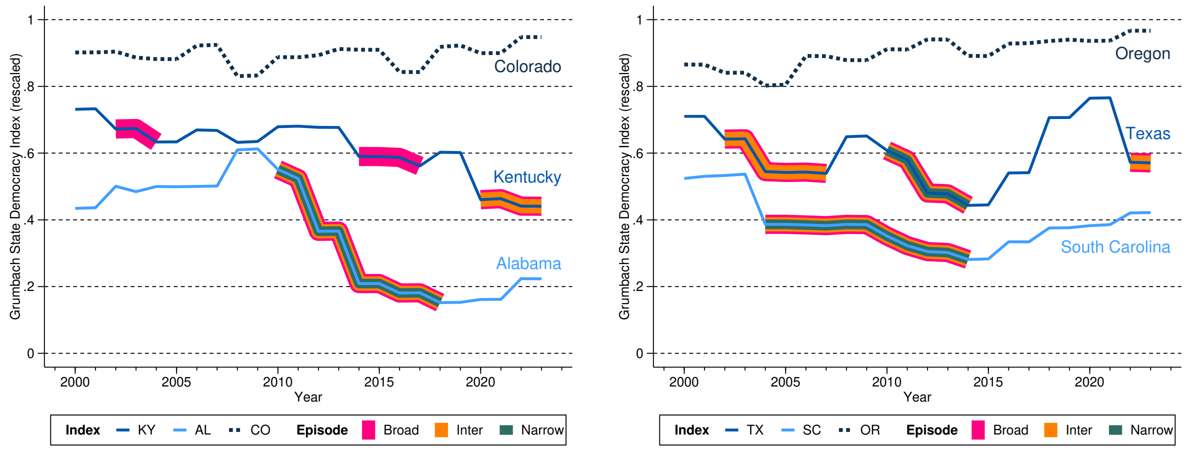


Notes: The black solid line in panel (a) represents the unweighted mean of the rescaled Grumbach index (SDI) across all 50 states in each year, the dark pink dashed line the V-Dem (v15) polyarchy index for the United States. Note that Freedom House has downgraded their 'political rights' measure for the U.S. from 1 to 2 (scale of 1-7, best to worst) since 2017 (survey edition 2018). Panel (b) studies how the distribution of the rescaled Grumbach index (SDI) changed over time: using four colours, we determine the four quartiles of the distribution in 2000 (black lines on the left, the SDI thresholds are indicated on the right) and then illustrate how these expanded and contracted over time. In panel (c) we compute the change in the rescaled Grumbach index over the 2000-2023 period by state and present a map of the contiguous U.S. states shaded by the quartiles of the distribution of changes (from large negative change in red to large positive change in dark green, with orange and light green for the intermediate categories with below and above zero total change) — see legend for categories. For ease of presentation, we exclude Alaska (light green) and Hawaii (orange).

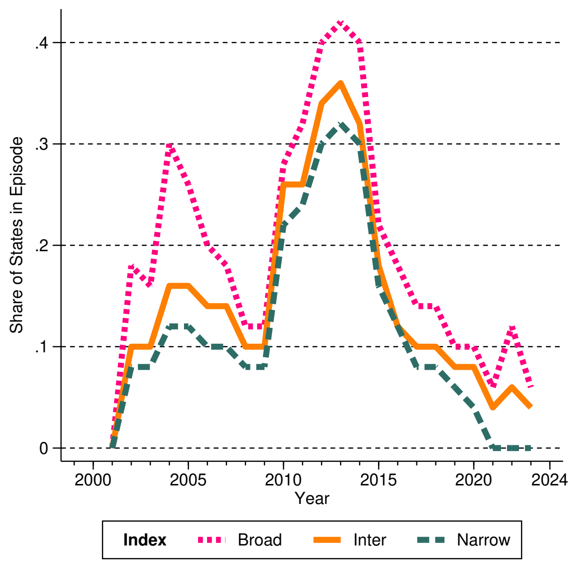
negative. States with the most substantive experience of democratic backsliding (shaded in red) are primarily concentrated where the Midwest meets the South, as well as further out West (Wyoming, Utah).[9]

Figure 2: Backsliding Episodes

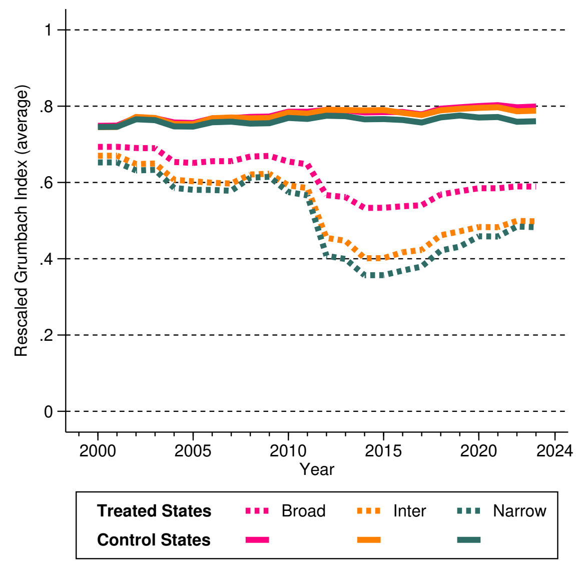
(a) Variants of Backsliding Episodes: Illustrations



(b) Share of States in a Backsliding Episode



(c) Average SDI in Treated and Control States



Notes: The plots in panel (a) illustrate alternative definitions of democratic backsliding in individual states. Plots for all states can be found in Appendix Figure B-2. In panel (b) we illustrate the share of states experiencing a backsliding episode in any given year for each of our three backsliding definitions. Panel (c) illustrates the average SDI evolution in control states that never experienced an episode (solid lines) and treated states that experienced one or more episodes (dashed lines) for the three definitions of backsliding, respectively.

Backsliding Episodes: Variants and Descriptives In developing three alternative variants of 'episodes' of democratic backsliding, we primarily rely on what we term the 'trigger' and

[9]For illustrative purposes we exclude Alaska (light green) and Hawaii (orange) from this map.

'total drop' conditions: first, for all three alternatives, we adopt a decline in the rescaled Grumbach index of -0.025 as the 'trigger' for potential episodes. This represents a substantial drop (the 18th percentile of the distribution of changes in the rescaled SDI), equivalent to half a standard deviation.[10] Second, potential episodes are promoted to actual episodes if they constitute a 'total drop' in the rescaled index for the entire episode of at least -0.1 ('broad' definition), -0.15 ('intermediate'), or -0.2 ('narrow'), equivalent to 2, 3 and 4 standard deviations. For practical implementation, we require some additional rules to help us determine how long an episode lasts: (a) an episode ends in the year before the first difference of the index turns positive (exceptions apply, see next point); (b) an episode can continue even if there are single *intermittent* years with positive changes, provided these changes are each smaller than $+0.01$;[11] (c) we allow for single-year episodes, subject to the 'total drop' condition.[12]

This procedure yields a 'broad' definition of backsliding for 33 states experiencing 46 episodes, an 'intermediate' definition for 22 states (27 episodes), and a 'narrow' definition for 18 states (20 episodes) — for all three definitions, the median time a (treated) state spends in one or more episodes is 7 years.[13] Respective control samples of states without backsliding episodes contain 17, 28, and 32 states. We always present our results for *all* states with backsliding episodes, but to underline that states experiencing repeated episodes do not distort our findings, we further provide results for only those states that experienced a *single* backsliding episode. For the latter, we observe 22 (instead of 33), 18 (27), and 16 (18) states, adopting the broad, intermediate, and narrow backsliding definitions, respectively. For more details, see Appendix Tables B-1 and B-2.

Panel (a) of Figure 2 provides some illustrations from our episodes data (plots for all states can be found in Appendix Figure B-2). These examples highlight the differences between our three definitions. For instance, in the left plot, Kentucky features two episodes around 2003 and 2015, which are only picked up by the 'broadest' definition of a backsliding episode (pink bands), whereas the episode around 2020 is picked up by the 'intermediate' definition as well (hence, pink and orange bands). The 'narrowest' definition does not pick up *any* episodes for

[10]The first difference of the rescaled Grumbach index has a mean and median of around 0, a standard deviation of 0.049, and an interquartile range (IQR) of 0.015. In comparison with the V-Dem ERT measure for autocratic episodes (Edgell et al., 2020), which adopts a trigger of -0.01 (the standard deviation and IQR of the change in the V-Dem polyarchy index between 2000 and 2023 are 0.36 and 0.01, respectively), our trigger measure is quite conservative. Nevertheless, this first step yields 202 *potential* episodes, experienced across 48 states in our sample (only Nebraska and Vermont never experienced an annual decline of this magnitude).

[11]The evolution of democratic quality in individual states seems to be characterised by fits and starts, when a less granular view of their evolution would suggest 'decline' or 'increase' over the period in question. We see our provision (b), along with the construction of episodes as a treatment variable in the first place (rather than the use of the state index as an independent variable), as a means to tackle any counter-intuitive idiosyncracies as well as the unavoidable uncertainties of producing democracy indices.

[12]We obtain identical episodes if we use the original SDI (range -3.39, 1.66) with 'trigger' 0.125 ($1/2$ sd) and a 'total drop' of at least 0.5, 0.75 and 1 (2, 3, and 4 sd, respectively).

[13]In our empirical analysis, we cannot separate individual episodes, only states, hence we relate the state backsliding treatment effect to the total number of years the state spent in episodes. We do condition our results on the number of episodes per state and also provide results for the subsample of states with just a single backsliding episode.

Kentucky (no teal-coloured bands). For Texas in the right plot, an early and late episode are only picked up by the 'broad' and 'intermediate' definitions, whereas the episode after 2010 is picked up by all three definitions. Oregon and Colorado do not have any episodes. Note that in the latter, the drop in 2008 measures -0.094 and that in 2016 -0.066—hence both are below the required minimum -0.1 'total drop' for even our broadest definition of backsliding. The case of Alabama also highlights the inclusion of single, intermittent years of positive index change as part of a backsliding episode: 2013, 2015, and 2017 all feature as periods of minimal positive change followed by large drops of -0.157, -0.029, and -0.029, respectively. See also South Carolina for similar minimally positive intermittent years in 2005 and 2008.

Panel (b) of Figure 2 charts the share of states experiencing an episode in any one year between 2001 and 2023. This illustrates the increase in backsliding following the 2010 election, with up to 40% of states experiencing an episode in 2013. Although the end of our sample period is characterised by an absence of episodes, it should be emphasised that the *average* SDI is around 8% lower in 2023 compared with 2002 (its peak). We can also see in panel (c) of Figure 2 that states which feature in our control samples on average marginally increased their SDI during the sample period (solid lines), whereas states in the treated samples (dashed lines) on average witnessed increasingly large declines as we narrow the definition of backsliding. The cessation of most or all backsliding activity after 2020, depending on our definition, is apparent in the plot in panel (b) of Figure 2. However, it should be pointed out that many states experiencing backsliding by then had settled at *permanently* lower levels of democratic quality: first and foremost Tennessee (drop in the rescaled Grumbach index of -83% between 2000 and 2023), Wisconsin (-68%), Alabama (-49%), Arkansas (-46%), and Indiana (-46%), but these are not isolated cases — see Appendix Table B-2.

While these statistics certainly do look dramatic, it bears reminding that our rescaled state democracy index *accentuates* any variation within and between U.S. states. A devil's advocate may suggest that these magnitudes are meaningless, that during this period democracy *levels* in the U.S. were relatively high, and may point out that the variation implied was contained within relatively narrow ranges of cross-country democracy indices, for instance, as we have seen, between 0.80 and 0.90 for the V-Dem polyarchy index (Coppedge et al., 2025). We are happy to accept this scepticism, but note that if our backsliding episodes are meaningless noise, derived from meaningless variation in the state-level data, then all that lack of meaning should translate into a lack of empirical evidence for any meaningful economic effects of democratic backsliding.[14]

Backsliding and State Partisanship We conduct some simple analysis focused on the party-political dominance in a state (house and senate/assembly in Democratic or Republican,

[14]As an aside, the Center for Systemic Peace polity score designated the U.S. as an anocracy (+5) in late 2020, as a democracy (+8) in 2021, and as a borderline autocracy (0) in early 2025 (after the end of our sample period).

or 'Mixed' hands) during episodes. The simple pairwise correlation coefficients between our backsliding dummies and the partisan capture of the state by the Republicans, at around 0.3, are modest but not insubstantial. Zooming further in on the start of a backsliding episode and dynamics of state partisanship, we conduct univariate event analyses for Republican state capture: Appendix Figure C-2 charts the propensity of a state safely being in Republican hands in the five years before and after the start of a backsliding episode. Yet we only find a statistically significant effect for the broad and intermediate definitions, and notably only in a single year (the first after an episode has started), indicating a ten percentage point increase.

In Appendix Table C-1 we simply count the number of episodes that took place when Republicans, Democrats, or neither (Mixed) dominated state politics. It can be seen that for Republican dominance, we distinguish a number of special cases where during the episode a first year of Mixed or Democrat dominance is followed by several years of Republican dominance. Tallying up all episodes with Republican involvement indicates that, in line with Grumbach's (2023) analysis for the continuous state democracy index, there is a strong correlation between backsliding and Republican dominance.

We visually convey this correlation in Appendix Figure C-1, which also charts the count of states 'captured' by Republicans (solid black line) and Democrats (dashed black line): using the broad definition of backsliding in panel (a), at its peak in the mid-2010s, around 70% of all states captured by the Republicans were experiencing a backsliding episode, whereas the peak for states captured by Democrats around the same time was below 20%. As can be seen in panels (b) and (c) for the intermediate and narrow definitions, the patterns of democratic backsliding in Republican states are very similar as we tighten the definition of backsliding.

Appendix Table C-2 presents results from simple linear probability models for democratic backsliding. We regress the episode dummy (separately for each of the three definitions) on state and year fixed effects as well as two indicators for state-years when Republicans, respectively Democrats, enjoyed the majority in both the state house and senate (the omitted category is Mixed/power-sharing state-years). Benchmark results suggest that the propensity of the state experiencing a backsliding episode is 17-20% higher (9-10% lower) relative to the 'power sharing' baseline when the Republicans (Democrats) have captured the majority—all these estimates are statistically significant at the 5% level. Even when we further condition on the level of the rescaled Grumbach index, the results for the Republican dominance remain highly significant and positive (10-12%), whereas those for the Democrats are only significant (but still negative) in the most restrictive definition of backsliding.

2.2 State-level Macro and Bilateral Trade Data

From the U.S. Bureau of Economic Analysis (BEA), we take state-level real GDP in 2017 US$ values, as well as population and employment headcounts to construct state real GDP in per capita or per worker terms and the employment growth rate (a control variable). From the

U.S. Census Bureau (CB) we take state-level data for median household income (in real 2023 US$), the headcount of individuals living in poverty, as well as the percentage of the population living in poverty (poverty being defined by the poverty line). In extensions, we adopt CB data on the number of individuals in each state without any health insurance coverage and the state Gini coefficient. The World Inequality Database (WID) provides U.S. state-level data for top income shares (top-10% and top-1%). Patent data are assembled from historical reports by the U.S. Patent and Trademark Office (USPTO): we study total patent grants, which include utility, plant, and design patents; and grants of utility patents.[15] Data on current business R&D expenditure at the state level are taken from reports by the U.S. National Science Foundation (NSF): we distinguish state-level total business R&D spending (including funding from federal or state government, as well as from businesses and organisations outside the state or the U.S.) and businesses' own R&D expenditure. We use the BEA (nation-wide) private domestic investment deflator to transform these into real values and as a robustness check adopt R&D/GDP ('R&D intensity') using current value terms. All of the above are available for 2000-2023, with the exception of the Gini (from 2006), the WID data (up to 2018), the granted patents (up to 2020) and the R&D data (up to 2022). Appendix Tables B-3 and B-4 provide some descriptive statistics by state, in Appendix Figure B-1 we chart the median evolution of treated and control states over 2000-2023 for the main economic variables of interest.

State exports in US$ are provided by USA Trade Online, a division of the U.S. CB — these data are available from 2002 to 2023. Additional data for the gravity regressions include the BEA state population headcount and state GDP as well as state per capita personal income. Destination (country-level) data on population, GDP and per capita GDP are taken from the World Bank Word Development Indicators database. In line with standard practice in the literature, all monetary variables in the gravity regressions are in current (US$) value terms.

3 Methodology

3.1 Factor-augmented Regressions

Our empirical methodology builds on the common factor framework, which we employ to capture unobserved time-varying heterogeneity. This is a popular approach exploited in the synthetic control method (Xu, 2017), difference-in-differences models (Gobillon and Magnac, 2016; Chan and Kwok, 2022), and other approaches where omitted variable bias is of concern (Pesaran, 2006; Bai, 2009). In regression models, the factor proxies are included as additional covariates, representing a control function, akin to the practice in firm-level production function

[15] The average time from application to grant is presently around 13-15 months for design patents, 18 months for plant patents, and 35 months for utility patents.

estimators (Olley and Pakes, 1996; Levinsohn and Petrin, 2003).[16] In the context of economic development, capturing unobserved heterogeneity, in essence total factor productivity, has historically been approached in a variety of ways. The 'Barro regressions' of the early 1990s (Barro, 1991), for instance, attempted to include as many growth determinants as possible as additional regressors, while the Bayesian Model Averaging approach (Sala-i Martin, 1997; Sala-i Martin et al., 2004) focused on the 'relevance' of included determinants à la Barro—both approaches are now deeply unpopular, perhaps for the simple reason that, varied data availability and quality aside, these 'kitchen sink regressions' saturating the empirical model with additional determinants can only proxy for known phenomena, but cannot accommodate the 'unknown unknowns'.[17]

The common factor framework addresses this as a dimensionality problem (too many determinants) and offers a solution using dimensionality-reducing tools. A simple analogy may help illustrate the basic intuition of how and why this approach works. Consider unobserved heterogeneity in the form of different paint colours, from vibrant reds to emerald greens to stylish ochres, one unique colour for each unit of analysis (say, countries or U.S. states), and assume these unobservables are related to observable variables (x) in a simple regression model of y regressed on x. The Barro regression and BMA strategies would be to explicitly control for as many distinct colours as possible, with the latter employing an algorithm to determine which colours really 'matter' for inclusion in the model. The factor model instead postulates that all unobserved time-varying heterogeneity can be represented by a small number of building blocks ('factors') with country/state-specific parameters ('factor loadings'). In our analogy of colours, we need exactly *four* such building blocks to be able to capture every single possible colour: the primary colours (red, blue, yellow), and white.[18] The complexity and vastness of possible differences in colours has been reduced to a very small and easily-managed number. Like primary colours, common factors are profound and 'mutually exclusive' (orthogonal to each other). Like combining colours, we can 'mix' factors in different proportions. Conveniently, we do not need to know what the factors are, or indeed what their relevant combinations (loadings) are: provided we employ sufficient factors to capture the heterogeneity, and have sufficient data to estimate the factors, we can proxy the time-varying unobserved heterogeneity.

Our empirical approach laid out below proxies common factors only using estimates derived from the control sample of 'never-treated' states and includes these proxies in the treatment regressions for states experiencing democratic backsliding. Hence, we exploit the DID data

[16]In a simpler panel model with time-*invariant* unobserved heterogeneity, country fixed effects are control functions which capture these unobservables. The factor model approach, using factor proxies and country-specific parameters (factor loadings), more flexibly captures time-*variant* unobserved heterogeneity, which is why it is sometimes referred to as 'interactive fixed effects'.

[17]In parallel, standard structural gravity estimation is concerned about unobserved multilateral resistance and addresses this by means of dummy variable saturation, which rules out the analysis of any monadic (country/state-specific) variables such as democracy or its demise.

[18]We use paints/pigments ('subtractive mixing') for this analogy, if we considered light ('additive mixing') instead, then a combination of the primary colours would also capture white light.

structure, and unlike alternative factor-based methods do not adopt data from 'not-yet treated' states in constructing factor proxies. There are two main concerns: (i) do we have the right number of factors, and (ii) how can we gauge the 'relevance' of the common factors in both treated and control samples. In our colour analogy, it may be that none of the colours we want to capture involves mixing white, hence we can simply employ the three primary colours on their own. However, adding white to the offering does not undermine our ability to capture all unobserved heterogeneous colours. There is an econometric equivalent to this in the factor model: adding too many common factor proxies does not lead to bias (Moon and Weidner, 2015). In practice, we will provide results using several specifications with four or five factors included. A separate worry is that the control sample maybe only includes colours derived from red and blue, whereas the treated sample additionally requires shades of yellow as well. In this situation our model would be misspecified, since the common factors differ between treated and control samples. We can gauge whether this problem is present in our treatment models with an 'Alpha' test for expected factor loading equality between treated and control samples (Chan and Kwok, 2022). This establishes whether the 'information' we extract from the control sample is equally relevant in the treated sample. We now provide a more formal introduction to our empirical implementations.

3.2 Heterogeneous Difference-in-Differences Model

We employ the Chan and Kwok (2022) Principal Component Difference-in-Differences (PC-DID) estimator which allows for non-parallel trends and heterogeneous treatment effects.[19] This estimator enables us to study non-absorbing treatments such as switching in and out of backsliding episodes. In the potential outcomes framework, the observed outcome of a treatment D_{it} (backsliding) in U.S. state i at time T_0 can be written as

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

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

where the $\mathbf{1}_{\{\cdot\}}$ are indicator variables for the treated states and the time period treated; Δ_{it} is a time-varying heterogeneous treatment effect, X any observed controls (these do not have to be included), $\mu_i' f_t$ represents a set of unobserved common factors f_t with state-specific loadings μ_i. $\widetilde{\epsilon}_{it}$ is the error term.

The treatment effect can be decomposed 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}. $\overline{\Delta}_i$ is the individual treatment effect averaged over the treatment period. The reduced form model is

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

[19]Applications of the PCDID include Boese-Schlosser and Eberhardt (2024, 2025) and Cho et al. (2025).

where ϵ_{it} has zero mean but can be heteroskedastic and/or correlated over time and space.

We estimate the heterogeneous treatment effect $\overline{\Delta}_i$ in two steps: first, using PCA, we estimate proxies of the unobserved common factors from the residuals $\hat{e}_{it}$ of the following auxiliary regression in the control sample: $y_{it} = b_{0i} + b'_{1i}X_{it} + e_{it}$, or, if we exclude additional controls, $y_{it} = b_{0i} + e_{it}$; second, using least squares, we estimate a treatment regression equation augmented with the factor proxies as additional regressors (control functions):

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

where $\hat{f}$ are the estimated factors from the first step and d_i is the state-specific treatment estimate. We estimate (4) augmented with four or five common factors. The model is estimated for each treated state separately and only 'never-treated' states are used in the construction of the factor proxies: no 'not-yet-treated' observation enters the control group.

The above setup can accommodate endogeneity of treatment D_{it} such as correlation between treated units and factor loadings, the timing of treatment and factor loadings, or between observed covariates and timing or units of treatment. States, most importantly those in the treated versus control samples, can have non-parallel trends.

There are three assumptions for the consistency of $\hat{d}_i$ or its Mean Group average: (i) all unobserved heterogeneity determining y can be proxied by a small number of factors (following Pesaran, 2006; Bai, 2009; Athey et al., 2021), such that u in equation (4) is orthogonal to the treatment; (ii) in our treatment and control group setup, there is expected factor loading equality between treated and control samples: the factors extracted from the control group are equally 'relevant' in the treatment group; and, (iii) our use of an estimator assuming an absorbing treatment is not violated by the economic implications of the post-treatment period: in essence, we assume that the backsliding episode is an interim 'regime', at the end of which the economy of treated states returns to a pre-regime equilibrium *without* any temporal spillovers, such as continued economic decline or an economic 'bounce-back' effect after an episode has ended.

We address the first assumption by adding a substantial number of estimated factors (four or five) to the treatment regressions. The Alpha test introduced below will formally address the concerns raised in the second assumption. An extension to the PCDID, which involves a set of 'post-treatment dummies' in the treatment regressions (covering the four years after the end of a backsliding episode), seeks to address the third assumption. We elaborate below.

In other robustness checks, we include/exclude employment growth as an additional covariate X.[20] We also consider the inclusion of year dummies for the Covid years 2020-2022

[20]We experimented with other controls, such as population growth or state total international exports, but these typically represented 'bad controls' or the specifications failed the Alpha test. Recent analysis by Bretschneider and Westerlund (2025) indicates that the PCDID is inconsistent if included covariates are correlated with the unobserved common factors, due to a misspecified first step (no factors). We obtain qualitatively identical results for GDP per capita and headcount of the poor (outcomes where our models include employment growth as a covariate) and find qualitatively identical results if we demean the variables

— in particular in the analysis of innovation (where patent and R&D data ends during Covid) but also for the export flow results, this strengthens our empirical findings.

Typically, results would be presented in form of average treatment effects (ATET), for which inferential statistics can be based on the variance estimator of Pesaran (2006): $\widehat{\text{var}}(\hat{d}^{MG}) = [N(N-1)]^{-1}\sum_{i=1}^{N}(\hat{d}_i - \hat{d}^{MG})^2$ for N treated states. However, we adopt multivariate running line regressions following Royston and Cox (2005) to provide a graphical representation of the relationship between treatment effect magnitude and years of treatment (time spent in backsliding episodes). We regard running line regressions as 'local ATET', where 'local' refers to a similar number of years spent in backsliding episodes, and hence adopt the standard errors from this methodology (see Boese-Schlosser and Eberhardt, 2024). Our result graphs do not depict an event analysis but map the magnitude of the treatment effect to the total number of years spent in a backsliding episode or backsliding episodes.

3.3 Diagnostic Testing

We inform our empirical analysis with two sets of diagnostic tests: first, the Chan and Kwok (2022) Alpha test for expected factor loading equality between treated and control samples; and second, a Wald test for specifications including an additional control (we limit this to the inclusion of the state employment growth rate since adding other controls was always rejected by the test) to determine whether the control might constitute a 'bad control' following Angrist and Pischke (2008).

The Alpha test informs us whether the unobserved time-varying heterogeneity in the control sample spans the same space as that in the treated sample. The PCDID allows for non-parallel trends across countries, most importantly between those in the treated and control samples.[21] But if the underlying latent factors differ between treated and control samples (recall the example of primary colours), then adding estimated factors from the control sample in the treatment regression will not be sufficient to capture the unobserved heterogeneity in the latter, and the PCDID is misspecified.

The Alpha test is implemented by (i) computing the cross-section average of the residuals in the control sample regressions, $\bar{\hat{e}}_t = N^{-1}\sum_i \hat{e}_{it}\, \forall i \notin E$, (ii) including this averaged residual as an additional regressor in the treatment regression replacing the estimated factors,

$$y_{it} = b_{0i} + d_i \mathbf{1}_{\{t>T_{0i}\}} + c_i' \bar{\hat{e}}_t + b_{1i}' X_{it} + u_{it}, \tag{5}$$

and, (iii), applying a t-test for whether the (Mean Group) average of $\hat{c}_i\, \forall i \in E$ across all treated states is equal to one ($\Leftrightarrow N^{-1}\sum_i(\hat{c}_i - 1) = 0$).[22]

with respect to state means.

[21] This is possible since individual countries can have unrestricted parameter coefficients on the common factors included in the model, which could be positive or negative.

[22] The intuition of why the averaged c should be one is as follows: consider a standard OLS panel regression of y on some x with year fixed effects, then extract the estimated coefficients on the year dummies as a

We report the outcomes of the Alpha test for each specification (the test does not differ by the number of factors included in the model) in two ways: first, in the main text, we use a simple signalling device (e.g. ••○) to highlight passed (•) or failed (○) Alpha tests for the three alternative backsliding definitions; second, we report the equivalent t-ratio for each test in Appendix Tables D-1 to D-3, with an absolute value of $t > 1.96$ rejecting the null and rendering the PCDID model in question misspecified.

For the test for 'bad controls' we regress the treatment dummy on the estimated factors and the control variable(s) in question: our assumption (and null hypothesis) is that once the factors are included, the control variable is not statistically significantly related to the treatment. We can test this assumption by regressing the backsliding indicator on estimated factors and the control(s) in the treated sample and carrying out a Wald test for the (joint) insignificance of the control(s). If the null is rejected, we need to conclude that the controls may constitute 'bad controls'. Implementation is via the Mean Group estimator, and results (p values) are reported in Appendix Tables D-1 and D-2 for specification with four and five factors (we do not test this in the gravity model). In practice we limit our presentation in the maintext to specifications which pass this test.[23]

3.4 Post-Treatment Trajectories

Virtually all our state-specific models include treatment reversal: the backsliding episode is not eternal but ends after a number of years (on average 7). This setup imposes strong assumptions on our PCDID, namely that the post-treatment reversal does not systematically affect the causal estimates of backsliding episodes. One scenario is that relative to the pre-episode period, the post-episode period has no specific trajectory: after the episode, the economic conditions studied in the state do not bounce back (positive effect), but they also do not deteriorate further ('aftershock' effect). This scenario, assuming away temporal spillovers effectively allows us to treat pre- and post-episode periods as some form of (uniform or temporally exchangeable) status quo or equilibrium level, with the episode constituting a shock which can quantitatively be assessed against this equilibrium. An alternative scenario is that after the backsliding episode, the state economy 'bounces back', which violates any assumption that the pre- and post-episode periods are uniform and/or interchangeable.

We address this issue in a robustness check which introduces year dummies for the four years immediately following the end of a backsliding episode.[24] The intuition of this exercise

single variable f and add this into the same OLS regression of y on some x (without the year dummies): the coefficient on the variable f containing the year dummy coefficients will be one.

[23]As in standard DiD estimators, provided they are not 'bad controls', the inclusion of additional control variables is not required if the parallel trend test (or in our case the Alpha test) is passed, although they may improve the precision of the estimates — in case of the PCDID this would be via the precision of the factors extracted from the control sample residuals.

[24]If a state experiences more than one episode, the post-treatment effects as captured by the year dummies are assessed *jointly*.

is to capture the economic effect of reversal (on the four years post-treatment) and to gauge whether the economic treatment effects are robust to this extension. Formally, we estimate in treated states $i \in E$

$$y_{it} = b_{0i} + d_i^* \mathbf{1}_{\{t>T_{0i}\}} + \sum_{s=1}^{4} d_{is}^{post} \mathbf{1}_{\{T_{Ti}+s\}} + a_i' \hat{f}_t + b_{1i}' X_{it} + u_{it}, \tag{6}$$

where $\hat{f}$ are the estimated factors from the first PCDID step and d_i^* is the state-specific treatment estimate. T_{Ti} denotes the final year of an episode: four dummies, d_{is}^{post}, capture the period after the end of the episode. We report these results in the treatment effects-time in treatment space, like for the main results, but add separate plots for the robust post-treatment effect means in years 1 to 4. Results presented in Appendix Figure F-1 are discussed in detail below.

4 Empirical Results

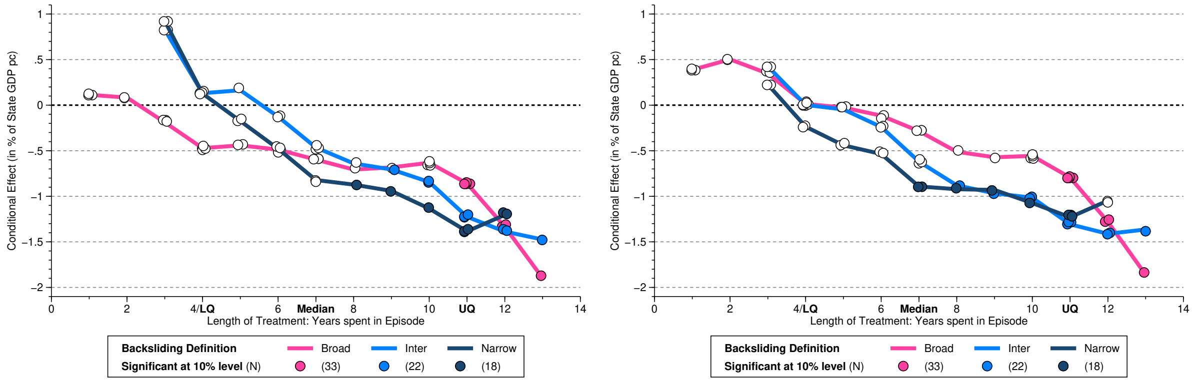
Presentation and Interpretation of Results We follow Boese-Schlosser and Eberhardt (2024) and present our results graphically to highlight the relationship between time spent in a backsliding episode and the magnitude of any causal economic effect: if deteriorating democratic institutions are bad for economic prosperity, then prolonged episodes are likely to have worse implications than shorter ones. We adopt predictions from multivariate running line regressions (Royston and Cox, 2005) which we estimate using the state-specific treatment effect $\hat{d}_i$, the total years spent in backsliding episodes, and dummies for the total number of episodes experienced by the state. We also employ state-specific weights in this estimation procedure, created by an M-estimator (Rousseeuw and Leroy, 2005) when computing robust average treatment effects (ATET).[25] Our result plots present the *predicted values* from this multivariate smoothing procedure.

Panels (a) to (c) of Figure 3 provide results for average state real income per capita, top-income share (inequality), and the poverty headcount. Additional plots in the appendix, cover alternative measures and specifications or subsets of treated states. Each results plot is presented as follows: on the x-axis we capture the (total) number of years a state has spent in an episode (or episodes), the y-axis indicates the treatment effect of democratic backsliding. The plotted line represents the flexible prediction of our multivariate running line regression. The markers in each plot indicate the distribution of state-level estimates in terms of total treatment length, and we minimally perturb these markers to aid visualisation. A filled (white) marker signifies statistical (in)significance at the 10% level. On the x-axis we further indicate the lower quartile (LQ), median, and upper quartile (UQ) of the length of time spent

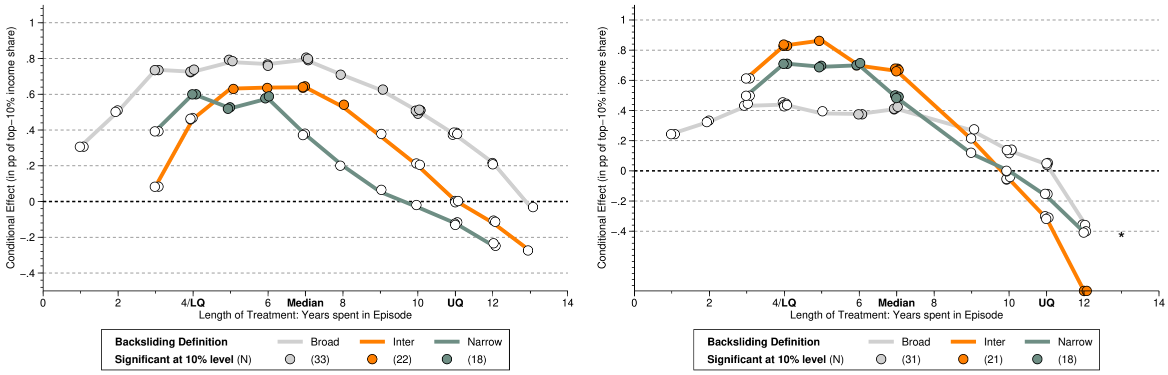
[25] These weights will be smaller for outlier state treatment effects. Our results are qualitatively unchanged if we use the total 'drop' in the rescaled SDI for a state as the weight or if do not apply any weights (available on request).

Figure 3: Democratic Backsliding Episodes — Income, Inequality, and Poverty

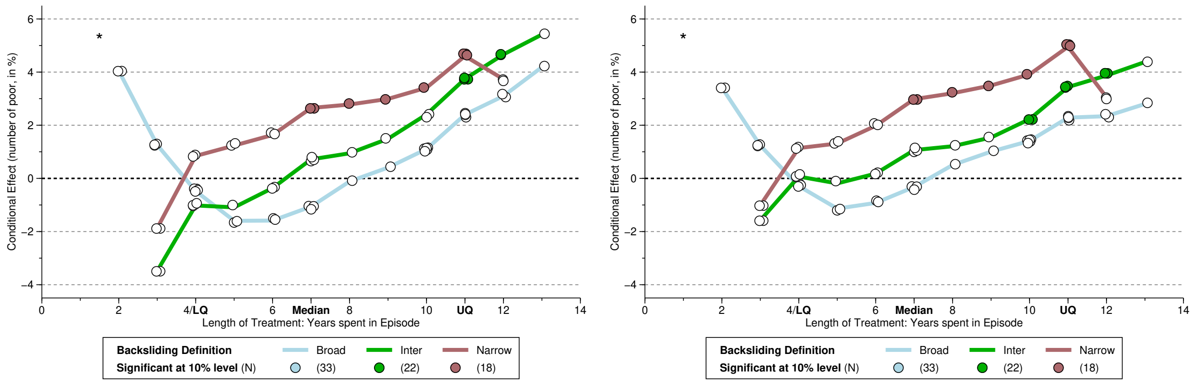
(a) GDP per capita,••• specifications with 4 (left) and 5 factors (right)



(b) Top 10% Income Share,••• specifications with 4 (left) and 5 factors (right)



(c) Poverty headcount,••• specifications with 4 (left) and 5 factors (right)



Notes: We present treated-state predictions from multivariate running line regressions (Royston and Cox, 2005) illustrating the economic effects of democratic backsliding. The underlying PCDID specifications have the employment growth rate as additional control (except in panel (b) where the specification without controls had more favourable diagnostics) and augment the treatment regression with four or five estimated factors as indicated (models with three or six estimated factors yield qualitatively very similar results, available on request). The (total) number of years a state has spent in one or more backsliding episodes is shown on the x-axis of each plot, the economic effect on the y-axis. The running line regressions control for the number of times a state experienced a backsliding episode and use weights derived from the robust ATET estimate using an M-estimator (Rousseeuw and Leroy, 2005). A filled (white) marker indicates statistical (in)significance at the 10% level. An estimate of -1 (+1) indicates a 1% reduction (increase) in income, top-10% income share, and the number of poor people, in panels (a) to (c), respectively. Each plot presents results for our three definitions of backsliding: a broad one (33 treated states), an intermediate one (22), and a narrow one (18) — see text for definitions. We use ••• to signal which specifications using these three different definitions of backsliding (Broad, Inter, Narrow) pass (•) or fail (°) the Alpha test.

in episode(s), namely 4, 7, and 11 years, respectively.[26] We present three sets of results, for the 'broad', 'intermediate', and 'narrow' definitions of backsliding. Finally, the two separate plots in each panel are for the results derived from a PCDID specification augmented with four and five estimated factors, respectively.

4.1 Average Income

Diagnostics All specifications presented in panel (a) of Figure 3 pass the Alpha test (as indeed do specifications without an additional control variable), and furthermore the Wald tests provide no indication that the inclusion of the employment growth rate in the PCDID regression constitutes a 'bad control'. See Appendix Table D-1 for detailed results of all macro-variables studied. Recall that in the result plots we employ a simple signalling device (e.g. ••○) to highlight passed (•) or failed (○) Alpha tests for the three alternative backsliding definitions.

Main findings All running line graphs in both plots of Figure 3, panel (a), are downward-sloping in treatment length, near-monotonically so. Starting from the broad definition of backsliding (in pink), treatment effects are only statistically significant for a few states with the longest treatment length; however, as we move to the intermediate (light-blue) and narrow (navy) definitions of backsliding, more and more states are predicted to have statistically significant negative treatment effects. For our most conservative (narrow) definition, effects are statistically significant from around the median treatment length (7 years), with a treatment effect of -1 to -1.5 percent. Hence, all definitions point to a deterioration in per capita income over time in states experiencing democratic backsliding, with the statistical evidence becoming stronger as we tighten the definition for democratic retreat.

Robustness Our above predictions are conditioned on the number of episodes states experience (between 1 and 3). To confirm that mixing single- and multiple-episode treated states does not distort our findings, we present results for states that just experienced a *single* episode in Appendix Figure E-1. Despite the reduction in sample size (from 33 to 22 for 'broad', 22 to 18 for 'intermediate', and 18 to 16 for 'narrow' definition), the overall findings are qualitatively robust, and although statistical significance varies, the prediction lines for the three definitions of backsliding are now almost congruent. In Appendix Figure E-7 panel (a), we include three year dummies for the Covid years and find results qualitatively unchanged.

In Appendix Figure E-2 we report the results based on the PCDID specification without additional controls. The common downward trajectories across backsliding definitions are still present, though the statistical evidence is somewhat weaker.

We further experimented with using real GDP *per worker* as outcome variable (in models with or without the employment growth rate as additional control), but for all definitions of

[26] These are common across the three definitions, with the exception of the LQ for the narrow definition (5) and the UQ for the broad one (10).

backsliding these specifications always fail the Alpha test and hence may be misspecified. For reference, running line plots are presented in panel (a) of Appendix Figure E-3, indicating negative effects of similar magnitude as those in the per capita analysis. We next investigate the effect of backsliding on income inequality.

4.2 Income Inequality

Diagnostics All specifications for state median household (presented in panel (a) of Appendix Figure 3) fail the Alpha test, suggesting that PCDID regressions analysing state median household income may be misspecified. Put simply, the unobservables driving median hh income in the treated versus control states are not the same—this hints at the possibility that *cross-state* inequality may be a predictor for backsliding. Note that the diagnostic tests also fail if we study various alternative specifications. Diagnostics are however favourable, including for Wald tests for 'bad instruments', when we use the 'within-state inequality' data (Gini coefficients), which are, however, only available from 2006, and top income shares data, available 2000-2018 (presented for top-10 income share in panel (a) of Appendix Figure 3).

Results: Cross-state Inequality Across the main results for median HH income presented in panel (a) of Figure E-4 as well as in various robustness checks limiting the treated sample (single episode states: Appendix Figure E-1) or not including control variables (Appendix Figure E-2), the running line predictions are always near-universally statistically insignificant. Adding dummies for Covid years, Appendix Figure E-7 panel (b), also makes no difference.

Results: Within-state Inequality For the Gini, Panel (b) of Appendix Figure E-3 indicates that none of the predictions are statistically significantly different from zero, with estimates for the intermediate and broad backsliding definitions exclusively positive, whereas those for the narrow definition are more mixed. The results for top-10% income share are presented in panel (b) of Figure 3. The running line predictions are more consistent across definitions of backsliding and suggest the state with median time spent in a backsliding episode experiences a statistically significant increase in the top-10% income share by around half a percentage point. For states with longer treatment time, there is no significant effect (statistically or in terms of magnitude). Results for the top-1% income share, in Appendix Figure E-4, are less consistently statistically significant but point to a positive increase. Since the top-income share data ends in 2018 these results are not affected by the Covid pandemic.

Taken together, the results for within- but not between-state income inequality provide some tentative evidence of widening inequality as a result of backsliding, with the top-10% income share increasing for states at the median episode length. We now turn to look more specifically at individuals *at the bottom* of the income distribution in each state.

4.3 Impoverishment

Diagnostics All specifications presented in panel (c) of Figure 3 pass the Alpha test as well as the Wald test for 'bad controls'. Similarly for the poverty rate (share of state population below the poverty line) and for the Alpha test in the specification without additional controls.

Main Results The prediction lines for impoverishment in panel (c) of Figure 3 are all upward-sloping (an initial spike for the 'broad' backsliding definition aside) and as in our previous results the statistical evidence for a detrimental effect of backsliding gets stronger as we tighten the definition, while in the present case the magnitudes of the treatment effects are also getting larger. The 'narrow' result suggests that backsliding increases the number of individuals living below the poverty line by around 3-5% (median and Upper Quartile of the treatment distribution).

Robustness Results are less consistent across backsliding definitions if we limit the sample to states which experienced a single backsliding episode. In Appendix Figure E-1, panel (c) indicates weaker evidence for widespread increase in the number of individuals living below the poverty line in the broad and intermediate definitions. The results for the narrow definition, on the other hand, are robust and suggest an increase by 4-6%. In Appendix Figure E-7 panel (c) we include year dummies for the Covid years to our main specification and find that results are more precisely estimated and amplified, suggesting an increase in the poor headcount of up to 4-8%.

Results for the specifications without any additional controls (panel (c) of Appendix Figure E-2) closely follow those of our main results, with the 'narrow' results suggesting a 4-6% increase in the poor.

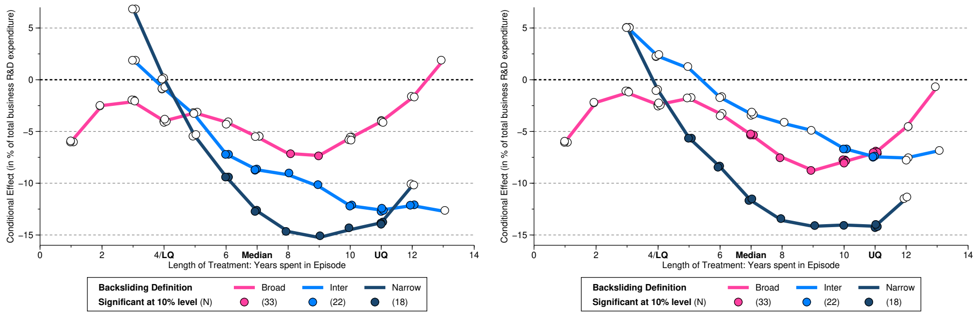
In panel (c) of Appendix Figure E-3 we analyse the effect of backsliding on the poverty rate (i.e. share of state population), with results interpretable as percentage point changes. All three backsliding definitions suggest an increase in the poverty rate as treatment length increases, albeit with varying empirical precision: compared with the sparse statistical evidence for the broad and intermediate definitions, results for the narrow variant are statistically significant for almost all treated states, with a treatment effect of around half a percentage point.

Extension: The Uninsured Panel (c) of Appendix Figure E-3 charts the running line plots for our analysis of the headcount of individuals without health insurance coverage. These results again indicate positive treatment effects with varying degree of precision, whereby the narrow backsliding definition suggests both the highest magnitudes as well as the strongest statistical evidence for a positive treatment effect (on the order of around half a percent).

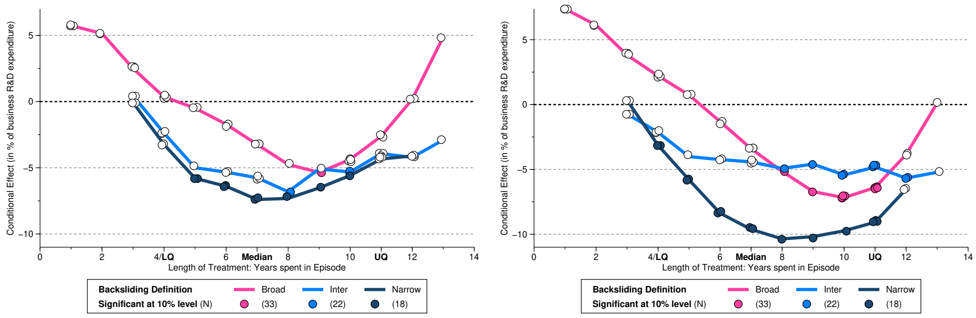
Our focus on the bottom of the income distribution suggests an absolute and relative increase in deprivation, as indicated by positive significant treatment effects for the poor headcount, the poverty rate, and to a lesser extent the headcount of the uninsured.

Figure 4: Democratic Backsliding Episodes — Innovation

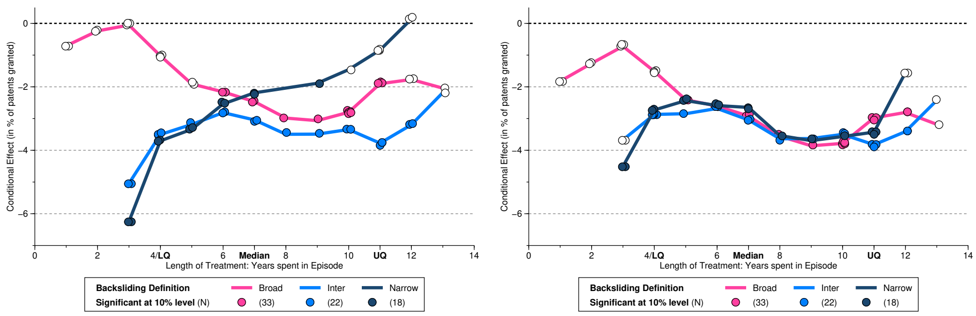
(a) Total Business R&D Expenditure,●●● specifications with 4 (left) and 5 factors (right)



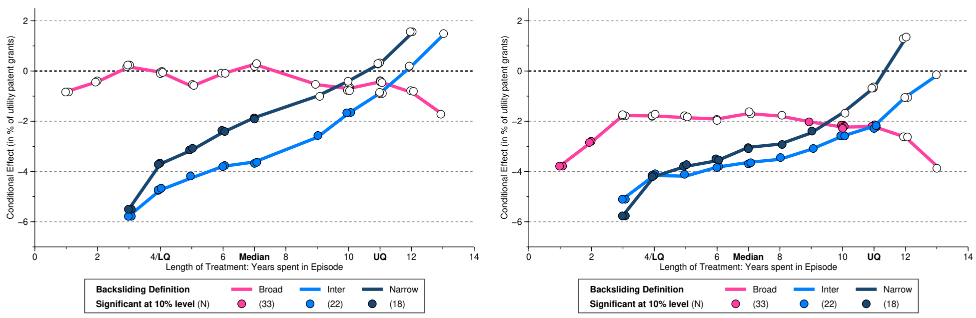
(b) R&D Expenditure by Businesses,●●○ specifications with 4 (left) and 5 factors (right)



(c) Total Patents Granted,●●○ specifications with 4 (left) and 5 factors (right)



(d) Utility Patents Granted,●●○ specifications with 4 (left) and 5 factors (right)



Notes: We present results from the PCDID specifications without additional control variable — specifications with employment growth always fail the Alpha test or the Wald test for 'bad controls'. The R&D (patent) data is limited to 2000-2022 (2000-2020). 'Total business R&D' includes expenditure by businesses sourced from the federal government (e.g. grants) and from 'others' (i.e. other firms located inside or outside the US, state or foreign government agencies, and other organizations inside or outside the US). 'Business R&D' refers to the expenditure paid for by the firms themselves. 'Total Patents' includes utility, plant and design patents. We use ●●○ to indicate whether specifications using the liberal, intermediate, and conservative definitions of backsliding pass (●) or fail (○) the Alpha test at the 10% significance level. For other details, see notes to Figure 3.

4.4 Innovation

Diagnostics For the analysis of R&D expenditure we find that specifications including state employment growth always either fail the Alpha test or the Wald test for 'bad controls' or both. The diagnostics for specifications without a control variable are more favourable, only the 'narrow' definition of backsliding for the business R&D expenditure fails the Alpha test.[27]

For the patent analysis, we again studied specifications including employment growth as additional control, but found these fail the Alpha test in all specifications considered. Wald tests typically indicated employment growth to be a bad control. The models without additional controls perform better, only the narrow definition of backsliding fails the Alpha test in the models for granted patents and granted utility patents.

Main Results Figure 4 presents results for the real R&D expenditure by private businesses: in panel (a), apart from their own funds, this further includes funding these businesses received from the federal government, from other businesses or organisations in or outside the U.S., as well as from U.S. state or foreign government agencies or labs (total business expenditure); in panel (b) we focus on businesses' own R&D expenditure. All results indicate that private businesses in states that experience democratic backsliding cut back their R&D expenditures. Focusing on the intermediate and narrow definitions of backsliding, for the expenditures including federal funding and other sources in panel (a), the treatment effect is between 8 and 14% lower expenditure in states with median episode length, and between 4 and 10% lower expenditure when we just consider businesses' own funding in panel (b).[28] These are sizeable effects, which are qualitatively identical if we adopt real R&D expenditure deflated by state population (available on request).

Figure 4 further presents running line prediction plots for state-level innovation output (patent counts, available 2000-2020):[29] Panel (c) uses data on all patents granted, while panel (d) zooms in on utility patents granted. The data for all patent types granted in panel (c) shows a decline of between -3 and -4% for treated states, which is fairly stable over the episode length. Focusing on utility patents in panel (d) indicates that states with short episodes experience a significant drop in patents granted of up to -6%, whereas with longer 'exposure' to democratic backsliding, the treatment effect is reduced and eventually turns insignificant. Overall, granted patents of all types are clearly negatively affected by democratic backsliding, with patents covering large innovative steps seemingly slowly converging back to the original levels, whereas plant and design patents, which represent smaller innovative steps (and hence commercial potential), seem subject to a persistent drop-off.

[27] We also adopted measures of R&D intensity, i.e. R&D expenditure deflated by the state GDP, but these specifications always fail the Alpha test. Using R&D expenditure deflated by state population yields sound diagnostics except for the 'narrow' definition of backsliding in both sets of models.

[28] The difference may arise since firms seek fewer government grants or collaboration and funding from firms or organisations outside the state or from abroad.

[29] Here, we cannot separate out patents of private businesses but study all state patents instead.

Robustness In Appendix Figure E-8 panels (a) and (b) we study the implications for business R&D expenditure and patent counts in specifications including year dummies for the Covid years and find results substantially strengthened. For the former, across all definitions of backsliding and factor augmentations we find results substantially increased in magnitude, suggesting up to 20-30% decline in R&D expenditure. For patenting in panels (c) and (d) we now observe consistent patterns between all patents and utility patents granted, indicating substantially higher detrimental effects of -14 to -18%.[30] We also considered patent *applications*, which yield statistically insignificant treatment effects in the main specification (without controls), but arrive at a statistically significant backsliding effect of -5 to -10% when we add Covid dummies (available on request).

Taken together, these findings suggest that democratic backsliding severely harms private businesses' innovation *effort* as well as state innovation *output*.

4.5 Treatment and Post-Treatment Effects

In Appendix Figure F-1 we present the running line predictions derived from extended PCDID models which include additional year dummies for the first four years after the end of a backsliding episode. This exercise addresses the concern that our treatment effects estimator assumes that the economic implications of backsliding episodes do not spill over to the post-treatment period, i.e. there is no bounce-back of the economy with increased economic progress relative to the pre-treatment years and also no prolonged depression following the episode. In essence, we assume that democratic backsliding has one-off levels effects and not growth effects on economic outcomes and that furthermore the dynamics of these regime switch periods are relatively simple and/or short-lived (like switching a light on or off). Adopting four post-treatment years is both parsimonious and speaks to the U.S. election cycle.

The running line plots are presented in exactly the same fashion as for our main results, while for the four post-episode dummies we estimate outlier-robust means using an M-estimator (Rousseeuw and Leroy, 2005) and report these along with the 90% confidence intervals for each of the four years and three backsliding definitions. Alpha tests for these extended specifications are summarised in the same fashion as before, using $^{\bullet}$ and $^{\circ}$ to indicate whether the specification passes or fails this test, respectively: e.g. $^{\bullet\bullet\circ}$ would indicate that broad and intermediate backsliding variants pass the test, whereas the narrow variant does not (t-ratios are reported in Appendix Table D-2). As before we report results for treatment regressions augmented with four and five estimated factors.

Panel (a) suggests that state income per capita experiences a post-treatment 'bounce-back', whereby the average post-treatment effects in years one to three are statistically significantly positive for many of the specifications. As a result, it appears that our finding of a progressive deterioration in average income per capita in states experiencing longer backsliding

[30] In the patent analysis, only a single year (2020) is captured by the additional dummy augmentation.

episodes is rendered statistically insignificant. While the average American in treated states hence does not appear to be economically affected by backsliding, panels (b) and (c) suggest, in line with our previous findings, that income inequality deteriorates (top income shares rise with treatment length) and more state citizens drop below the poverty line—treatment effects are somewhat larger in magnitude than in our main results. In all related plots for the four post-treatment years the robust mean effects are mostly statistically insignificant. Similarly, in panels (d) and (e) for total and own business R&D expenditure, and in panels (f) and (g) for patent grants (all patent types) and utility patents only.[31] These results imply that our assumption of pre- and post-treatment periods representing some form of equilibrium from which the state economy deviates during the backsliding episode is on average valid for all our findings with the exception of average state income.

4.6 International Export Flows

Heterogeneous Structural Gravity Model For our dyadic trade flow regressions, we adopt the estimator developed in Desbordes et al. (2025), which marries the Poisson Pseudo Maximum Likelihood (PPML) structural gravity regression with Chan and Kwok's (2022) factor-augmented difference-in-differences model. Instead of extracting factors from control sample regressions using Principle Component Analysis, we use the Pesaran (2006) 'common correlated effects' (CCE) approach and include control sample cross-section averages of the 'economic mass' variables computed in the treatment regressions. Using PPML, we estimate the following equation at the pair-level for exporter state i and destination country j for pairs where the former experienced democratic backsliding:

$$\begin{aligned}\mu_{ijt} \quad = \quad & exp\Big[\alpha_{ij} + \theta_{ij} D_{it} + \eta_{ij} D_{jt} \\ & +\delta^i_{ij}\,\overline{ln(Y)}^i_t + \kappa^i_{ij}\,\overline{ln(Pop)}^i_t + \delta^j_{ij}\,\overline{ln(Y)}^j_t + \kappa^j_{ij}\,\overline{ln(Pop)}^j_t \\ & +\psi^i_{ij} ln(Y/L)_{it} + \psi^j_{ij} ln(Y/L)_{jt} + \epsilon_{ijt}\Big].\end{aligned} \tag{7}$$

The dependent variable represents the exports from state i to country j at time t. Our parameters of interest are the θ_{ij}, which relate to the binary indicator for U.S. state democratic backsliding. We also include a dummy for destination country democratic collapse D_{jt} to account for potential changes in state exports in response to political economy factors *in the destination* (see Boese-Schlosser et al., 2025).[32]

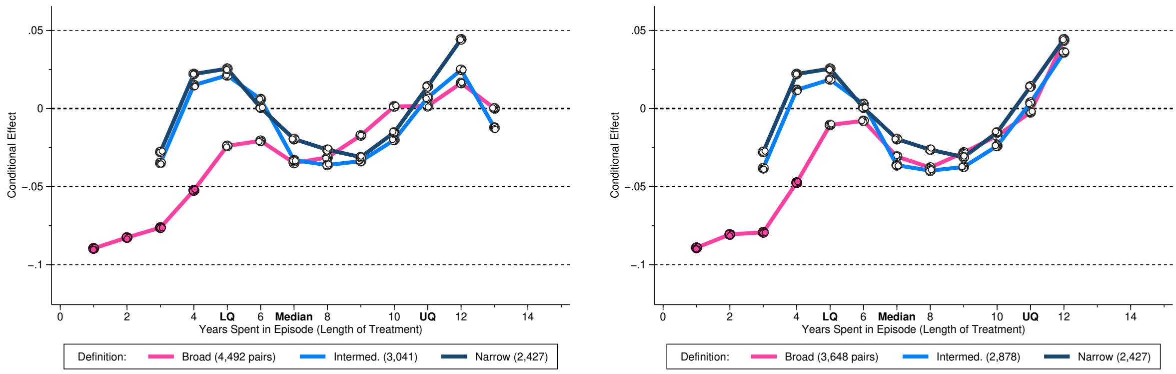
The second line represents the cross-section averages at time t ($\overline{ln(X)}^i_t = N^{-1}\sum_i ln(X)_{it}$) of the 'economic mass' variables employed in the gravity model (GDP, population; in logs).

[31] Note that the Alpha test rejects the specifications for the narrow backsliding definition for business own R&D and the two patent analyses.

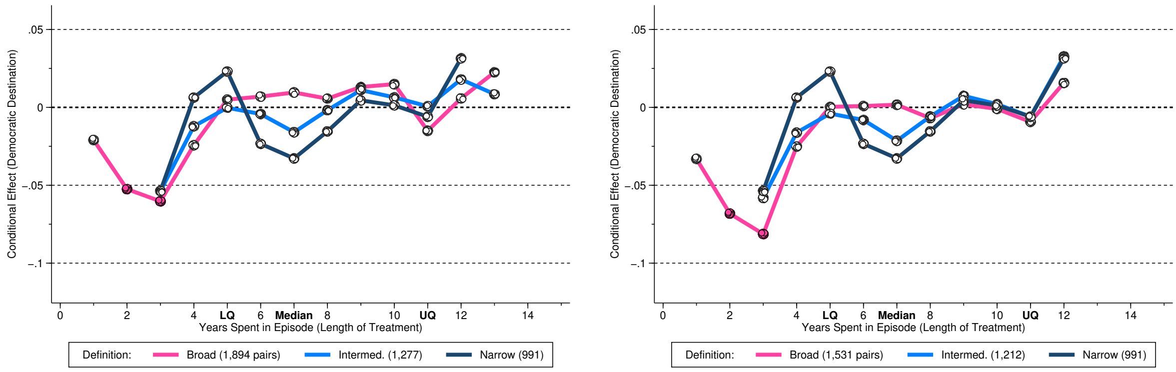
[32] Here, we adopt the Lührmann et al. (2018) Regimes of the World definition of democracy (≥ 2) and autocracy (< 2).

Figure 5: Democratic Backsliding Episodes and Exports

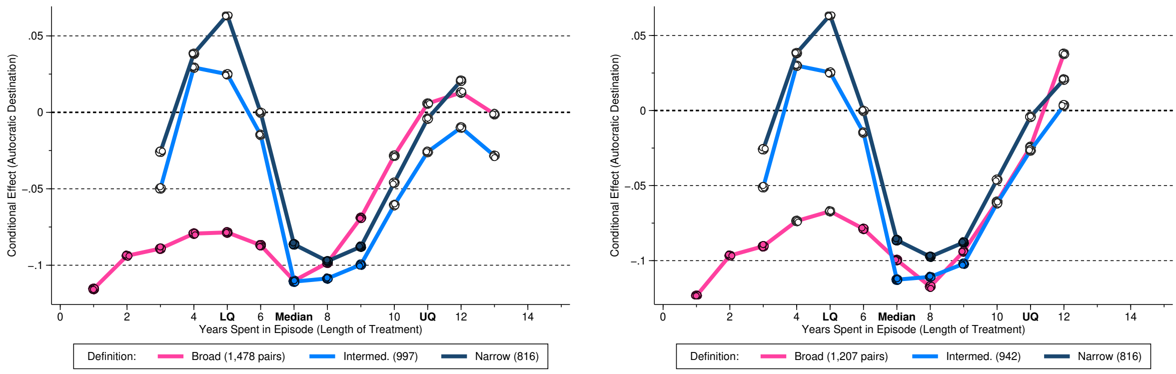
(a) Any Destination, all states•∘∘ (left) and states with single backsliding episode•∘• (right)



(b) Democratic Destinations, all states•∘∘ (left) and states with single backsliding episode•∘∘ (right)



(c) Autocratic Destinations, all states••• (left) and states with backsliding single episode••• (right)



Notes: We present treated-state predictions from multivariate running line regressions (Royston and Cox, 2005) illustrating the economic effects of democratic backsliding for state exports to (a) all destinations, (b) destinations which are democratic throughout 2002-2023, and (c) destinations which are autocratic throughout 2002-2023. The (total) number of years a state has spent in one or more backsliding episodes is shown on the x-axis of each plot, and the economic effect on the y-axis. The running line regressions control for the number of times a state experienced a backsliding episode and use weights derived from the robust ATET estimate using an M-estimator (Rousseeuw and Leroy, 2005). An estimate of -.1 indicates a reduction of -9.5% $(exp(-0.1) - 1 = -.095)$ in exports. Each plot presents results for our three definitions of backsliding: a broad one (33 treated states), an intermediate one (22), and a narrow one (18) — see main text for definitions. A filled (white) marker indicates statistical (in)significance at the 10% level. We use • and ∘ in the subfigure title to indicate whether a specification passes or fails the Alpha diagnostic test at the 10% level: •∘∘ indicates that the liberal definition passes the test, whereas the intermediate and conservative definitions do not.

These averages are computed over all exporter states i (first two terms) and all destination countries j (last two terms), which, respectively, never experienced democratic backsliding and remained democratic throughout the sample period.[33] This setup controls for the 'multilateral resistance terms', which are functions of output/expenditures, captured by the cross-section averages, and bilateral trade costs, captured by the bilateral factor loadings (Anderson and Van Wincoop, 2003).

The last line of equation (7) refers to the exporter and destination income terms (state income per capita and country per capita GDP, respectively): if we exclude these, we study the 'total' effect of democratic backsliding on state exports but cannot determine whether backsliding causes trade or trade adjusts because backsliding causes a change in income, which then transmits to increased exports. If we include them, we condition on income and hence identify the 'direct' effect of backsliding on export flows.

The implementation here omits a correction proposed by Desbordes et al. (2025) in the gravity model of *country-to-country* export flows, when country i experiences treatment (e.g. democratic regime change) but *never* has positive trade flows with country j. They argue that ignoring these 'always-zero' trade pairs (due to the pairwise regression setup of the estimator) would likely lead to a bias in the average treatment effect,[34] and propose the inclusion of additional cross-section averages terms to correct for this bias. In our dyadic U.S. state-destination country data, however, we observe only a handful of states (HI: 135 partners, WY: 150) that trade with significantly fewer than the 162 to 165 total number of international export destinations in the dataset (i.e. have non-zero trade values in at least one sample year). Given our limited time series of 22 years, we would argue that in the present case, the remedy proposed by Desbordes et al. (2025) — an additional four cross-section averages terms in each pairwise regression — is likely worse than the actual problem.

Diagnostics In the heterogeneous gravity model, we have to adjust our testing procedure for the Alpha test since we do not have *estimated* factors but *cross-section averages* proxying the unobserved factors. Following Desbordes et al. (2025) we test whether the parameters on the cross-section averages are jointly equal to one. Appendix Table D-3 reports the p-values for these tests related to our export flow analysis. Focusing on the analysis to all destinations, only the 'broad' definition passes this diagnostic test, whereas if we focus on states with a single backsliding episode the 'narrow' one also passes. If we zoom in further to consider only the top-100 or top-50 trading destinations, the diagnostics generally improve. Similarly if we consider specifications which include period dummies for the Covid years.

[33] Using cross-section averages to proxy for unobserved common factors (see Pesaran, 2006 and for limited dependent variables Boneva and Linton, 2017) is a well-established alternative to estimating factors (e.g. Bai, 2009; Chan and Kwok, 2022).

[34] In the case of democratic regime change in Desbordes et al. (2025) ignoring the failure to affect zero trade flows (by having to drop 'always zero' trade pairs) would *overestimate* the democratic dividend for trade.

Main Results and Robustness Figure 5 presents the running line predictions for exports to (a) any destination, (b) destinations which were democratic throughout the sample period, and (c) destinations which were autocratic throughout the sample period. This split is informed by research on democratic collapse and backsliding in their effect on export flows (Boese-Schlosser et al., 2025), where reduced export flows were found to be driven by falling exports to *democratic* countries, suggesting a 'punishment' effect for democratic retreat. In each case we provide results for *all* states with backsliding episodes (left plot) and those which only experienced a *single episode* (right plot).

Our results for all destinations in panel (a) show some common patterns across backsliding definitions over treatment length, but crucially, the effects are by and large statistically insignificantly different from zero. This finding extends to the democratic destinations in panel (b). Results for the exports to autocratic destinations in panel (c) show strong U-shaped patterns with statistically significant negative effects of around -10% for 7 to 8 years in a backsliding episode. This finding is surprising and robust to a narrower focus on top-100 (see Appendix Figure E-5) and top-50 export destinations (available upon request) . However, inclusion of three Covid dummies (for each of the years 2020-2022), as can be seen in Appendix Figure E-6, reduces the magnitude of this effect and renders it statistically insignificant.

In contrast to the national-level analysis of democratic backsliding in Boese-Schlosser et al. (2025), we find no evidence for sub-national units such as U.S. states being (knowingly or implicitly) punished by democratic destinations for democratic backsliding—note that Boese-Schlosser et al. (2025) find that countries experiencing backsliding episodes over the 1986-2014 period saw their exports drop by around 11%, driven by the drop in exports to democratic destinations. In the present paper, a seeming reduction in exports to autocratic countries turns insignificant when we account for the Covid years. In conclusion, it appears that foreign trading partners of the United States focus on *national* democratic institutions and ignore sub-national backsliding.

5 Concluding Remarks

This paper provides the first systematic evidence that democratic backsliding within U.S. states has significant economic consequences. While earlier literature has focused primarily on national-level autocratization in developing countries, we show that even in a consolidated democracy like the United States, the erosion of democratic institutions at the sub-national level can harm economic outcomes, both immediately and in the long term.

We examine the economic consequences of democratic backsliding within U.S. states between 2000 and 2023. Leveraging the State Democracy Index (Grumbach, 2023, SDI) to capture sub-national democratic decline, we combine panel data on all 50 states with a difference-in-differences design to identify causal effects of backsliding episodes, which we construct from the SDI. Our results show that the economic costs are particularly salient for vulnerable populations: we find robust increases in impoverishment, as well as indications of rising inequality. Most strikingly, backsliding appears to undermine the foundations of long-run growth by reducing investment in research and development and depressing innovation, especially in commercially relevant patent categories.

Prior studies have shown that national-level democratic breakdowns often trigger international economic repercussions, as other democracies reduce trade and investment in a form of 'accountability through trade.' In contrast, our findings suggest that this mechanism does not operate at the subnational level: in the U.S. context, where individual states are embedded within a stable federal system, democratic backsliding at the sub-national level has not been associated with any decline in international exports. This suggests that foreign trading partners primarily assess risk through national-level institutions such as federal courts and overarching rule-of-law guarantees, effectively insulating backsliding U.S. states from external economic penalties. Consequently, while national autocratization may trigger international responses through trade disruptions, sub-national erosion of democracy unfolds largely under the protective umbrella of national stability, leaving internal economic consequences as the primary channel of cost.

In the present analysis, we have focused on the general notion of democratic backsliding by adopting Grumbach's (2023) democracy index in combination with some simple rules to create variants of backsliding episodes. Future research could revisit the data underlying the Grumbach SDI and 'drill down' to constituent components to seek to answer the question which particular type of meddling with electoral democracy (voter suppression, gerrymandering, etc.) leads to the economic malaise we have documented.

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Online Appendix – Not Intended for Publication

A State Democracy Index

To identify episodes of autocratization in U.S. states, we rely on the State Democracy Index (SDI) developed by Grumbach (2023), which tracks changes in the quality of democratic institutions at the subnational level between 2000 and 2023. This index is constructed from 51 indicators across multiple domains of electoral democracy and is publicly available. For a full breakdown of all individual indicators and their sources, see Grumbach (2023), Supplementary Information, Table A1.

The SDI is based on indicators capturing changes in election laws and practices, voting access, and electoral integrity such as: voter registration, voter identification laws, early voting and absentee voting access, vote-by-mail policies, polling place availability, pre-registration and automatic registration, felony disenfranchisement, gerrymandering (partisan bias in districting), voter roll purging, access to registration (e.g., online registration), registration deadlines, state election administration partisanship, mail ballot verification rules, ballot collection restrictions, signature matching policies.

Interpretation and Scope

The SDI is specifically designed for use within the United States between 2000 and 2023, a context in which other baseline features of electoral democracy — such as civil liberties, separation of powers, and judicial independence — were established. As such, it is not a generalizable measure of electoral democracy suitable for cross-national or historical comparisons. Instead, it should be understood as a tool to capture subnational variation in the quality of democratic institutions within an already democratic federal system, rather than a measure to assess whether electoral democracy exists in the first place.

To illustrate this distinction, consider how other prominent democracy indices, such as the V-Dem measure of electoral democracy, include additional components that are absent in the SDI — such as freedom of expression, independent media, and the ability of civil society to operate freely. These elements are central to the functioning of electoral democracy in a cross-national context, but are implicitly assumed to be present in the U.S. federal system. This highlights that Grumbach's index does not operationalize electoral democracy per se, but instead reflects fluctuations in its quality across U.S. states. Recognizing this distinction is conceptually important for understanding what the index captures — and what it does not.

One key advantage of the SDI is that it captures both *de jure* aspects of electoral democracy (e.g., automatic voter registration, early voting laws) and *de facto* outcomes (e.g., gerrymandering bias measures, felony disenfranchisement rates, ballot rejection rates), allowing for a comprehensive assessment of democratic quality at the state level.

For our purposes, episodes of democratization and autocratization refer to substantive changes in the composite index derived from these indicators (as discussed in the main section of the paper).

In conclusion, while the measure is not suited for cross-national comparisons, it provides a valid and well-grounded instrument for studying subnational variation in democratic quality across U.S. states, i.e. within a federal democracy and during the sample period.

B Data, Sample Makeup, and Descriptives

Table B-1: Distribution and Length of Backsliding Episodes

Definition	**'Broad'** # Episodes				**'Intermediate'** # Episodes				**'Narrow'** # Episodes		
Years	1	2	3	Total	1	2	3	Total	1	2	Total
1	3			3				0			0
2	2			2				0			0
3	3			3	2			2	2		2
4/**LQ**	4			4	4			4	2		2
5	1	2		3	1			1	2		2
6	1	2		3	2			2	2		2
7/**MD**	2	2		4	3			3	2		2
8	1			1	1			1	1		1
9	1			1	1			1	1		1
10		10		10		4		4		2	2
11/**UQ**	2	2	3	7	2	2		4	2	2	4
12	2			2	2			2	2		2
13			3	3			3	3			
Total episodes	22	18	6	46	18	6	3	27	16	4	20
Total states	22	9	2	33	18	3	1	22	16	2	18

Notes: We present details of the distribution and length of backsliding episodes based on our three definitions (broad, intermediate, narrow) in the respective three blocks of columns. The numbers in these columns represent frequencies (number of episodes), while the rows represent the number of years states spend in these episodes. Starting in the top-left corner of the table, the entry '3' indicates that three states each experienced single-year episodes exactly once using the 'Broad' definition of backsliding. The eye-catching entry of '10' in the row labelled '10' and the column labelled '2' indicates that there were five states, each experienced two episodes that, in each state, lasted a combined total of ten years (the distribution across episodes is not reported here). While any entry into a column labelled '1' can be read as the number of states (as well as the number of episodes), entries in columns labelled '2' and '3' are episode counts and hence need to be divided by two and three, respectively, to arrive at the number of states. We report in bold the lower quartile (LQ), median (MD), and upper quartile (UQ) years spent in an episode (treated samples only). The final two rows of the table report the total number of episodes as well as the total number of states distributed across the number of episodes. Hence, 17, 28, and 32 states experienced no episode at all using the broad, intermediate, and narrow definition of backsliding, respectively.

Table B-2: Sample Makeup — Democratic Quality

		Rescaled Grumbach SDI				'Broad' Episode			'Intermed.' Episode			'Narrow' Episode			Always
ID	State	2000	2023	Δ	%Δ	Ep	Years	Sample	Ep	Years	Sample	Ep	Years	Sample	T/C
AK	Alaska	0.72	0.74	0.016	2.2	0	0	control	0	0	control	0	0	control	control
AL	Alabama	0.43	0.22	-0.211	-48.6	1	9	treated	1	9	treated	1	9	treated	treated
AR	Arkansas	0.69	0.37	-0.319	-46.0	1	8	treated	1	8	treated	1	8	treated	treated
AZ	Arizona	0.69	0.78	0.095	13.9	1	2	treated	0	0	control	0	0	control	
CA	California	0.85	0.90	0.050	5.8	1	2	treated	0	0	control	0	0	control	
CO	Colorado	0.90	0.95	0.046	5.1	0	0	control	0	0	control	0	0	control	control
CT	Connecticut	0.72	0.80	0.082	11.3	2	6	treated	0	0	control	0	0	control	
DE	Delaware	0.60	0.70	0.106	17.7	0	0	control	0	0	control	0	0	control	control
FL	Florida	0.52	0.61	0.091	17.6	1	4	treated	1	4	treated	1	4	treated	treated
GA	Georgia	0.56	0.43	-0.127	-22.8	1	12	treated	1	12	treated	1	12	treated	treated
HI	Hawaii	0.79	0.75	-0.043	-5.4	2	10	treated	1	4	treated	0	0	control	
IA	Iowa	0.83	0.75	-0.084	-10.1	1	1	treated	0	0	control	0	0	control	
ID	Idaho	0.62	0.52	-0.101	-16.3	1	7	treated	1	7	treated	0	0	control	
IL	Illinois	0.61	0.93	0.319	52.1	0	0	control	0	0	control	0	0	control	control
IN	Indiana	0.71	0.38	-0.325	-45.8	1	3	treated	1	3	treated	1	3	treated	treated
KS	Kansas	0.64	0.72	0.072	11.2	1	4	treated	0	0	control	0	0	control	
KY	Kentucky	0.73	0.44	-0.290	-39.7	3	11	treated	1	4	treated	0	0	control	
LA	Louisiana	0.55	0.58	0.028	5.1	0	0	control	0	0	control	0	0	control	control
MA	Massachusetts	0.67	0.76	0.088	13.1	0	0	control	0	0	control	0	0	control	control
MD	Maryland	0.75	0.62	-0.129	-17.2	1	1	treated	0	0	control	0	0	control	
ME	Maine	0.90	0.91	0.010	1.2	0	0	control	0	0	control	0	0	control	control
MI	Michigan	0.50	0.78	0.273	54.1	1	5	treated	1	5	treated	1	5	treated	treated
MN	Minnesota	0.79	0.90	0.108	13.6	0	0	control	0	0	control	0	0	control	control
MO	Missouri	0.68	0.38	-0.295	-43.6	2	10	treated	2	10	treated	1	6	treated	treated
MS	Mississippi	0.66	0.36	-0.307	-46.3	1	11	treated	1	11	treated	1	11	treated	treated
MT	Montana	0.83	0.85	0.022	2.6	1	3	treated	0	0	control	0	0	control	
NC	North Carolina	0.75	0.61	-0.138	-18.5	1	4	treated	1	4	treated	1	4	treated	treated
ND	North Dakota	0.88	0.79	-0.093	-10.6	0	0	control	0	0	control	0	0	control	control
NE	Nebraska	0.70	0.70	0.004	0.6	0	0	control	0	0	control	0	0	control	control
NH	New Hampshire	0.80	0.65	-0.148	-18.6	0	0	control	0	0	control	0	0	control	control
NJ	New Jersey	0.67	0.92	0.248	37.1	1	1	treated	0	0	control	0	0	control	
NM	New Mexico	0.82	0.84	0.022	2.7	0	0	control	0	0	control	0	0	control	control
NV	Nevada	0.77	0.76	-0.017	-2.2	2	5	treated	0	0	control	0	0	control	
NY	New York	0.56	0.80	0.245	44.0	0	0	control	0	0	control	0	0	control	control
OH	Ohio	0.58	0.49	-0.093	-15.9	1	3	treated	1	3	treated	1	3	treated	treated
OK	Oklahoma	0.75	0.45	-0.301	-40.1	1	7	treated	1	7	treated	1	7	treated	treated
OR	Oregon	0.87	0.97	0.101	11.7	0	0	control	0	0	control	0	0	control	control
PA	Pennsylvania	0.70	0.81	0.112	16.1	2	10	treated	1	7	treated	1	7	treated	treated
RI	Rhode Island	0.62	0.71	0.093	15.1	1	6	treated	0	0	control	0	0	control	
SC	South Carolina	0.52	0.42	-0.102	-19.5	1	11	treated	1	11	treated	1	11	treated	treated
SD	South Dakota	0.77	0.68	-0.084	-10.9	1	4	treated	0	0	control	0	0	control	
TN	Tennessee	0.57	0.10	-0.471	-82.6	1	12	treated	1	12	treated	1	12	treated	treated
TX	Texas	0.71	0.57	-0.139	-19.6	3	13	treated	3	13	treated	1	5	treated	treated
UT	Utah	0.88	0.58	-0.303	-34.3	2	10	treated	2	10	treated	2	10	treated	treated
VA	Virginia	0.71	0.87	0.152	21.3	2	11	treated	2	11	treated	2	11	treated	treated
VT	Vermont	0.77	0.78	0.009	1.2	0	0	control	0	0	control	0	0	control	control
WA	Washington	0.89	1.00	0.105	11.7	0	0	control	0	0	control	0	0	control	control
WI	Wisconsin	0.81	0.26	-0.551	-68.2	2	10	treated	1	6	treated	1	6	treated	treated
WV	West Virginia	0.70	0.59	-0.105	-15.1	0	0	control	0	0	control	0	0	control	control
WY	Wyoming	0.85	0.55	-0.296	-35.0	2	7	treated	1	6	treated	0	0	control	
	Average	0.71	0.66	-0.05	-6.89	0.92	4.46		0.54	3.34		0.40	2.68		

Notes: We present sample statistics related to state democracy. A first block of columns after the state identifier and name covers the Grumbach (2023) State Democracy Index (SDI), which we rescale to [0,1]. We report the rescaled index values for 2000 and 2023 as well as the total change over these 24 years and the percentage change (%Δ). The next three blocks of columns present details of our three alternative definitions of 'democratic backsliding': a liberal, intermediate, and conservative version. In each case, we report the episode count (Ep), the total years spent in episode(s) (Years), the sample affiliation of the state (treated or control). For the intermediate and conservative definitions, we highlight which states switch from treated to control samples relative to the liberal and intermediate definitions, respectively. The final column indicates states that are either always in the treated or always in the control sample.

Table B-3: Sample Makeup — Macroeconomic Variables

		Real GDP pc (US$)		**MD HH Inc** (US$)		**Poverty Rate** (%)		**ΔEmploym** (%)		**ΔPopulation** (%)	
ID	State	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
AK	Alaska	51,109	5,683	86,697	5,859	10.46	1.89	0.85	1.74	0.57	5.30
AL	Alabama	38,738	3,011	58,579	3,612	15.43	1.30	0.88	1.71	0.50	2.27
AR	Arkansas	38,336	4,652	55,241	4,036	16.43	1.92	0.84	1.30	0.03	2.41
AZ	Arizona	41,634	4,639	69,279	6,751	15.19	3.24	2.03	2.37	1.21	2.12
CA	California	53,226	8,472	80,384	6,391	13.37	1.78	1.24	2.15	0.69	2.65
CO	Colorado	50,971	7,462	83,650	7,023	10.04	1.54	1.72	1.83	1.01	2.16
CT	Connecticut	67,772	4,501	89,197	4,379	9.13	1.17	0.65	1.60	0.02	2.58
DE	Delaware	50,540	1,509	76,319	6,031	9.99	2.18	1.25	1.76	0.52	3.29
FL	Florida	46,607	4,411	63,587	3,612	13.35	1.66	2.26	2.33	1.48	3.03
GA	Georgia	42,705	3,648	66,273	4,049	14.57	2.44	1.71	1.96	1.13	2.53
HI	Hawaii	48,310	3,309	86,421	7,983	10.18	1.60	0.92	3.13	0.71	3.92
IA	Iowa	44,522	4,200	72,256	5,713	9.65	0.97	0.49	1.29	0.72	2.52
ID	Idaho	38,907	4,872	68,534	6,281	11.22	2.08	2.03	2.14	1.40	3.14
IL	Illinois	51,146	4,149	75,745	6,984	11.48	1.66	0.52	1.78	0.13	2.96
IN	Indiana	42,574	4,193	67,618	6,094	11.91	2.43	0.65	1.83	0.48	3.10
KS	Kansas	45,743	4,825	70,310	7,974	11.45	2.30	0.58	1.33	0.55	2.26
KY	Kentucky	38,949	3,293	58,312	4,296	15.98	2.17	0.75	1.64	0.08	3.30
LA	Louisiana	42,324	3,055	55,876	3,882	18.50	2.24	0.77	1.73	-0.13	5.62
MA	Massachusetts	61,659	7,995	87,408	9,108	10.19	1.45	1.01	2.10	0.34	1.96
MD	Maryland	56,223	3,501	94,748	9,095	8.75	1.11	1.16	1.55	0.97	2.26
ME	Maine	44,548	4,008	66,783	6,139	11.37	1.73	0.64	1.64	0.27	1.98
MI	Michigan	43,307	4,065	69,499	4,437	12.37	1.70	0.34	2.42	-0.28	3.47
MN	Minnesota	50,857	4,990	84,601	6,368	8.38	1.64	0.75	1.69	0.55	2.50
MO	Missouri	43,573	3,669	68,579	4,774	11.91	1.99	0.63	1.39	0.06	2.29
MS	Mississippi	35,455	2,776	51,283	2,643	19.38	2.30	0.62	1.42	-0.08	2.74
MT	Montana	42,022	5,403	62,112	8,255	12.55	2.02	1.39	1.45	0.67	2.82
NC	North Carolina	42,880	4,109	62,250	5,186	14.63	1.79	1.43	1.88	0.98	2.56
ND	North Dakota	48,063	8,541	69,982	7,525	10.54	1.27	1.32	2.37	1.61	5.08
NE	Nebraska	47,965	4,754	74,148	6,892	9.85	1.07	0.79	1.07	1.11	2.27
NH	New Hampshire	54,768	5,900	91,755	5,617	6.49	1.20	0.91	1.70	0.35	2.53
NJ	New Jersey	59,755	4,386	89,223	6,453	8.92	1.43	1.16	2.03	0.25	2.49
NM	New Mexico	37,305	4,277	58,434	2,483	18.25	1.89	0.89	1.81	-0.01	3.84
NV	Nevada	46,908	3,465	71,298	6,206	12.26	2.79	2.41	3.61	1.90	3.63
NY	New York	58,469	5,306	72,272	6,192	13.88	1.57	1.08	2.19	0.50	2.65
OH	Ohio	43,755	3,698	67,292	5,056	12.58	1.75	0.42	1.64	0.17	2.68
OK	Oklahoma	40,530	5,528	61,473	4,321	14.33	1.81	0.95	1.50	0.48	2.99
OR	Oregon	43,519	6,778	74,471	9,132	11.70	1.86	1.13	2.02	0.63	2.32
PA	Pennsylvania	48,706	4,953	72,292	5,633	11.08	1.24	0.75	1.66	0.14	2.79
RI	Rhode Island	49,155	3,678	75,542	6,441	11.09	1.76	0.77	2.05	0.07	2.86
SC	South Carolina	39,767	3,922	61,988	5,665	14.36	1.87	1.42	1.97	0.86	2.50
SD	South Dakota	47,624	6,117	68,299	6,650	11.52	2.06	1.12	1.09	0.82	3.45
TN	Tennessee	42,579	4,372	60,462	5,666	14.44	2.27	1.27	1.84	0.86	2.26
TX	Texas	44,283	5,752	68,765	5,929	15.33	1.82	2.25	1.59	1.75	3.37
UT	Utah	40,326	5,832	84,229	8,991	8.81	1.43	2.46	1.98	1.81	2.41
VA	Virginia	52,469	4,639	85,064	5,710	9.70	1.06	1.23	1.45	0.74	2.05
VT	Vermont	47,467	4,709	74,534	6,254	9.24	1.23	0.53	1.78	0.07	2.53
WA	Washington	52,420	7,868	82,565	9,189	10.34	1.61	1.45	2.00	1.24	2.52
WI	Wisconsin	46,110	4,020	73,306	3,870	9.93	1.49	0.58	1.58	0.27	2.40
WV	West Virginia	37,000	3,012	54,674	4,276	15.99	1.65	0.17	1.64	-0.13	3.43
WY	Wyoming	52,871	5,824	71,634	4,236	9.91	1.06	1.42	1.96	0.43	5.59
	Average	46,729	8,311	71,905	12,194	12.17	3.33	1.09	1.93	0.61	3.04

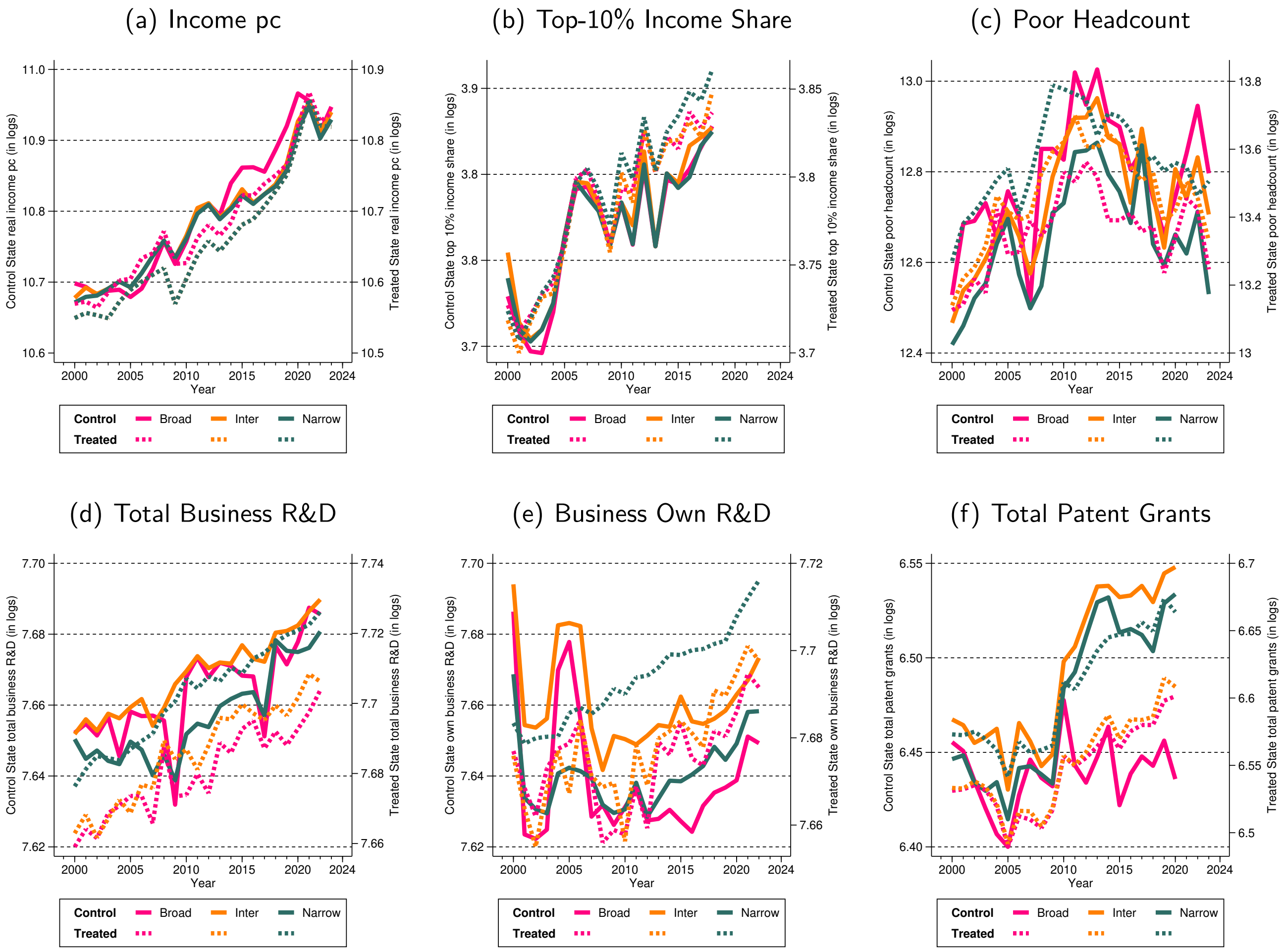
Notes: We present sample statistics (mean, standard deviation) related to state economic outcomes. Per GDP and Median (MD) household (HH) income are both in real values, poverty rate is the share of the state population living in poverty (in %), and the remaining columns report the annual growth rates of employment and population (in %).

Table B-4: Sample Makeup — Other Variables

		Inequality and Poverty Measures								Business R&D				Patent counts					
		Gini		Top 10% (%)		Top 1% (%)		Uninsured (%)		Total (exp/GDP)		Own (exp/GDP)		Applications '000		Granted '000		Utility only '000	
ID	State	M	SD	M	SD	M	SD	M	SD	M	SD	M	SD	M	SD	M	SD	M	SD
AL	Alabama	0.472	0.009	45.4	2.1	16.7	1.8	12.1	2.2	0.98	0.31	0.49	0.10	0.96	0.09	0.48	0.09	0.41	0.08
AK	Alaska	0.415	0.017	33.9	0.9	11.5	0.9	16.3	3.3	0.20	0.35	0.19	0.37	0.09	0.01	0.05	0.01	0.04	0.01
AZ	Arizona	0.457	0.010	48.1	2.4	18.5	2.2	14.8	3.6	1.77	0.25	1.38	0.16	4.52	0.78	2.26	0.56	2.06	0.52
AR	Arkansas	0.462	0.014	45.1	3.2	17.5	2.4	13.5	4.0	0.35	0.10	0.29	0.05	0.56	0.23	0.26	0.12	0.20	0.11
CA	California	0.477	0.012	50.1	2.6	22.4	2.6	13.9	5.4	4.13	1.10	3.69	1.07	70.87	17.15	32.21	11.37	29.20	10.67
CO	Colorado	0.451	0.011	45.4	2.4	18.6	2.0	12.5	4.1	1.69	0.26	1.48	0.31	5.48	0.81	2.69	0.68	2.40	0.60
CT	Connecticut	0.487	0.013	54.1	2.9	27.9	2.9	7.9	2.2	3.44	0.66	2.82	0.59	4.45	0.64	2.24	0.57	2.04	0.56
DE	Delaware	0.446	0.027	42.3	3.1	15.7	2.1	8.7	2.5	3.21	0.71	2.53	0.49	0.81	0.21	0.38	0.06	0.36	0.05
FL	Florida	0.475	0.011	55.9	4.9	27.1	4.1	16.8	3.7	0.67	0.07	0.47	0.07	8.58	1.54	3.95	1.03	3.25	0.92
GA	Georgia	0.470	0.013	46.4	2.7	17.9	1.7	15.8	2.6	0.83	0.16	0.71	0.10	4.97	0.93	2.24	0.72	1.94	0.63
HI	Hawaii	0.436	0.010	37.2	1.4	13.0	1.4	6.5	2.5	0.28	0.10	0.20	0.09	0.27	0.06	0.12	0.03	0.10	0.03
ID	Idaho	0.433	0.014	41.3	2.5	15.8	2.3	13.8	3.4	2.21	0.60	1.93	0.57	2.13	0.73	1.27	0.37	1.22	0.38
IL	Illinois	0.471	0.011	48.0	2.2	20.7	2.0	10.9	3.3	1.71	0.18	1.60	0.21	9.50	1.37	4.76	1.02	4.02	0.95
IN	Indiana	0.439	0.015	40.6	1.5	14.6	1.1	11.3	2.8	1.94	0.22	1.72	0.18	3.54	0.56	1.86	0.47	1.62	0.44
IA	Iowa	0.431	0.013	37.8	1.6	13.1	1.1	7.3	2.2	1.24	0.35	1.02	0.20	1.65	0.27	0.87	0.20	0.81	0.20
KS	Kansas	0.447	0.012	41.1	1.2	15.9	1.4	10.7	1.6	1.46	0.22	0.97	0.09	1.44	0.28	0.73	0.24	0.64	0.24
KY	Kentucky	0.468	0.011	43.5	2.0	15.3	1.2	10.6	4.1	0.58	0.09	0.47	0.08	1.22	0.20	0.60	0.15	0.54	0.13
LA	Louisiana	0.484	0.011	43.8	3.1	16.8	2.6	14.5	4.9	0.18	0.04	0.15	0.04	0.84	0.08	0.43	0.09	0.38	0.08
ME	Maine	0.443	0.013	41.2	1.8	14.1	1.1	9.3	1.7	0.60	0.13	0.53	0.13	0.38	0.05	0.19	0.04	0.17	0.04
MD	Maryland	0.446	0.010	41.7	1.3	15.7	1.4	9.6	3.1	1.51	0.21	0.96	0.17	3.87	0.39	1.76	0.34	1.61	0.33
MA	Massachusetts	0.475	0.013	51.7	3.2	23.6	3.0	5.4	3.2	4.48	0.90	3.66	0.81	12.95	3.02	5.62	1.88	5.24	1.78
MI	Michigan	0.453	0.014	45.8	3.4	16.9	2.0	8.8	2.9	4.00	0.46	3.73	0.58	8.91	1.55	5.13	1.49	4.57	1.31
MN	Minnesota	0.439	0.015	43.4	1.6	17.1	1.2	6.9	2.0	2.22	0.24	2.08	0.23	7.45	1.03	3.85	0.89	3.48	0.81
MS	Mississippi	0.474	0.012	43.5	2.1	15.0	1.3	15.1	2.9	0.31	0.25	0.21	0.05	0.33	0.03	0.18	0.03	0.15	0.02
MO	Missouri	0.453	0.014	43.8	2.1	17.0	1.5	11.1	1.8	1.69	0.66	1.17	0.16	2.38	0.41	1.13	0.29	0.98	0.28
MT	Montana	0.447	0.015	42.5	1.4	15.8	1.3	13.9	4.4	0.35	0.10	0.32	0.09	0.32	0.10	0.14	0.03	0.12	0.02
NE	Nebraska	0.438	0.012	37.9	1.6	14.9	1.6	9.7	1.8	0.58	0.10	0.55	0.09	0.60	0.10	0.30	0.07	0.25	0.07
NV	Nevada	0.448	0.014	52.8	3.9	27.0	3.8	16.3	4.3	0.46	0.11	0.40	0.08	1.68	0.34	0.66	0.24	0.56	0.22
NH	New Hampshire	0.428	0.014	42.4	2.2	16.7	1.9	8.4	2.4	2.74	0.58	1.25	0.17	1.57	0.21	0.81	0.19	0.73	0.18
NJ	New Jersey	0.468	0.013	49.0	1.3	20.3	1.4	11.2	3.0	3.13	0.45	2.77	0.47	8.94	1.06	4.29	0.79	3.82	0.75
NM	New Mexico	0.469	0.012	43.4	3.2	14.6	1.9	16.3	5.7	0.82	0.40	0.45	0.27	0.86	0.11	0.42	0.09	0.40	0.08
NY	New York	0.503	0.011	57.3	3.0	29.9	3.0	10.1	4.0	1.09	0.18	0.94	0.20	15.75	2.27	8.05	1.76	7.11	1.62
NC	North Carolina	0.466	0.011	44.2	2.7	16.1	1.5	13.7	2.8	1.67	0.36	1.35	0.16	6.32	1.44	2.90	0.82	2.61	0.80
ND	North Dakota	0.446	0.014	38.7	2.7	14.7	2.6	9.1	1.9	0.68	0.38	0.68	0.39	0.21	0.04	0.10	0.02	0.09	0.02
OH	Ohio	0.454	0.014	42.0	1.8	15.4	1.2			1.51	0.11	1.18	0.18	7.95	0.99	3.98	0.70	3.28	0.68
OK	Oklahoma	0.460	0.010	42.0	1.6	16.8	2.0	16.6	2.7	0.42	0.11	0.39	0.11	1.09	0.11	0.57	0.07	0.51	0.06
OR	Oregon	0.452	0.010	44.9	2.2	16.1	1.5	12.0	4.6	2.85	0.78	2.77	0.76	5.03	1.01	2.57	0.82	2.11	0.59
PA	Pennsylvania	0.462	0.012	44.9	1.6	17.6	1.4	8.3	2.3	1.78	0.23	1.63	0.22	7.92	0.86	3.83	0.67	3.42	0.63
RI	Rhode Island	0.461	0.014	44.6	1.4	16.8	1.3			1.70	0.93	0.99	0.17	0.73	0.09	0.38	0.06	0.31	0.05
SC	South Carolina	0.465	0.009	45.3	2.6	16.1	1.7	13.3	2.9	0.76	0.13	0.67	0.14	1.72	0.37	0.85	0.28	0.73	0.27
SD	South Dakota	0.440	0.012	39.7	2.3	16.5	1.8	10.7	1.4	0.32	0.06	0.28	0.06	0.24	0.06	0.11	0.04	0.10	0.03
TN	Tennessee	0.469	0.012	45.2	2.2	18.0	1.8	11.8	2.0	0.58	0.12	0.48	0.11	2.23	0.32	1.04	0.21	0.88	0.20
TX	Texas	0.473	0.008	47.7	2.7	20.8	2.5	21.1	3.2	1.23	0.12	1.08	0.14	17.72	3.70	8.57	2.52	8.00	2.31
UT	Utah	0.416	0.014	42.0	1.8	16.9	2.0	12.4	2.8	1.67	0.27	1.36	0.24	2.76	0.73	1.20	0.48	1.04	0.40
VT	Vermont	0.436	0.014	41.3	1.8	14.7	1.6	7.1	2.9	1.39	0.41	1.23	0.38	0.73	0.19	0.48	0.08	0.45	0.08
VA	Virginia	0.461	0.012	42.6	1.6	15.6	1.2	10.8	2.6	1.16	0.20	0.72	0.15	4.03	1.12	1.76	0.62	1.61	0.61
WA	Washington	0.448	0.014	45.8	2.4	19.0	2.2	10.6	3.6	4.51	1.24	4.37	1.25	12.05	3.71	5.22	2.38	4.72	2.23
WV	West Virginia	0.458	0.013	43.5	1.6	13.8	1.0	11.3	4.3	0.42	0.11	0.36	0.10	0.26	0.05	0.13	0.02	0.12	0.02
WI	Wisconsin	0.432	0.015	41.4	1.7	16.0	1.2	7.6	1.9	1.54	0.21	1.39	0.14	4.33	0.41	2.31	0.37	1.87	0.32
WY	Wyoming	0.429	0.015	49.6	10.2	28.5	10.5	13.6	1.9	0.34	0.65	0.32	0.65	0.20	0.06	0.09	0.03	0.08	0.03
	Average	0.455	0.022	44.4	5.3	17.7	4.8	11.7	4.5	1.51	1.24	1.26	1.12	5.27	10.61	2.52	5.02	2.25	4.57

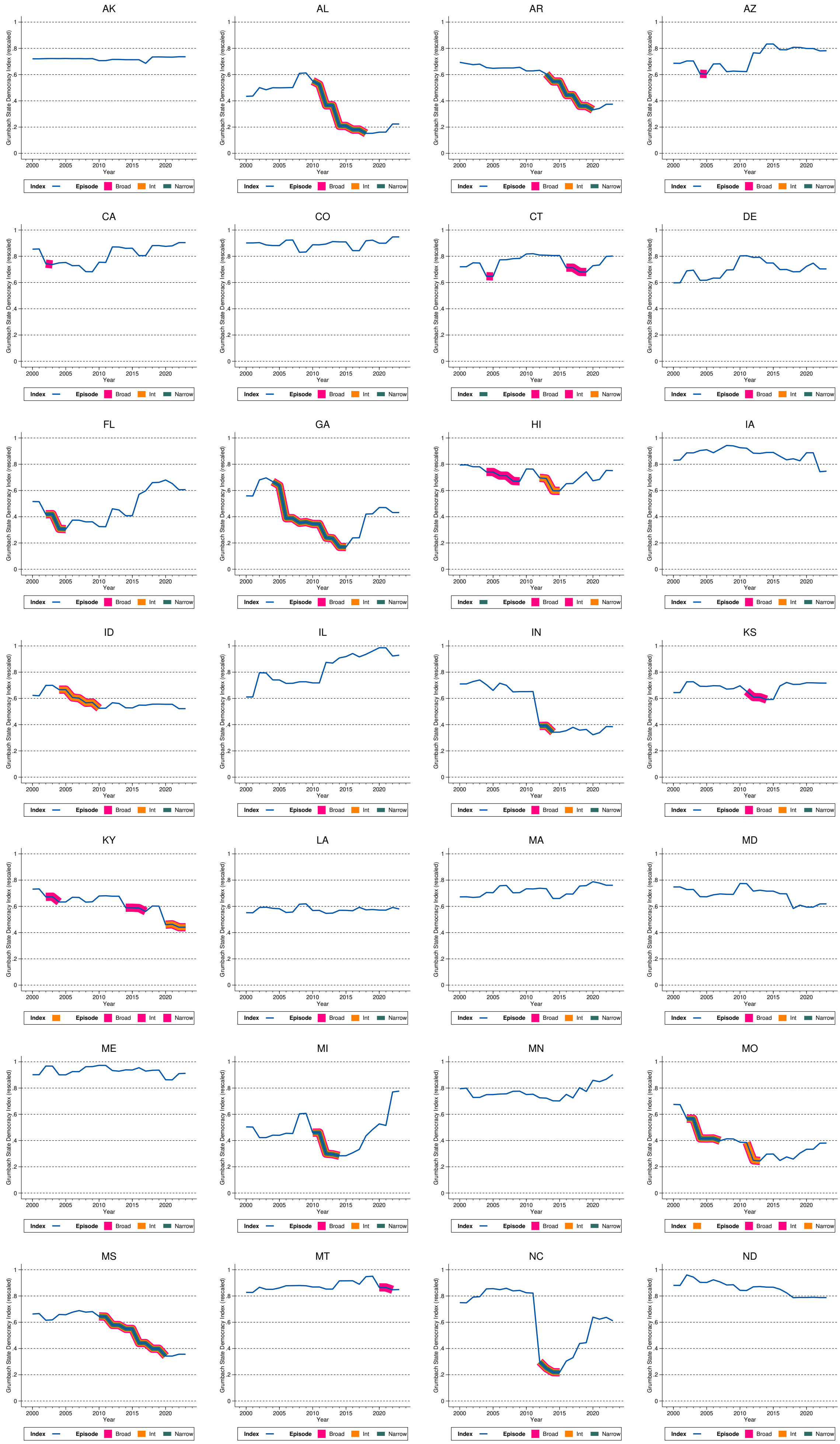
Notes: We present sample statistics (means, standard deviations) related to state economic outcomes. Inequality Measures — Gini coefficient, income share of the top-10% (top-1%) earners (in percent), share of population without insurance coverage (in percent). Business R&D expenditure expressed as a share of state GDP (in percent) — 'Total' includes funding from the federal government, businesses and organisations from outside the state (or abroad), among other sources; 'Own' covers only the expenditure made by the firm from its own sources. Patent counts (in thousands) — patent applications (all types); patents granted (dto); utility patents granted. All statistics refer to the annual averages by state. Data for the share of population without insurance are missing for Ohio and Rhode Island.

Figure B-1: Average Evolution of Main Outcomes by Treated vs Control Sample



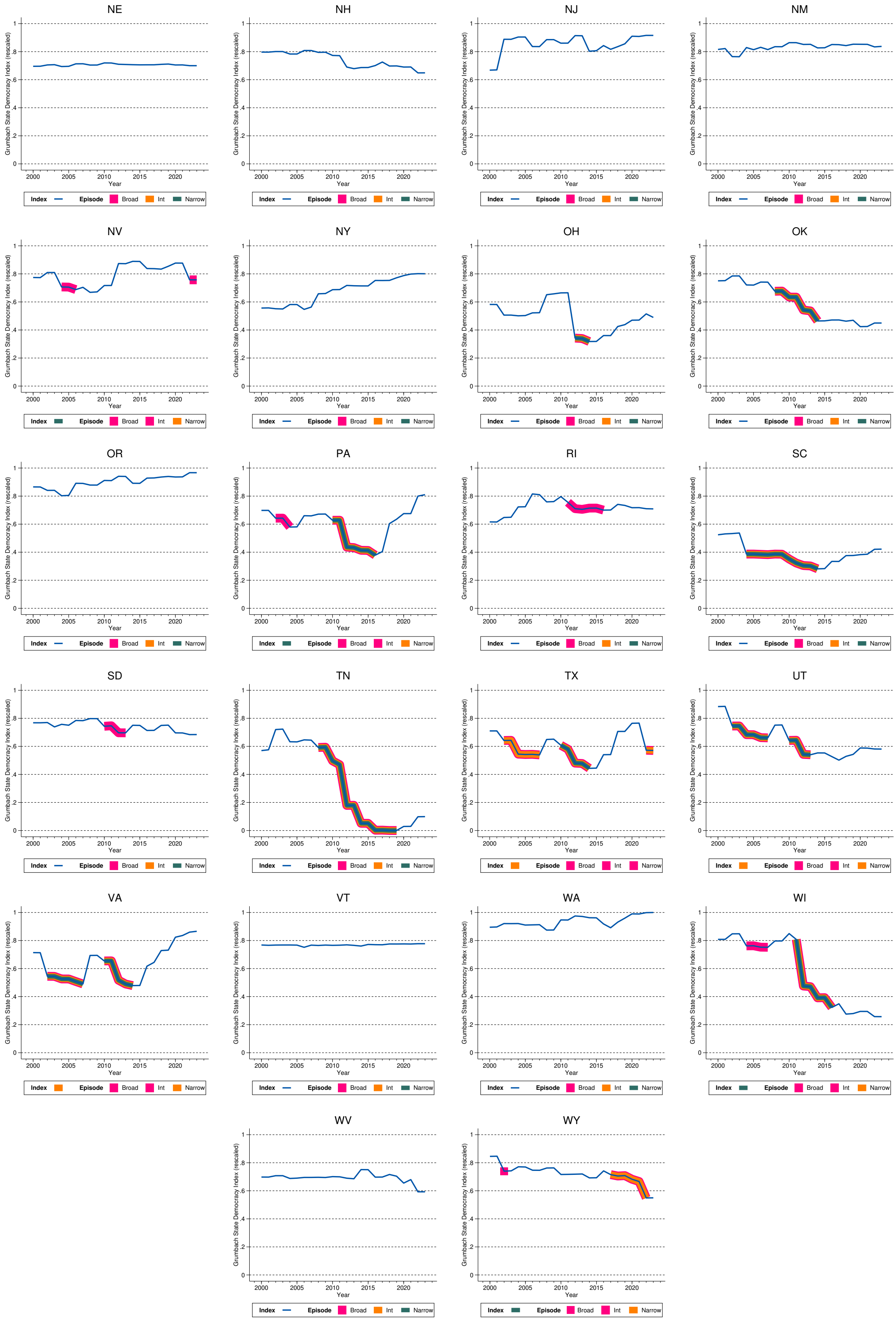
Notes: We plot the median evolution of our primary economic outcomes of interest for treated (dashed lines, right-hand scales) and control (solid lines, left-hand scales) states over the sample period.

Figure B-2: Democratic Backsliding Episodes — Illustration by State



(continued overleaf)

Figure B-2: Democratic Backsliding Episodes — Illustration by State (cont'd)



Notes: These plots present the time series evolution of each (rescaled) Grumbach (2023) democracy index (in blue) by state. They further highlight the backsliding episodes as determined by our three alternative definitions: pink bands for 'liberal', orange for 'intermediate', and emerald for 'conservative'. For instance, Alaska (AK) experienced no episodes under any definition, Alabama (AL) one lengthy episode in any of the definitions, and Arizona (AZ) one single-year episode in the 'liberal' definition but not in either of the other two.

C Episodes and State Party Partisanship

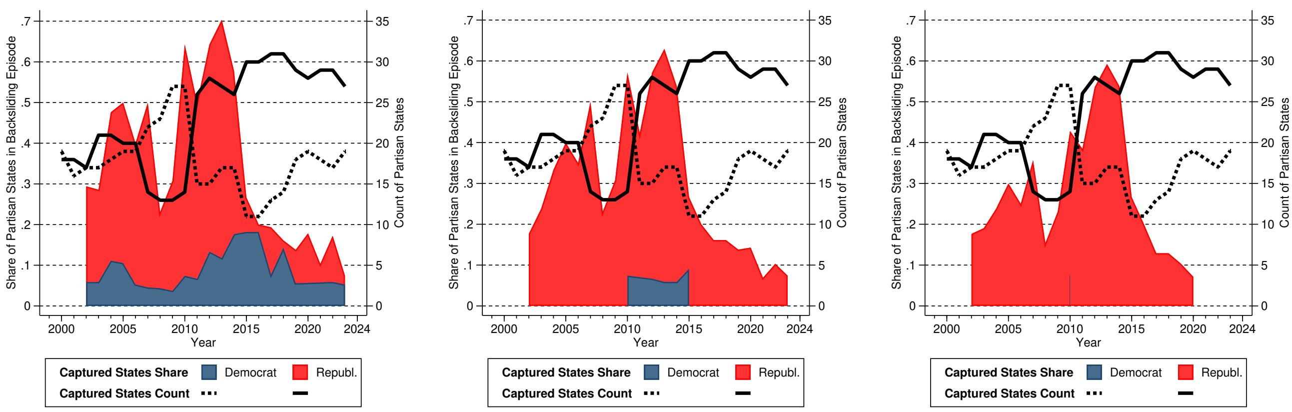
Table C-1: Episodes and Political Parties

Party in Power during Episode	Backsliding Definition: Broad	Intermed.	Narrow
Pure Republican	**24**	**16**	**11**
	52%	59%	55%
One year Democrat then Republican	2	2	2
One year Mixed then Repub.	5	5	4
One Year Dem and Mixed, resp., then Repub.	1	1	1
Two or more years Mixed then Republican	3	2	2
	24%	37%	45%
All Republican	**35**	**26**	**20**
	76%	96%	100%
Pure Mixed	**2**	**0**	**0**
	4%	0%	0%
Pure Democrat	**9**	**1**	**0**
	20%	4%	0%
Total Episodes	46	27	20

Notes: We present the distribution of backsliding episodes across parties dominating state politics. We assign a state-year as (a) Democrat or Republican if the respective party has majorities in both the state house and senate, and as (b) Mixed if the house and senate are dominated by different parties or there is power sharing. We distinguish a number of typologies for the case of the Republican party, when episodes occur during the party's dominance but start off either in Mixed regimes or under a Democratic majority. Data Source: https://ballotpedia.org, Historical partisan composition of state legislatures (accessed June 10, 2025).

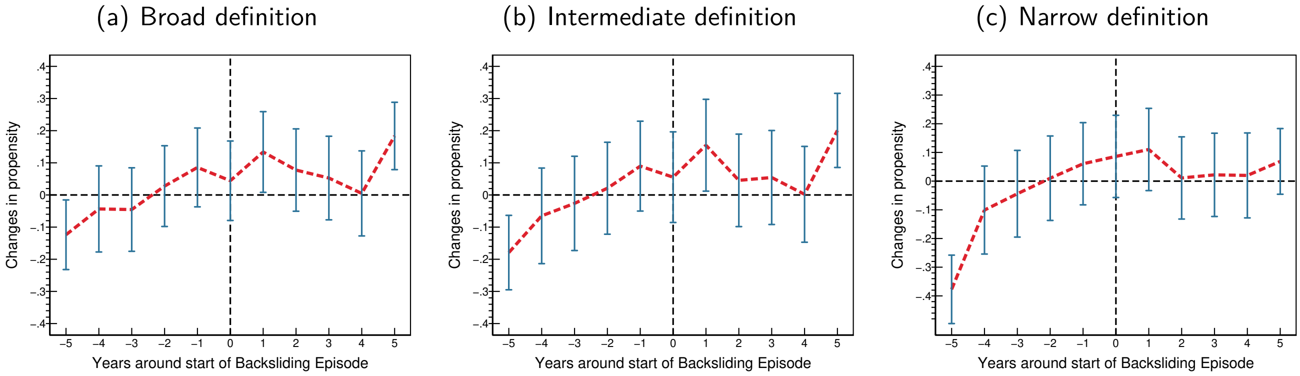
Figure C-1: Captured States and Backsliding Episodes

(a) Broad definition (b) Intermediate definition (c) Narrow definition



Notes: The plots chart the number of states captured by the Republicans (solid black line) and the Democrats (dashed black line), using the right scales. The red (blue) areas highlight, using the left scales, the share of states captured by the Republicans (Democrats) in year t which were experiencing a backsliding episode. The three plots are for the broad, intermediate, and narrow definition of backsliding, respectively. For instance, in the left-most plot, 50% of the 20 states captured by the Republicans in 2005 experienced a backsliding episode in that year.

Figure C-2: Republican State Capture and Backsliding Episodes: event analysis



Notes: The plots present univariate event analyses for the backsliding episode and the dummy for Republican state capture. The red line shows the propensity of a state being captured by the Republicans (estimated using robust regression). Blue bars are 90% CI. Regressions only include states that experienced a backsliding episode.

Table C-2: Backsliding Episodes and State Party Partisanship

Backsliding Definition	(1) Broad	(2)	(3) Intermediate	(4)	(5) Narrow	(6)
Republican Capture	20.33***	10.79**	20.18***	11.82**	16.70***	9.88**
(47% of obs)	(4.41)	(4.95)	(4.68)	(4.75)	(3.53)	(3.72)
Democrat Capture	-8.86**	-5.03	-8.66**	-5.30	-9.84**	-7.10*
(36% of obs)	(4.04)	(4.18)	(3.85)	(3.93)	(3.77)	(3.81)
State FE	×	×	×	×	×	×
Grumbach Index		×		×		×

Notes: We present the results from simple linear probability models of backsliding — all coefficients represent marginal effects and are multiplied by 100. Standard errors (in parentheses) are clustered at the state level. In addition to state and year fixed effects, the regression includes two dummies for state-years when Republicans, respectively Democrats, enjoy the majority in both the state house and senate — no majority is the omitted category. The models in even columns include the rescaled Grumbach Index as additional control. Each regression includes 1,200 observations for 50 states.

D Diagnostic Testing

Table D-1: Alpha and Wald Test Results

			Democratic Backsliding: Definition						
			Broad		Intermed.		Narrow		
Dependent Variable	**Specification**	**Statistic**	f=4	f=5	f=4	f=5	f=4	f=5	**Figure**
Panel A: Average incomes									
GDP per capita	No controls	Alpha t ratio	-1.673		-0.633		-0.169		E-2 (c)
	Control: Employment Growth Rate	Alpha t ratio	-1.702		-1.073		-0.604		3 (a)
		Wald p value	0.90	0.91	0.31	0.23	0.76	0.32	
GDP per worker	No controls	Alpha t ratio	-3.423		-2.872		-1.969		E-3 (a)
	Control: Employment Growth Rate	Alpha t ratio	-3.389		-2.962		-2.068		E-3 (a)
		Wald p value	0.01	0.00	0.00	0.00	0.00	0.00	
Panel B: Inequality									
Median HH income	No controls	Alpha t ratio	-1.816		-3.392		-2.881		E-2 (c)
	Control: Employment Growth Rate	Alpha t ratio	-1.893		-3.409		-2.833		E-2 (b)
		Wald p value	0.39	0.82	0.71	0.79	0.86	0.47	
Gini Coefficient	Control: Employment Growth Rate	Alpha t ratio	-1.304		-0.608		0.212		E-3 (b)
		Wald p value	0.52	0.12	0.97	0.49	0.98	0.95	
Top-10% Income Share	No controls	Alpha t ratio	1.409		1.558		0.324		E-4 (a)
	Control: Employment Growth Rate	Alpha t ratio	2.046		2.315		1.329		AOR
		Wald p value	0.00	0.00	0.00	0.00	0.00	0.00	
Top-1% Income Share	No controls	Alpha t ratio	1.354		0.845		-1.497		3 (b)
	Control: Employment Growth Rate	Alpha t ratio	1.764		1.350		-0.705		AOR
		Wald p value	0.00	0.00	0.00	0.00	0.00	0.00	
Panel C: Poverty									
Poverty headcount	No controls	Alpha t ratio	1.074		-0.632		-0.566		E-2 (c)
	Control: Employment Growth Rate	Alpha t ratio	0.986		-0.306		-0.059		3 (c)
		Wald p value	0.09	0.04	0.85	0.84	0.99	0.99	
Poverty Rate	No controls	Alpha t ratio	1.392		-0.161		0.184		AOR
	Control: Employment Growth Rate	Alpha t ratio	0.900		-0.400		0.112		E-3 (c)
		Wald p value	0.27	0.42	0.67	0.62	0.37	0.25	
Uninsured headcount	No controls	Alpha t ratio	-2.938		-3.203		-3.421		AOR
	Control: Employment Growth Rate	Alpha t ratio	-2.905		-3.530		-3.734		E-3 (d)
		Wald p value	0.08	0.02	0.49	0.09	0.25	0.00	
Panel D: Innovation									
Total Business R&D real expend	No controls	Alpha t ratio	-0.068		0.512		-1.497		4 (a)
	Control: Employment Growth Rate	Alpha t ratio	-0.079		-0.337		-1.683		AOR
		Wald p value	0.00	0.00	0.01	0.02	0.02	0.00	
Business own R&D real expend	No controls	Alpha t ratio	-1.246		-0.688		-2.914		4 (b)
	Control: Employment Growth Rate	Alpha t ratio	-0.982		-0.778		-3.524		AOR
		Wald p value	0.00	0.00	0.00	0.00	0.01	0.01	
All Patent Applications	No controls	Alpha t ratio	0.382		0.238		1.708		AOR
	Control: Employment Growth Rate	Alpha t ratio	0.530		0.308		1.916		AOR
		Wald p value	0.00	0.00	0.11	0.03	0.07	0.93	
All Patents Granted	No controls	Alpha t ratio	1.722		0.700		2.560		4 (c)
	Control: Employment Growth Rate	Alpha t ratio	2.034		0.868		3.306		AOR
		Wald p value	0.00	0.00	0.18	0.21	0.04	0.81	
Utility Patent Granted	No controls	Alpha t ratio	1.708		0.681		2.777		4 (d)
	Control: Employment Growth Rate	Alpha t ratio	2.053		0.850		3.585		AOR
		Wald p value	0.00	0.00	0.05	0.06	0.01	0.47	

Notes: We present the diagnostic test results for the PCDID regressions underlying the running line plots we present in this paper. The Alpha test investigates average factor loading equality between treated and control samples (null: model not misspecified) and produces a t-ratio with the standard interpretation. The Wald test asks whether the additional control in the PCDID regression is a 'bad control' (in the sense of Angrist and Pischke, 2008), with the null that the control is fine — we report the p-value from this test. AOR — plots available on request.

Table D-2: Alpha and Wald Test Results (post-treatment effect models)

Dependent Variable	Specification	Statistic	Democratic Backsliding: Definition: Broad f=4	Broad f=5	Intermed. f=4	Intermed. f=5	Narrow f=4	Narrow f=5	Figure
State GDP per capita	Control: Employment Growth Rate	Alpha t ratio	-1.418		-1.397		-0.855		F-1 (a)
		Wald p value	0.90	0.91	0.31	0.23	0.76	0.32	
Top-10% Income Share	No controls	Alpha t ratio	0.510		0.707		-0.940		F-1 (b)
Poverty headcount	Control: Employment Growth Rate	Alpha t ratio	1.498		0.498		0.620		F-1 (c)
		Wald p value	0.09	0.04	0.85	0.84	0.99	0.99	
Total Business R&D real exp	No controls	Alpha t ratio	0.282		0.829		-0.784		F-1 (d)
Business own R&D real exp	No controls	Alpha t ratio	-0.879		-0.373		-2.504		F-1 (e)
All Patent Granted	No controls	Alpha t ratio	1.756		0.952		3.342		F-1 (f)
Utility Patent Granted	No controls	Alpha t ratio	1.726		1.044		4.099		F-1 (g)

Notes: We present the diagnostic test results for the PCDID regressions underlying the running line plots we present in this paper. This table focuses on the robustness analysis, including four post-treatment dummies in the treatment regressions. We use the same setup with regards to employment growth as additional control (or not) as in the main results in Figures 3 and 4 in the main text. For all other details, see notes to Appendix Table D-1.

Table D-3: Alpha Test Results (export flow analysis)

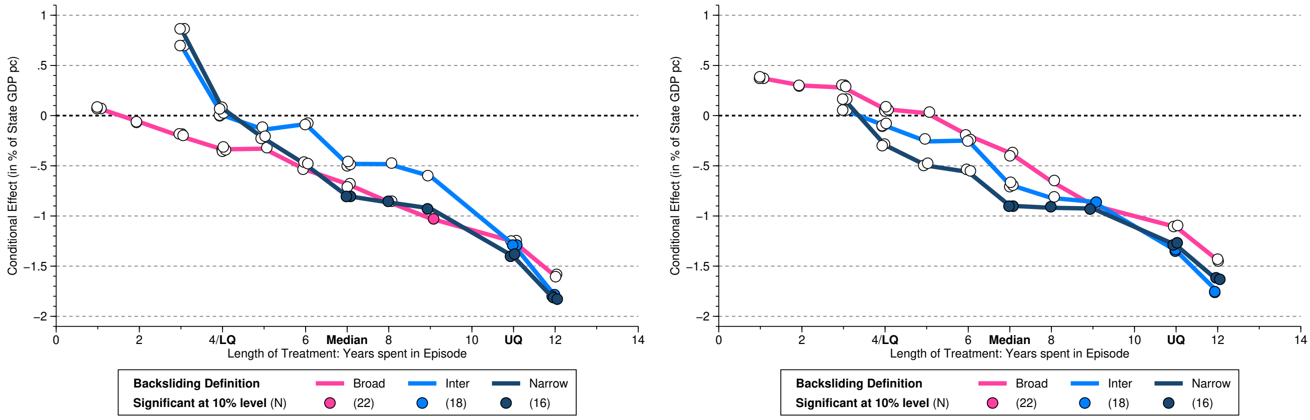
Dep. Var.	Destinations	US States	Destination Regime	Democratic Backsliding: Definition: Broad	Intermed.	Narrow	Figure
Exports	All	All	Any	0.635	0.036	0.043	5 (a) left
			Democracies	0.837	0.007	0.030	5 (b) left
			Autocracies	0.481	0.374	0.670	5 (c) left
Exports	All	Single episode	Any	0.556	0.039	0.314	5 (a) right
			Democracies	0.791	0.008	0.049	5 (b) right
			Autocracies	0.215	0.366	0.996	5 (c) right
Exports	Top-100	All	Any	0.243	0.728	0.089	E-5 (a) left
			Democracies	0.062	0.152	0.535	E-5 (b) left
			Autocracies	0.171	0.008	0.014	E-5 (c) left
Exports	Top-100	Single episode	Any	0.371	0.739	0.068	E-5 (a) right
			Democracies	0.060	0.150	0.486	E-5 (b) right
			Autocracies	0.607	0.004	0.005	E-5 (c) right
Exports	Top-50	All	Any	0.877	0.967	0.041	AOR
			Democracies	0.766	0.831	0.285	AOR
			Autocracies	0.299	0.604	0.026	AOR
Exports	Top-50	Single episode	Any	0.246	0.984	0.081	AOR
			Democracies	0.123	0.856	0.374	AOR
			Autocracies	0.435	0.631	0.055	AOR
Exports‡	All	All	Any	0.063	0.293	0.757	E-6 (a) left
			Democracies	0.166	0.732	0.116	E-6 (b) left
			Autocracies	0.163	0.442	0.563	E-6 (c) left
Exports‡	All	Single episode states	Any	0.075	0.258	0.929	E-6 (a) right
			Democracies	0.138	0.919	0.041	E-6 (b) right
			Autocracies	0.446	0.424	0.477	E-6 (c) right

Notes: We present the diagnostic test results for the PCDID regressions underlying the running line plots for state export flows we present in this paper. The Alpha test investigates average factor loading equality between treated and control samples (null: model not misspecified) and, in our implementation, produces a χ^2-statistic with two degrees of freedom — we report the associated p-value for this test. Results are available for all export destinations, the top-100 and the top-50 (based on ranks for 2002-2023); for all U.S. states which experienced one or more backsliding episodes and for those which experienced just a single episode; for any destination (country), and for those destinations that are democratic, respectively autocratic, throughout the sample period. AOR — plots available on request. ‡ indicates the diagnostics for specifications where we include year dummies (2020-22) for the Covid period.

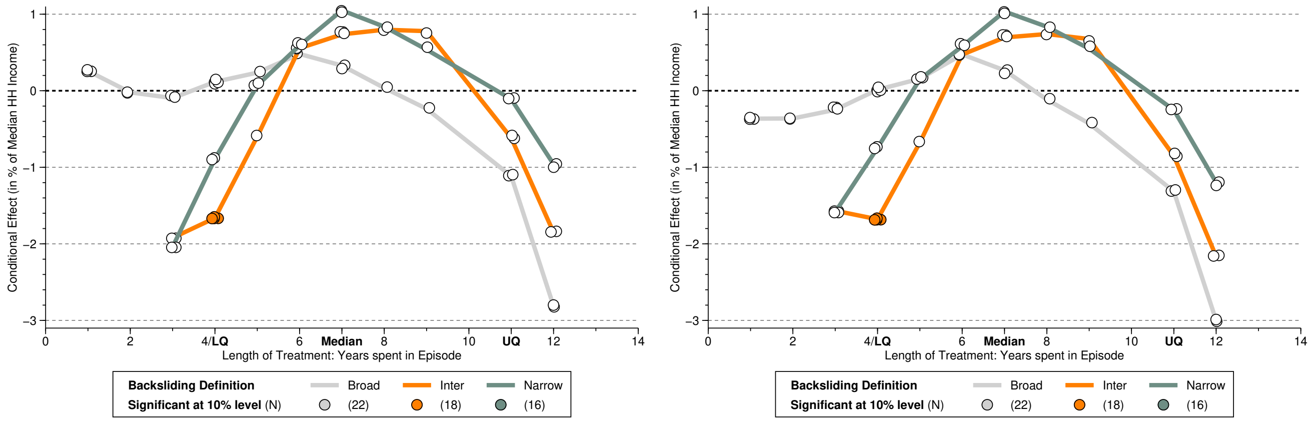
E Additional Results

Figure E-1: Democratic Backsliding Episodes and Economic Outcomes — Single Episode States

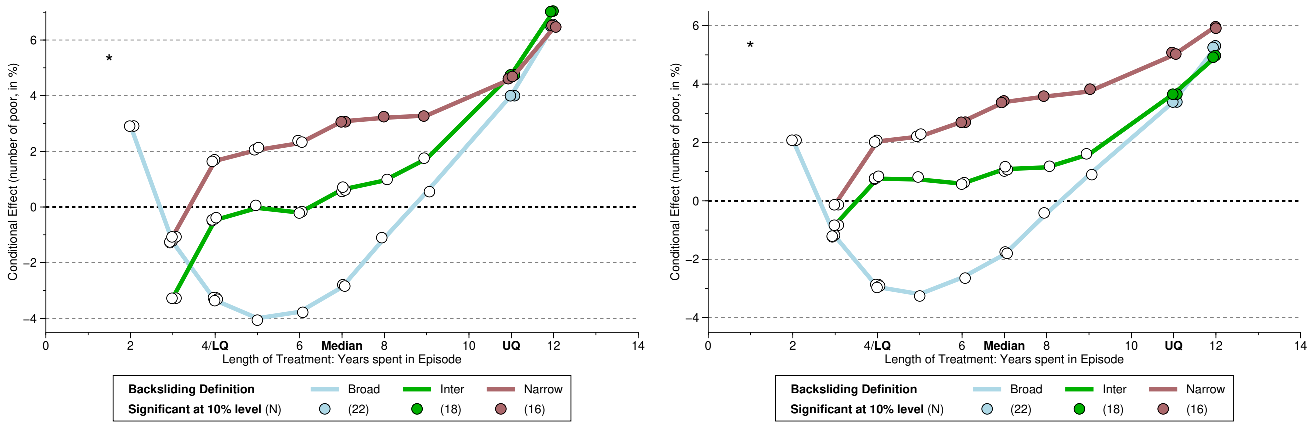
(a) GDP per capita,••• specifications with 4 (left) and 5 factors (right)



(b) Median HH Income,•○○ specifications with 4 (left) and 5 factors (right)



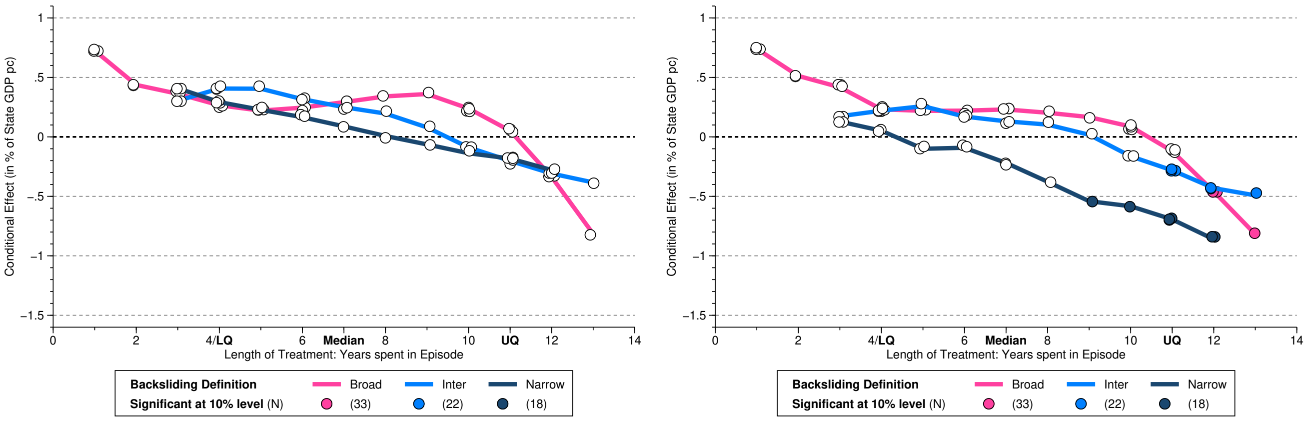
(c) Number of People Living in Poverty,••• specifications with 4 (left) and 5 factors (right)



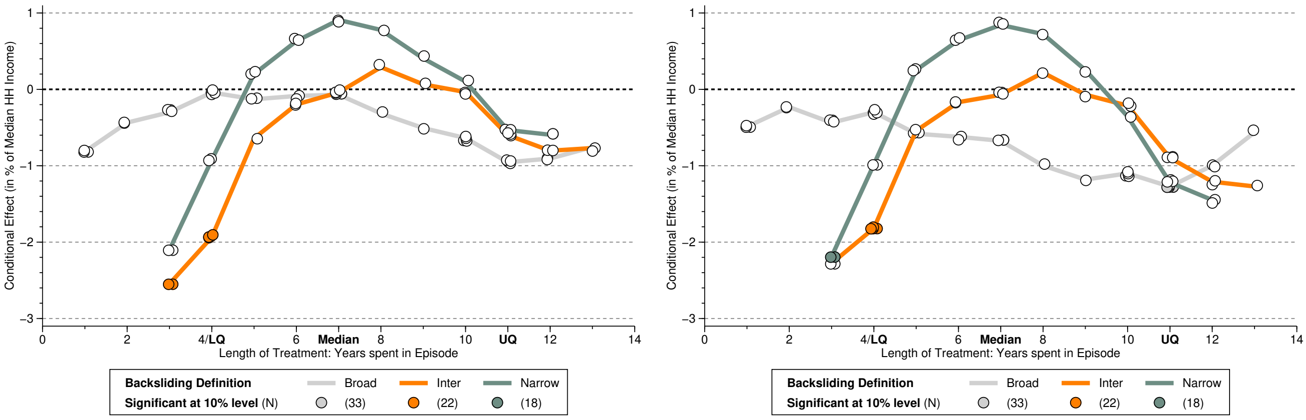
Notes: We present results from the PCDID specifications with the employment growth rate as an additional control variable. Here, we only plot treatment effects if states experienced a single episode of democratic backsliding. Models using the intermediate and conservative backsliding definitions in panel (b) fail the Alpha test. For all other details, see notes to Figure 3 in the main text.

Figure E-2: Democratic Backsliding Episodes and Economic Outcomes — No Controls

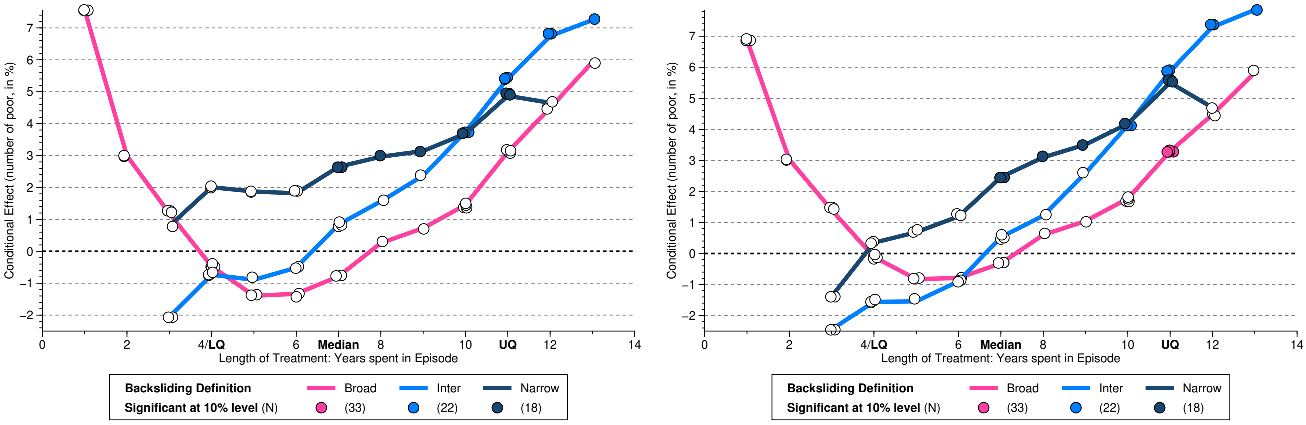
(a) State GDP per capita,●●● specifications with 4 (left) and 5 factors (right)



(b) State Median HH Income,●○○ specifications with 4 (left) and 5 factors (right)



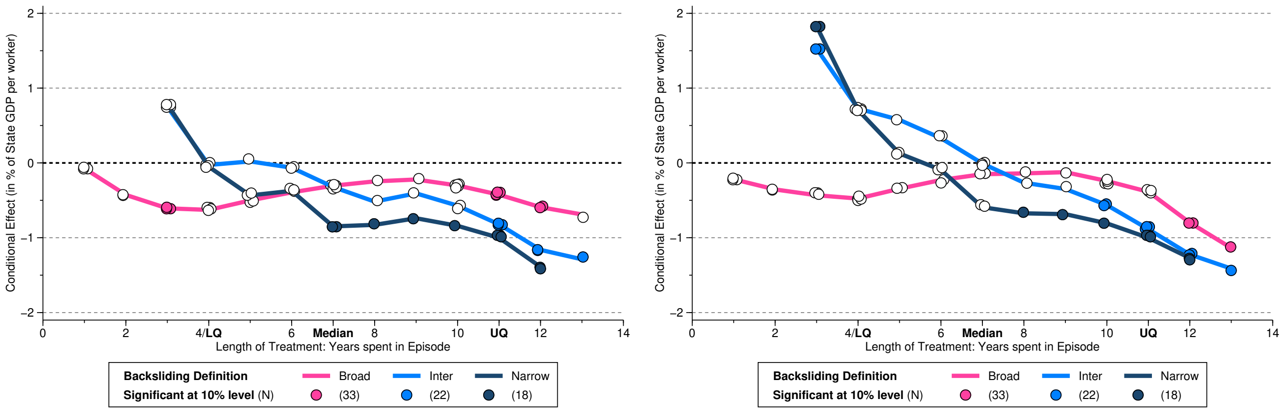
(c) Number of People Living in Poverty,●●● specifications with 4 (left) and 5 factors (right)



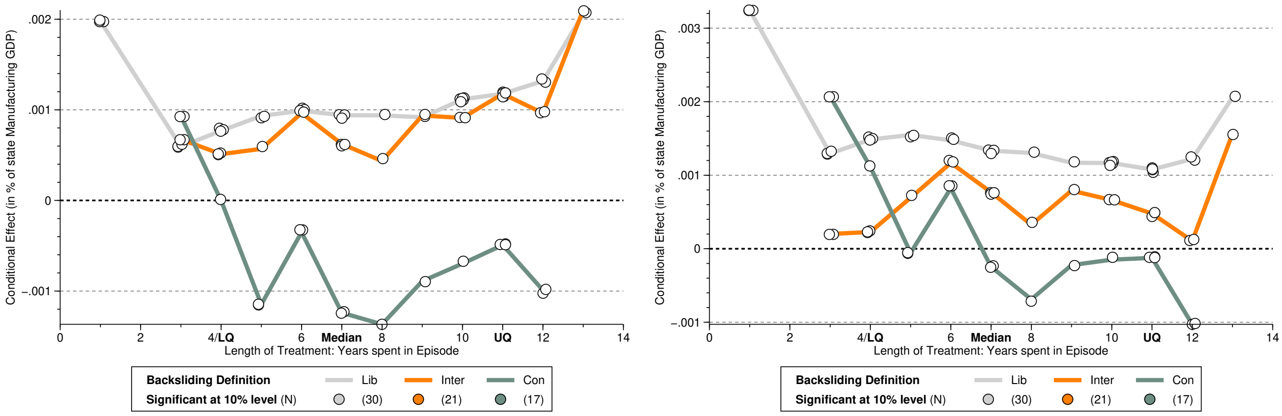
Notes: We present results from the PCDID specifications without any additional control variables. Models using the intermediate and conservative backsliding definitions in panel (b) fail the Alpha test. For all other details, see notes to Figure 3 in the main text.

Figure E-3: Democratic Backsliding Episodes and Economic Outcomes — Alternative Outcomes

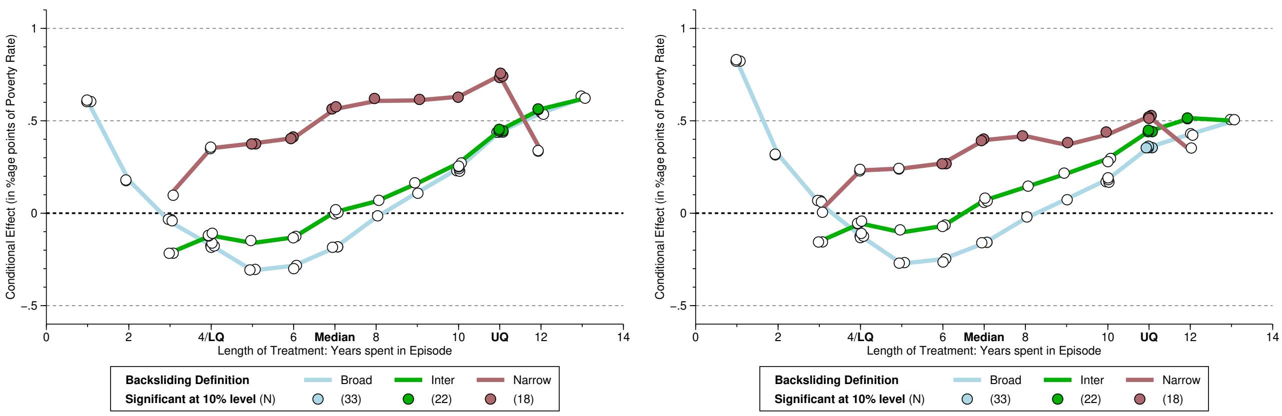
(a) State GDP per worker,°°° specifications with 4 (left) and 5 factors (right)



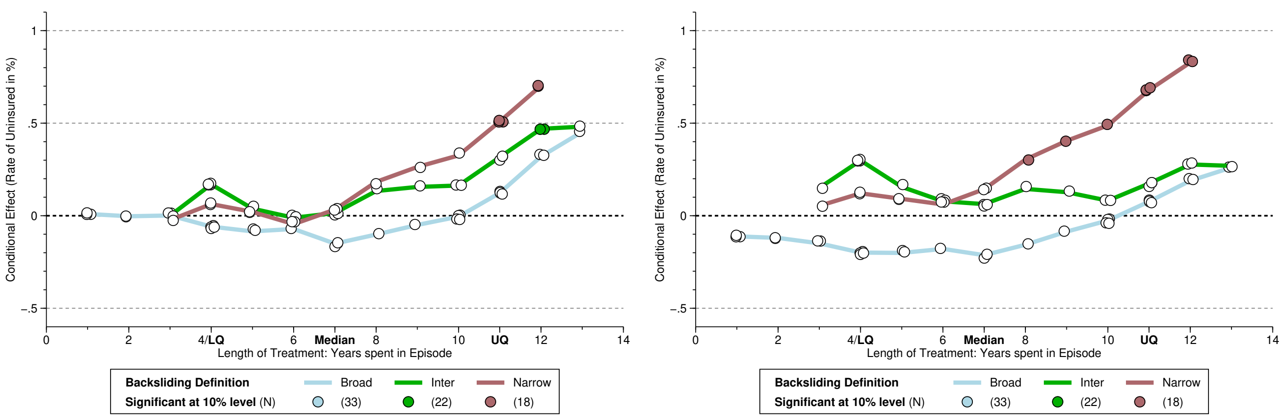
(b) State Gini coefficient,•°° specifications with 4 (left) and 5 factors (right)



(c) State Poverty Rate,••• specifications with 4 (left) and 5 factors (right)



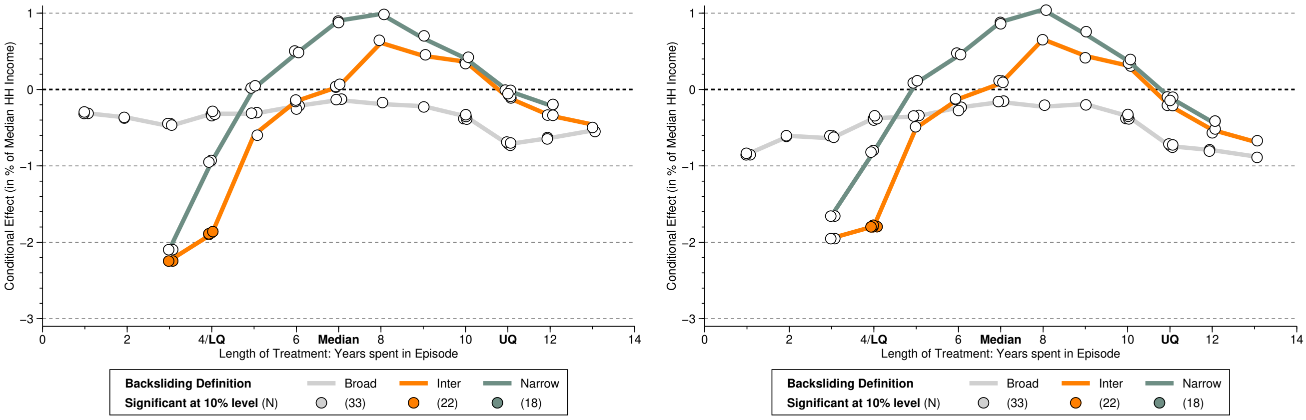
(d) Number of Uninsured Individuals,°°° specifications with 4 (left) and 5 factors (right)



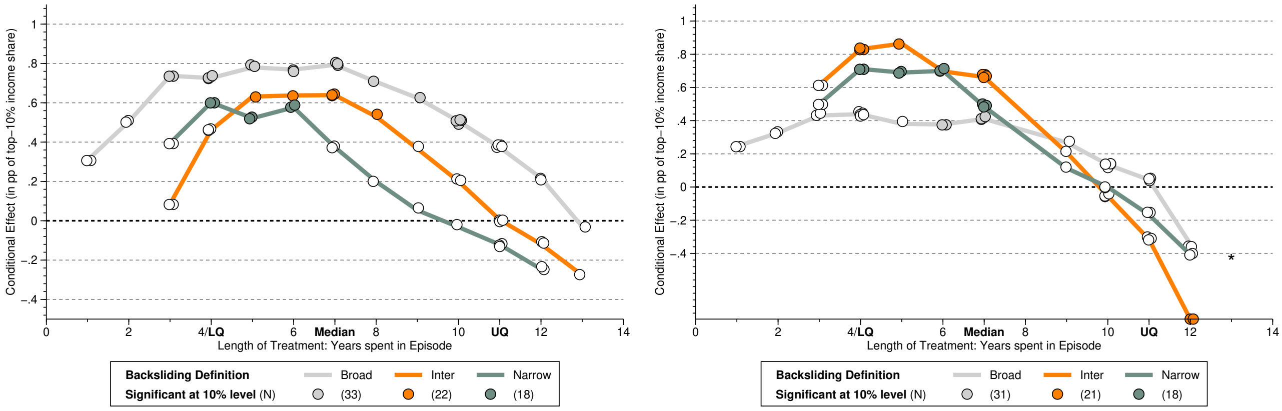
Notes: We present results from the PCDID specifications with the employment growth rate as an additional control variable. For all other details, see notes to Figure 3 in the main text.

Figure E-4: Democratic Backsliding Episodes and Economic Outcomes — More Alternative Outcomes

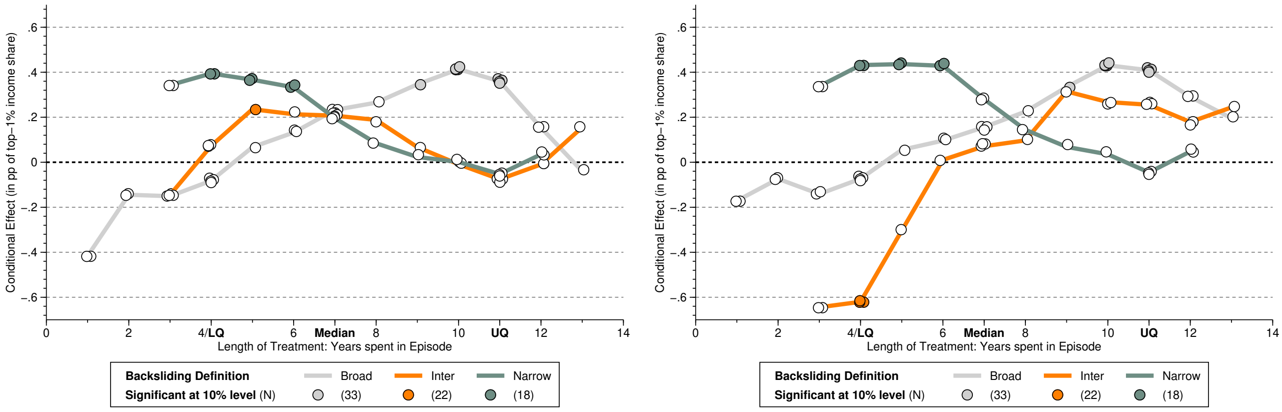
(a) Median HH Income,•∘∘ specifications with 4 (left) and 5 factors (right)



(b) Top 10% Income Share,••• specifications with 4 (left) and 5 factors (right)



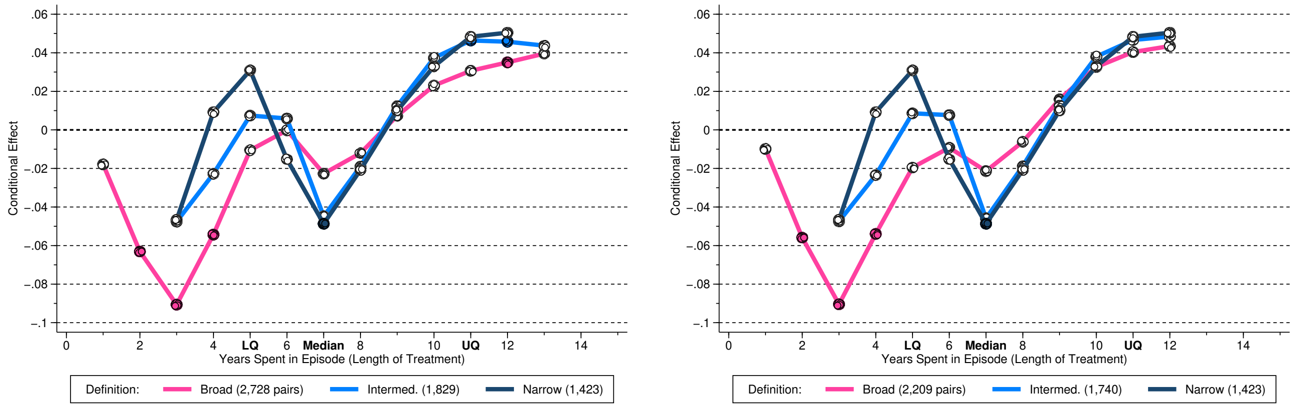
(c) Top 1% Income Share,••• specifications with 4 (left) and 5 factors (right)



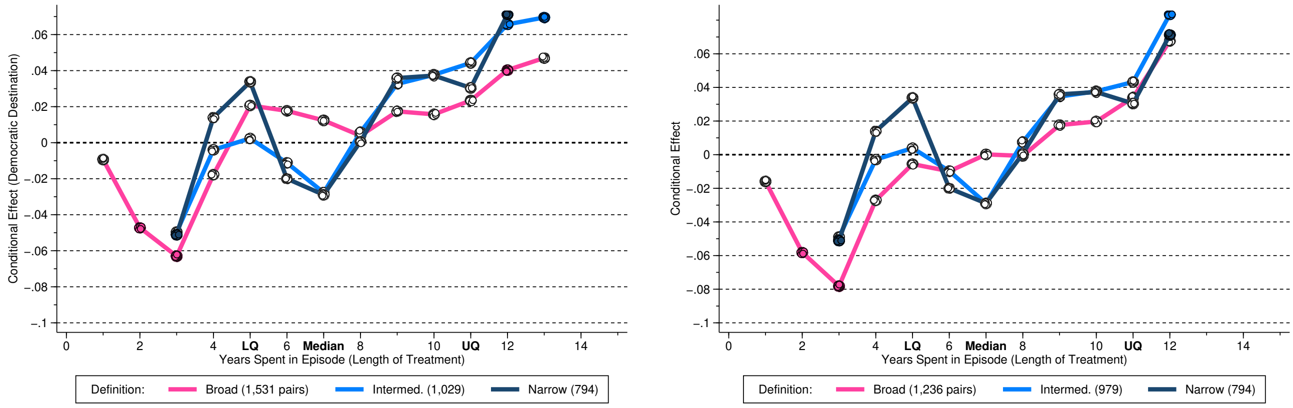
Notes: We present results from the PCDID specifications without any additional control variable — specifications with employment growth as control always fail the Wald test for 'bad controls' and some further fail the Alpha test. The sample for these results is for 2000-2018 only. For all other details, see notes to Figure 3 in the main text.

Figure E-5: Democratic Backsliding Episodes and Exports (Top-100 Destinations)

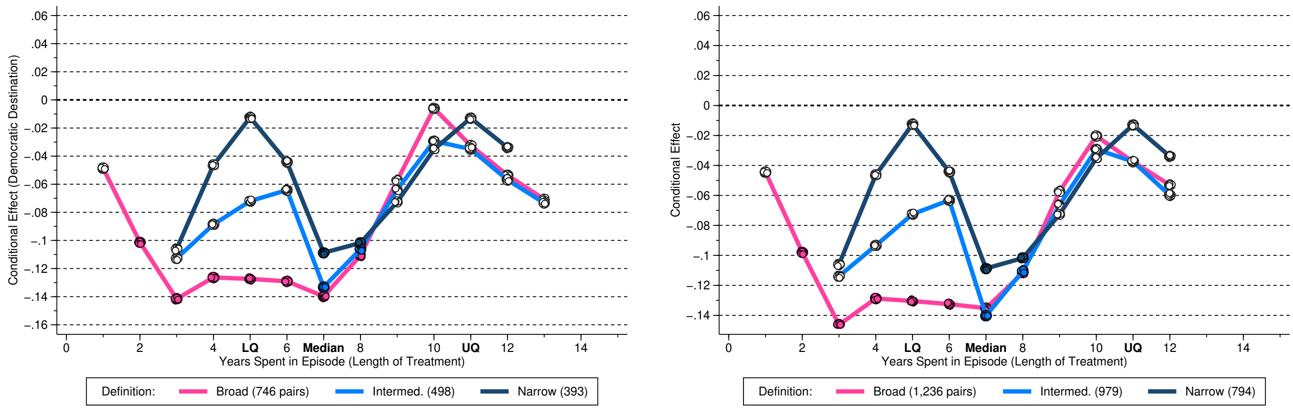
(a) Any Destination, all states•• ◦ (left) and states with single backsliding episode•• ◦ (right)



(b) Democratic Destinations, all states◦•• (left) and states with single backsliding episode◦•• (right)



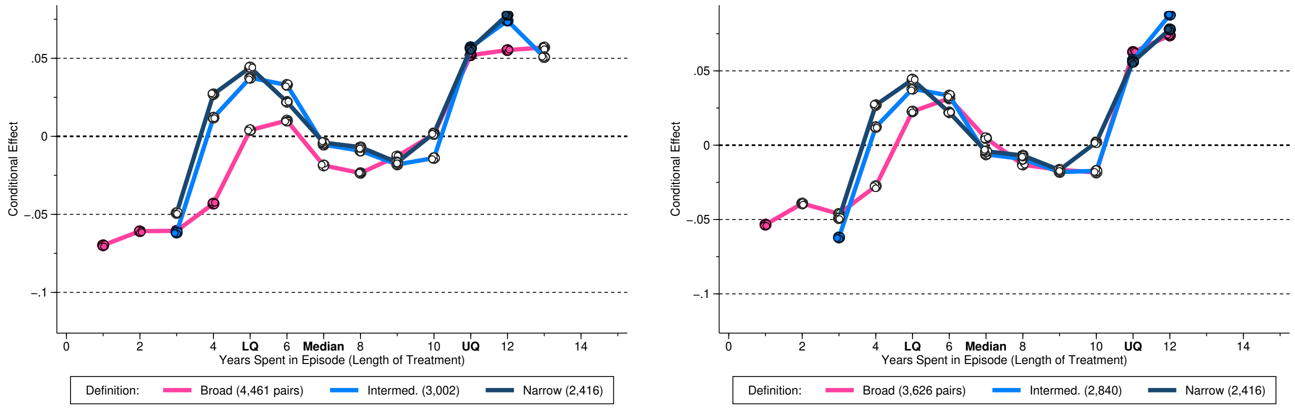
(c) Autocratic Destinations, all states•◦◦ (left) and states with backsliding single episode•◦◦ (right)



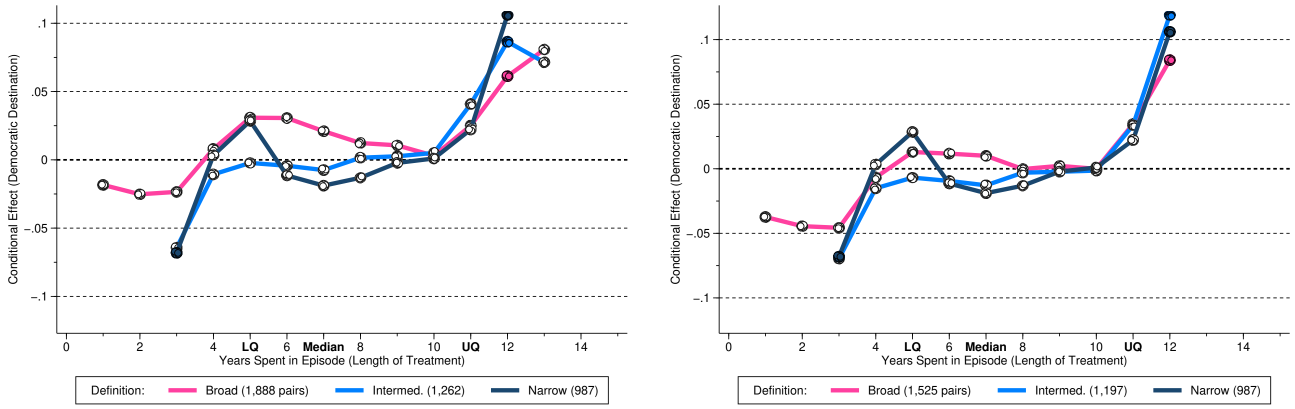
Notes: We present treated-state predictions from multivariate running line regressions (Royston and Cox, 2005) illustrating the economic effects of democratic backsliding for state exports to the top-100 U.S. export destinations. The (total) number of years a state has spent in one or more backsliding episodes is shown on the x-axis of each plot, and the economic effect on the y-axis. The running line regressions control for the number of times a state experienced a backsliding episode (dummies for two or three) and use weights derived from the robust ATET estimate using an M-estimator (Rousseeuw and Leroy, 2005). An estimate of -1 indicates a 1% reduction in export values. Each plot presents results for our three definitions of backsliding: a broad one (33 treated states), an intermediate one (22), and a narrow one (18) — see main text for definitions. A filled (white) marker indicates statistical (in)significance at the 10% level. We use • and ◦ in the subfigure title to indicate whether a specification passes or fails the Alpha diagnostic test at the 10% level: ••◦ indicates that the liberal and intermediate definitions pass the test, whereas the conservative definition does not.

Figure E-6: Democratic Backsliding Episodes and Exports (including Covid dummies)

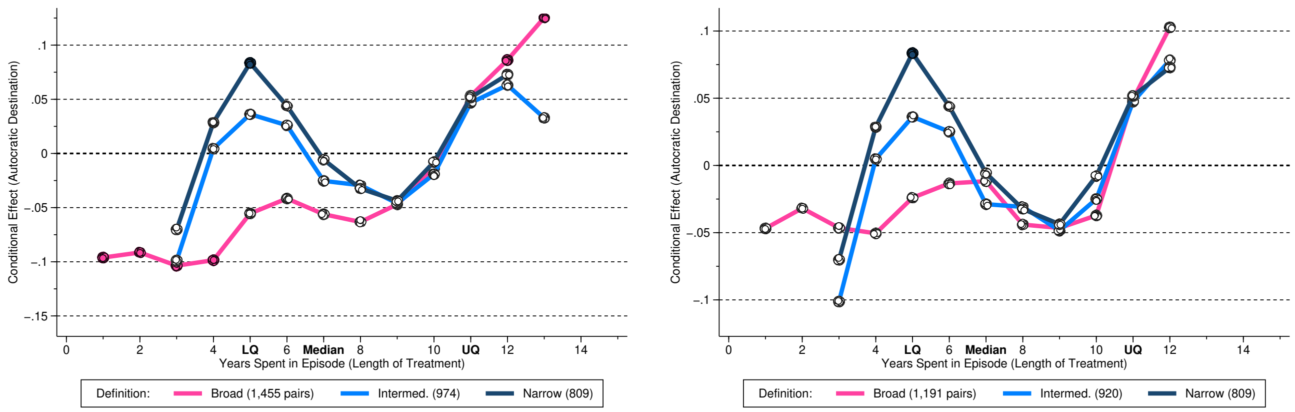
(a) Any Destination, all states$^{\circ\bullet\bullet}$ (left) and states with single backsliding episode$^{\circ\bullet\bullet}$ (right)



(b) Democratic Destinations, all states$^{\bullet\bullet\bullet}$ (left) and states with single backsliding episode$^{\bullet\bullet\circ}$ (right)



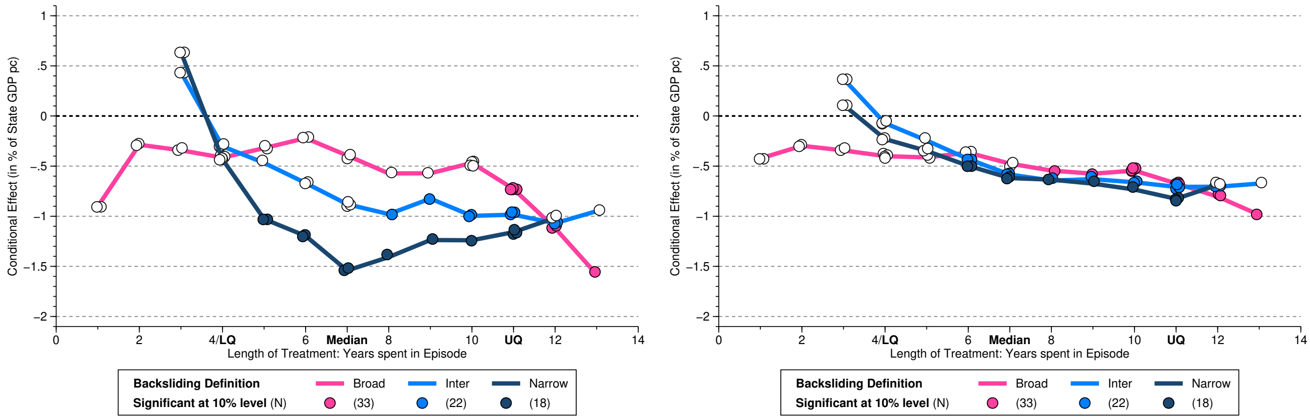
(c) Autocratic Destinations, all states$^{\bullet\bullet\bullet}$ (left) and states with single backsliding episode$^{\bullet\bullet\bullet}$ (right)



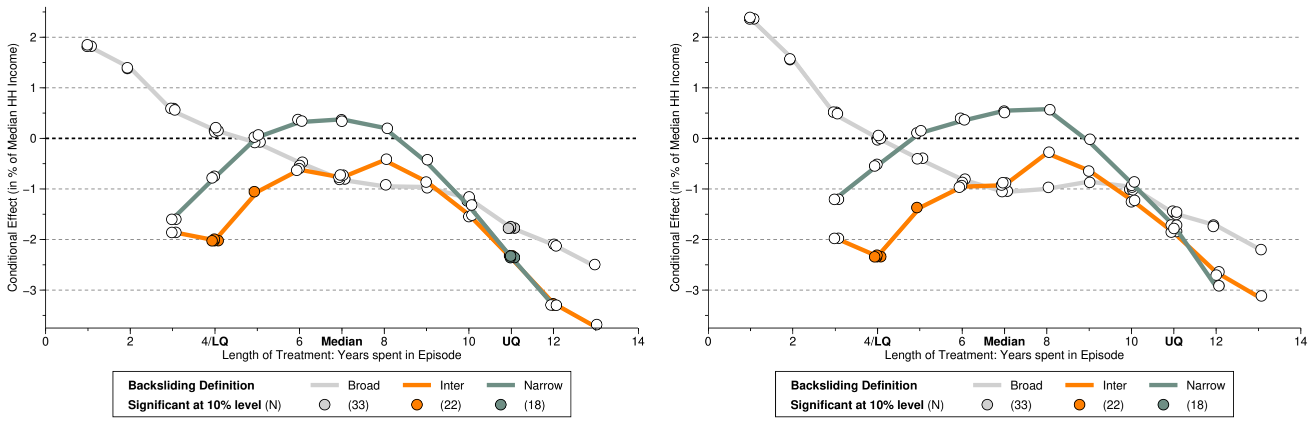
Notes: We present treated-state predictions from multivariate running line regressions (Royston and Cox, 2005) illustrating the economic effects of democratic backsliding for state exports to the top-100 U.S. export destinations. The (total) number of years a state has spent in one or more backsliding episodes is shown on the x-axis of each plot, and the economic effect on the y-axis. The running line regressions control for the number of times a state experienced a backsliding episode (dummies for two or three) and use weights derived from the robust ATET estimate using an M-estimator (Rousseeuw and Leroy, 2005). An estimate of -1 indicates a 1% reduction in export values. Each plot presents results for our three definitions of backsliding: a broad one (33 treated states), an intermediate one (22), and a narrow one (18) — see main text for definitions. A filled (white) marker indicates statistical (in)significance at the 10% level. We use • and ○ in the subfigure title to indicate whether a specification passes or fails the Alpha diagnostic test at the 10% level: ••○ indicates that the liberal and intermediate definitions pass the test, whereas the conservative definition does not.

Figure E-7: Democratic Backsliding Episodes — Main results with Covid dummies

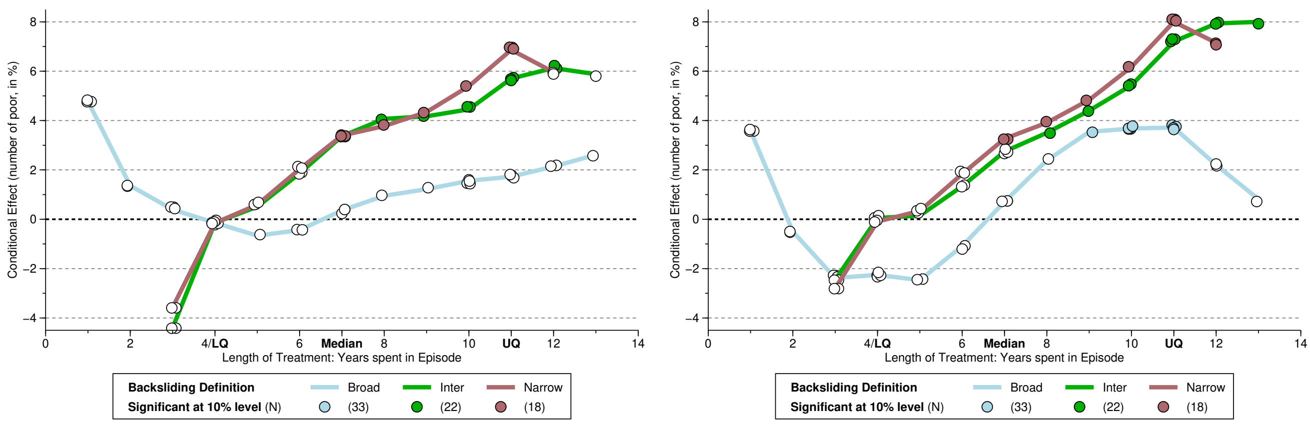
(a) GDP per capita,$^{\bullet\bullet\bullet}$ specifications with 4 (left) and 5 factors (right)



(b) Median HH income,$^{\circ\circ\circ}$ specifications with 4 (left) and 5 factors (right)



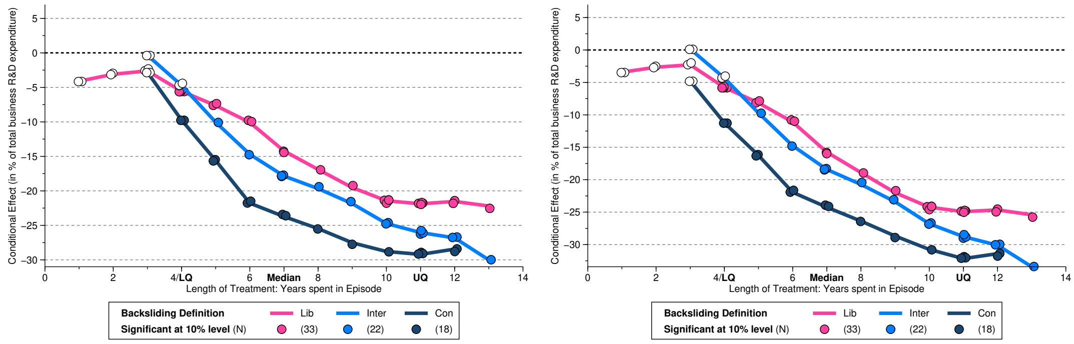
(c) Poverty headcount,$^{\bullet\bullet\bullet}$ specifications with 4 (left) and 5 factors (right)



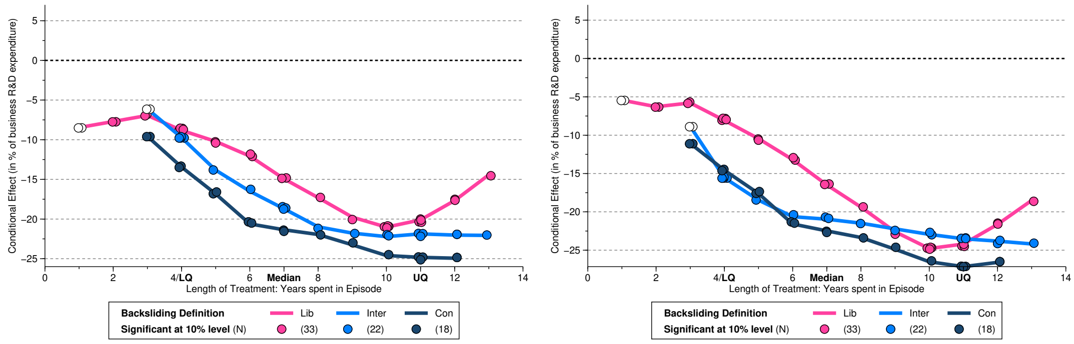
Notes: We present treated-state predictions from multivariate running line regressions (Royston and Cox, 2005), illustrating the economic effects of democratic backsliding. The underlying PCDID specifications have the employment growth rate as additional control and augment the treatment regression with four or five estimated factors, as indicated (models with three or six estimated factors yield qualitatively very similar results, available on request). The (total) number of years a state has spent in one or more backsliding episodes is shown on the x-axis of each plot, and the economic effect on the y-axis. The running line regressions control for the number of times a state experienced a backsliding episode and use weights derived from the robust ATET estimate using an M-estimator (Rousseeuw and Leroy, 2005). A filled (white) marker indicates statistical (in)significance at the 10% level. An estimate of -1 (+1) indicates a 1% reduction (increase) in income, median household income, and the number of poor people, in panels (a) to (c), respectively. Each plot presents results for our three definitions of backsliding: a broad one (33 treated states), an intermediate one (22), and a narrow one (18) — see text for definitions. We use $^{\bullet\bullet\bullet}$ to signal which specifications using these three different definitions of backsliding (Broad, Inter, Narrow) pass ($^{\bullet}$) or fail ($^{\circ}$) the Alpha test. For other details, see notes to Figure 3 in the main text.

Figure E-8: Democratic Backsliding Episodes — Innovation (with Covid dummies)

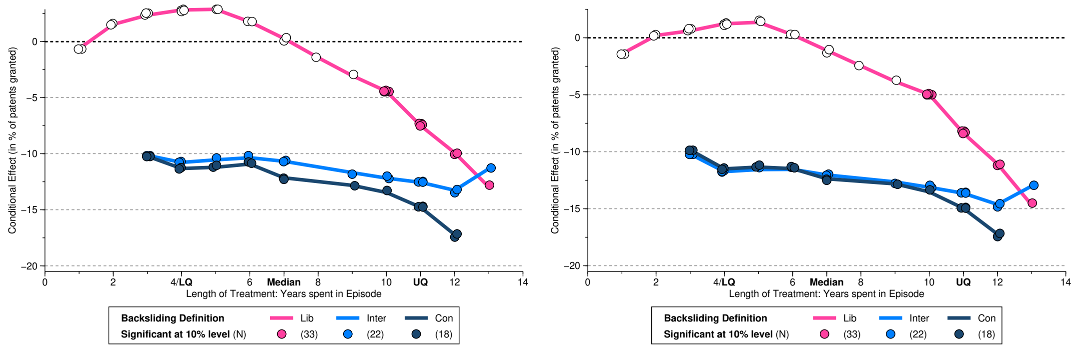
(a) Total Business R&D Expenditure,••• specifications with 4 (left) and 5 factors (right)



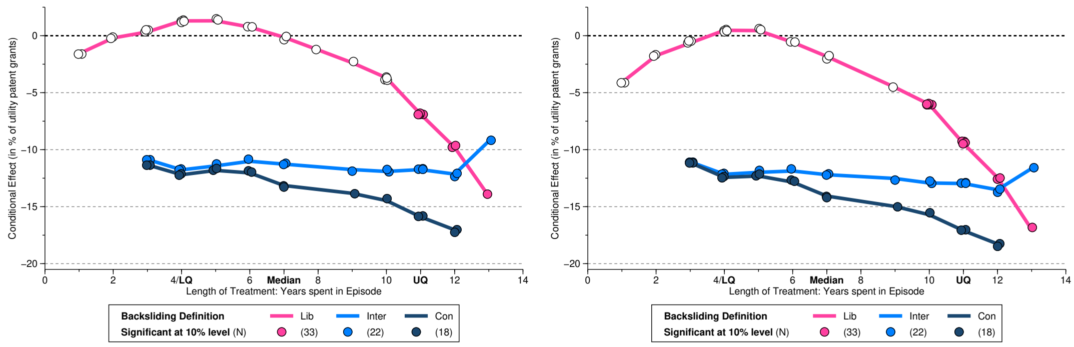
(b) R&D Expenditure by Businesses,••• specifications with 4 (left) and 5 factors (right)



(c) Total Patents Granted,••◦ specifications with 4 (left) and 5 factors (right)



(d) Utility Patents Granted,••◦ specifications with 4 (left) and 5 factors (right)

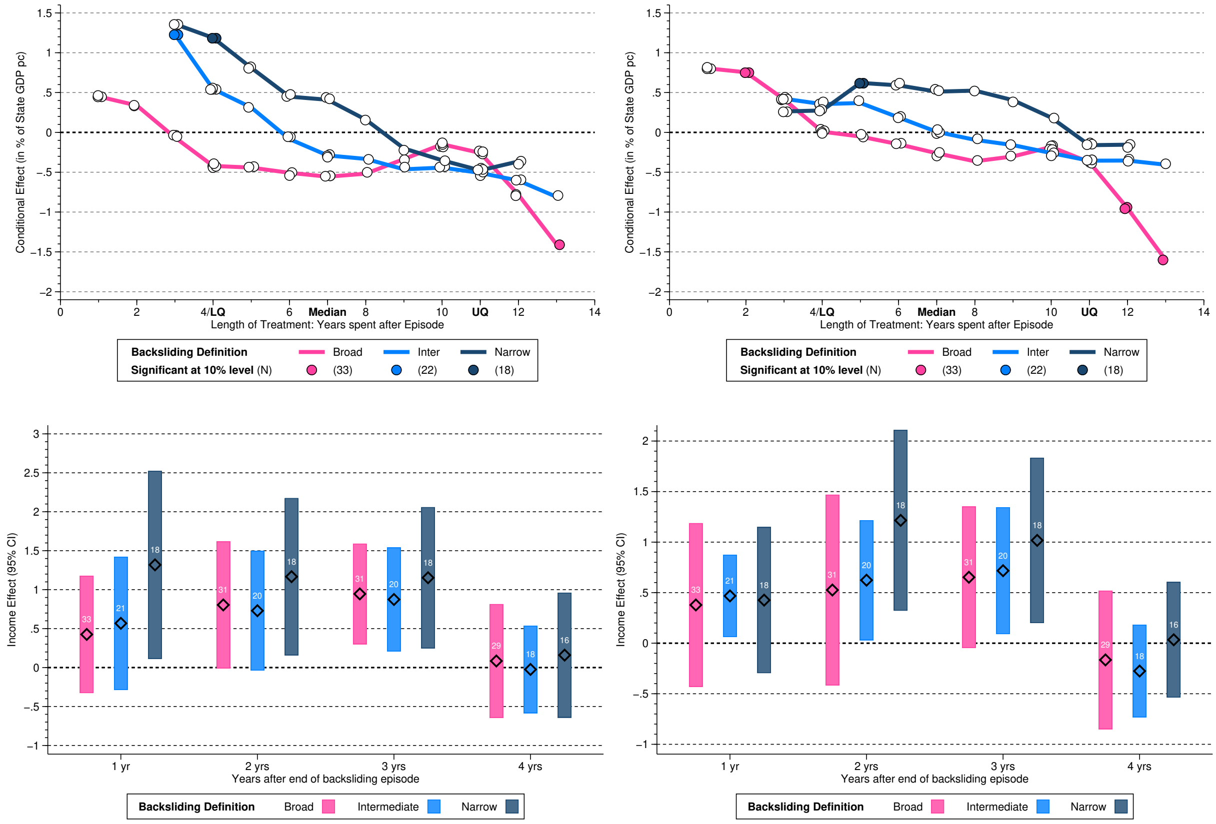


Notes: We present results from the PCDID specifications without additional control variable but including up to three Covid dummies (for 2020, 2021, and 2022, if applicable). The R&D (patent) data are for 2000-2022 (2000-2020). 'Total business R&D' includes expenditure by businesses sourced from the federal government (e.g. grants) and from 'others' (i.e. other firms located inside or outside the US, state or foreign government agencies, and other organizations inside or outside the US). 'Business R&D' refers to the expenditure paid for by the firms themselves. 'Total Patents' includes utility, plant and design patents. We use ••◦ to indicate whether specifications using the liberal, intermediate, and conservative definitions of backsliding pass (•) or fail (◦) the Alpha test at the 10% significance level. For other details, see notes to Figure 3 in the main text.

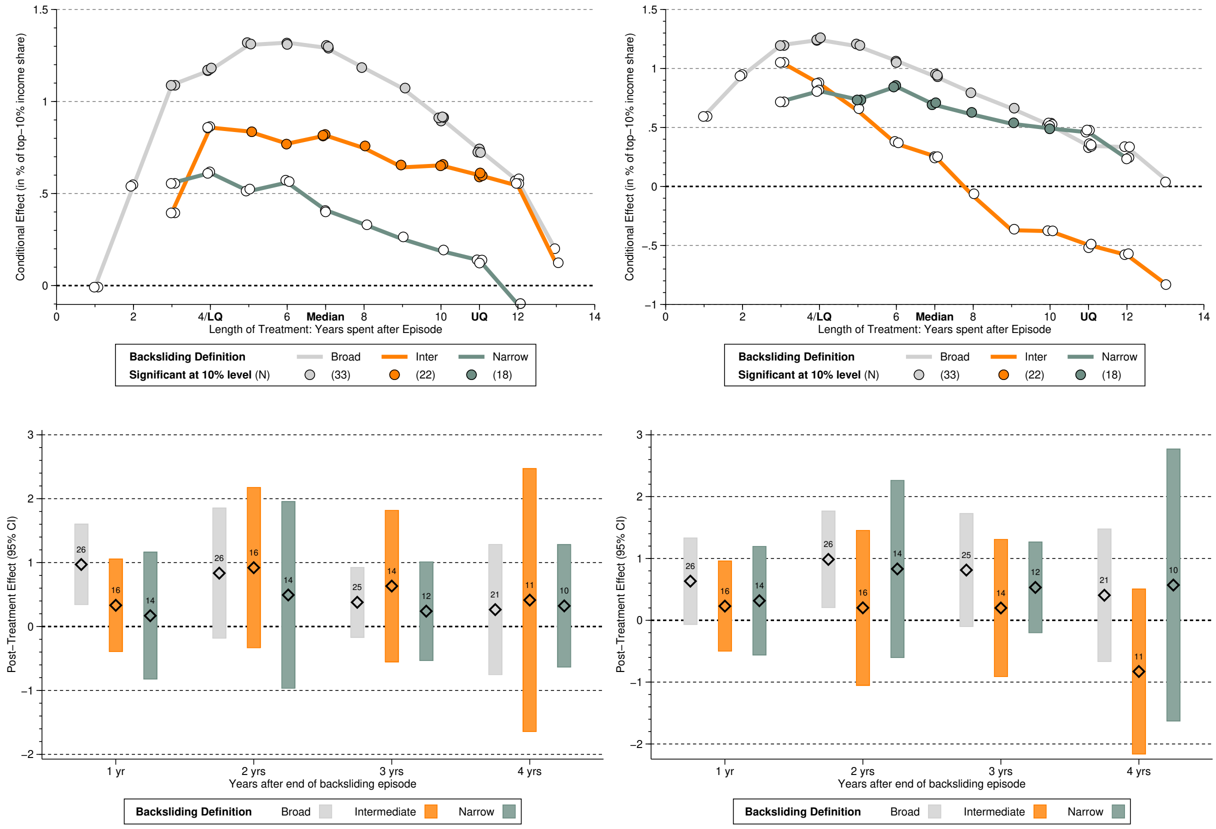
F Treatment and Post-treatment Effects

Figure F-1: Democratic Backsliding Episodes — Treatment and Post-treatment

(a) GDP pc•••: treatment estimates for 4 and 5 factors (top), post-treatment estimates



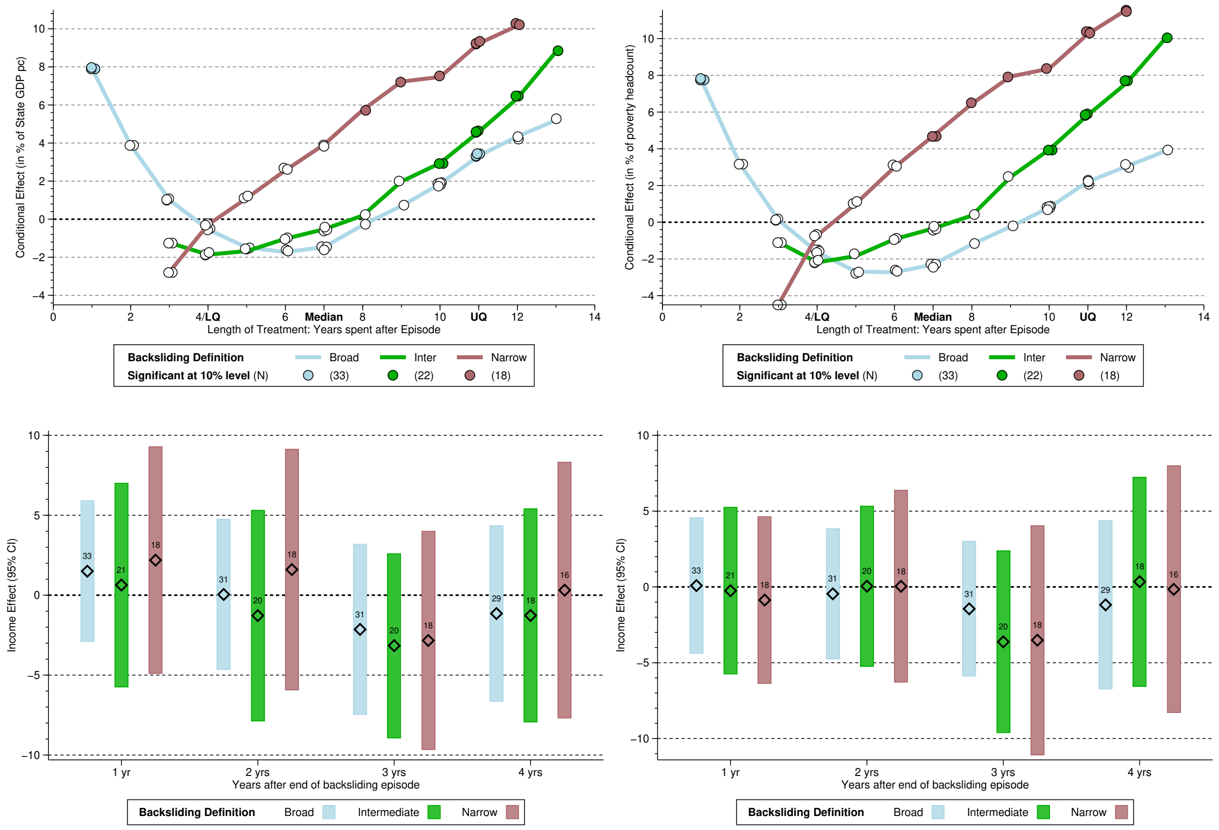
(b) Top-10% Income Share•••: treatment estimates for 4 and 5 factors (top), post-treatment estimates



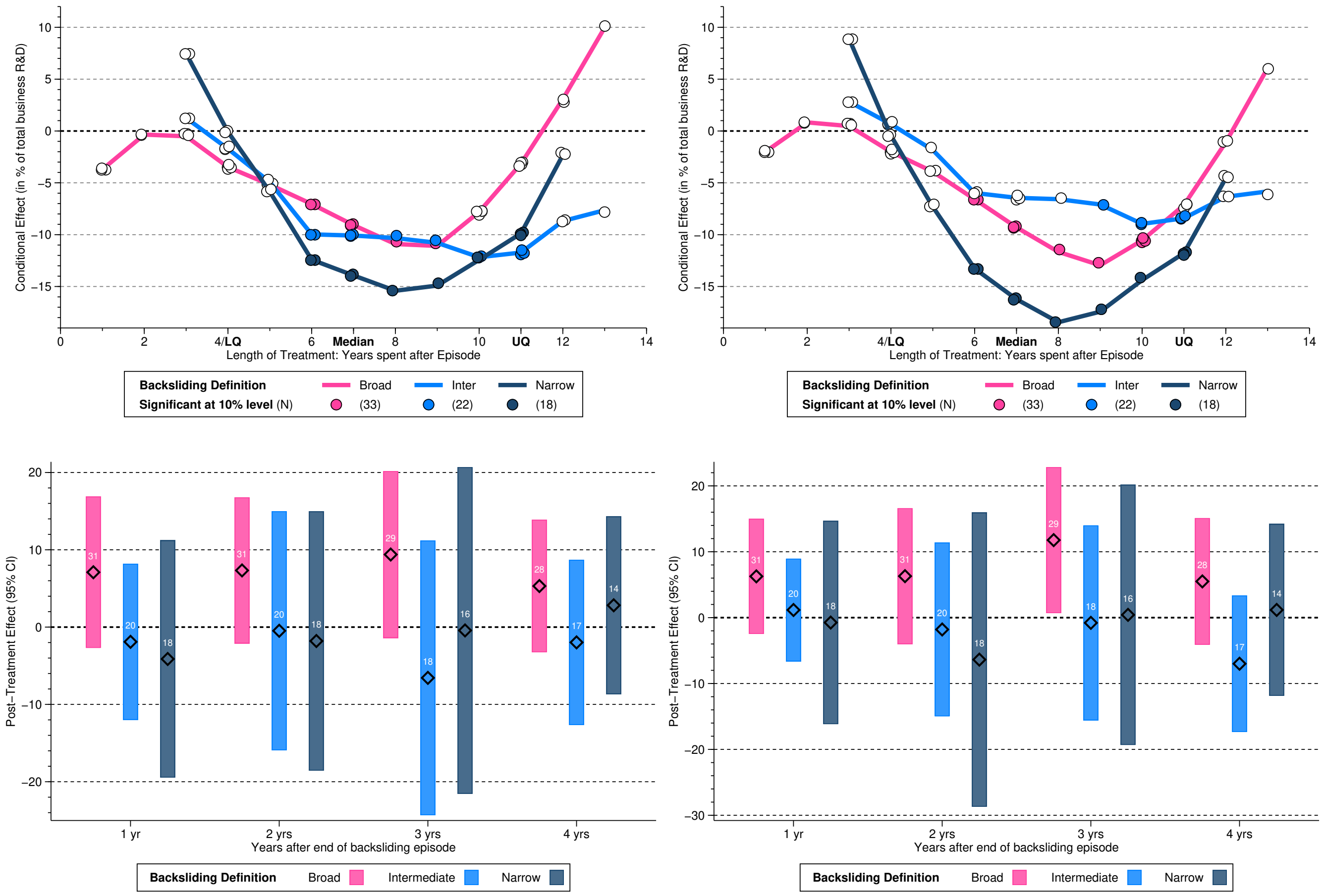
(Figure continued overleaf)

Figure F-1: Democratic Backsliding Episodes — Treatment and Post-treatment (cont'd)

(c) Poverty headcount•••: treatment estimates for 4 and 5 factors (top), post-treatment estimates



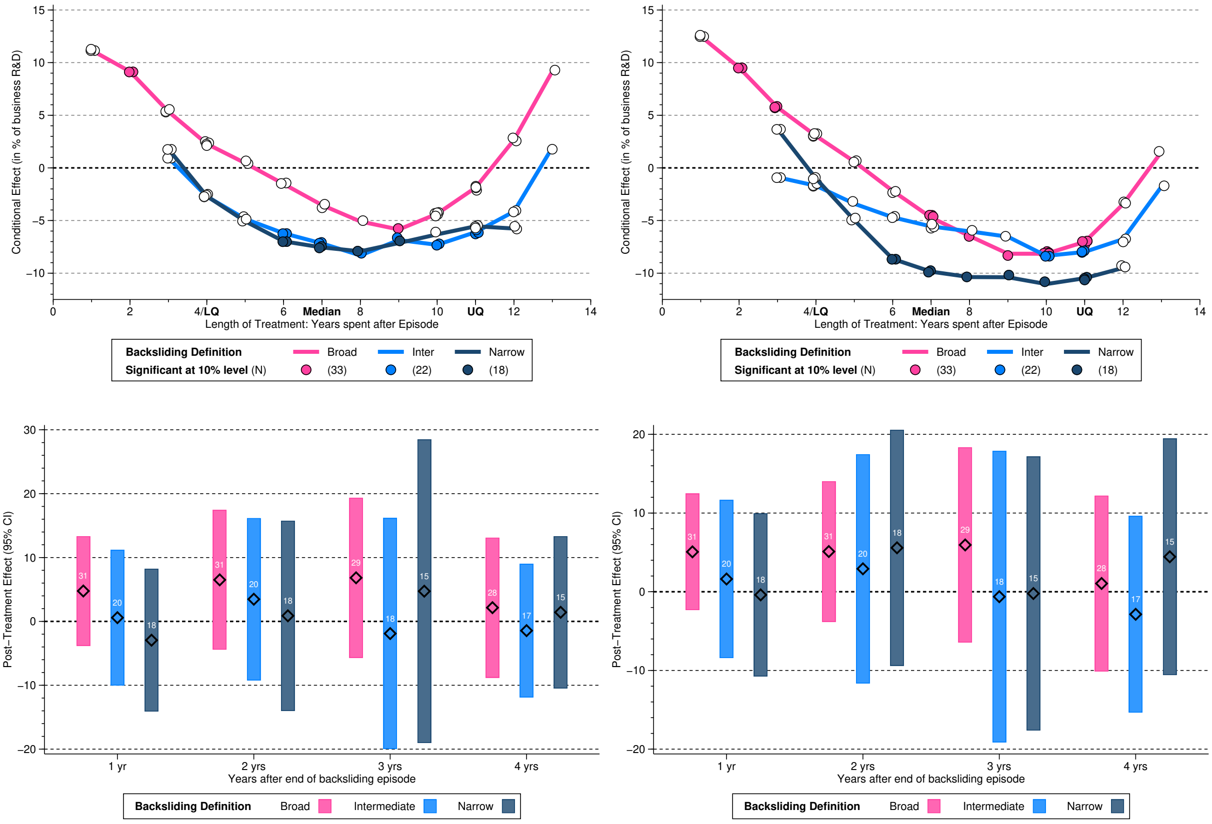
(d) Total Business R&D expenditure•••: treatment estimates for 4 and 5 factors (top), post-treatment estimates



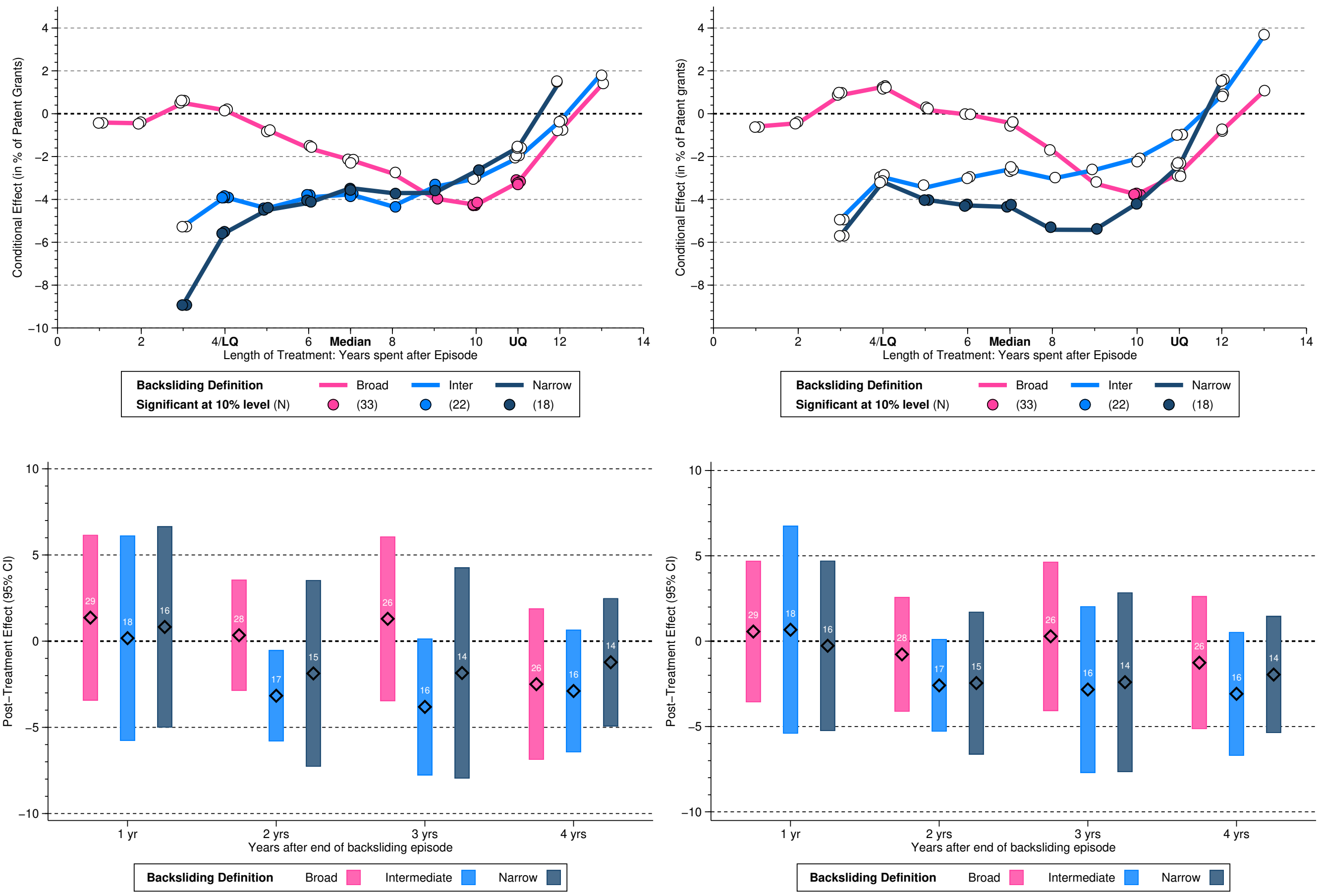
(Figure continued overleaf)

Figure F-1: Democratic Backsliding Episodes — Treatment and Post-treatment (cont'd)

(e) Business R&D expenditure●●○: treatment estimates for 4 and 5 factors (top), post-treatment estimates



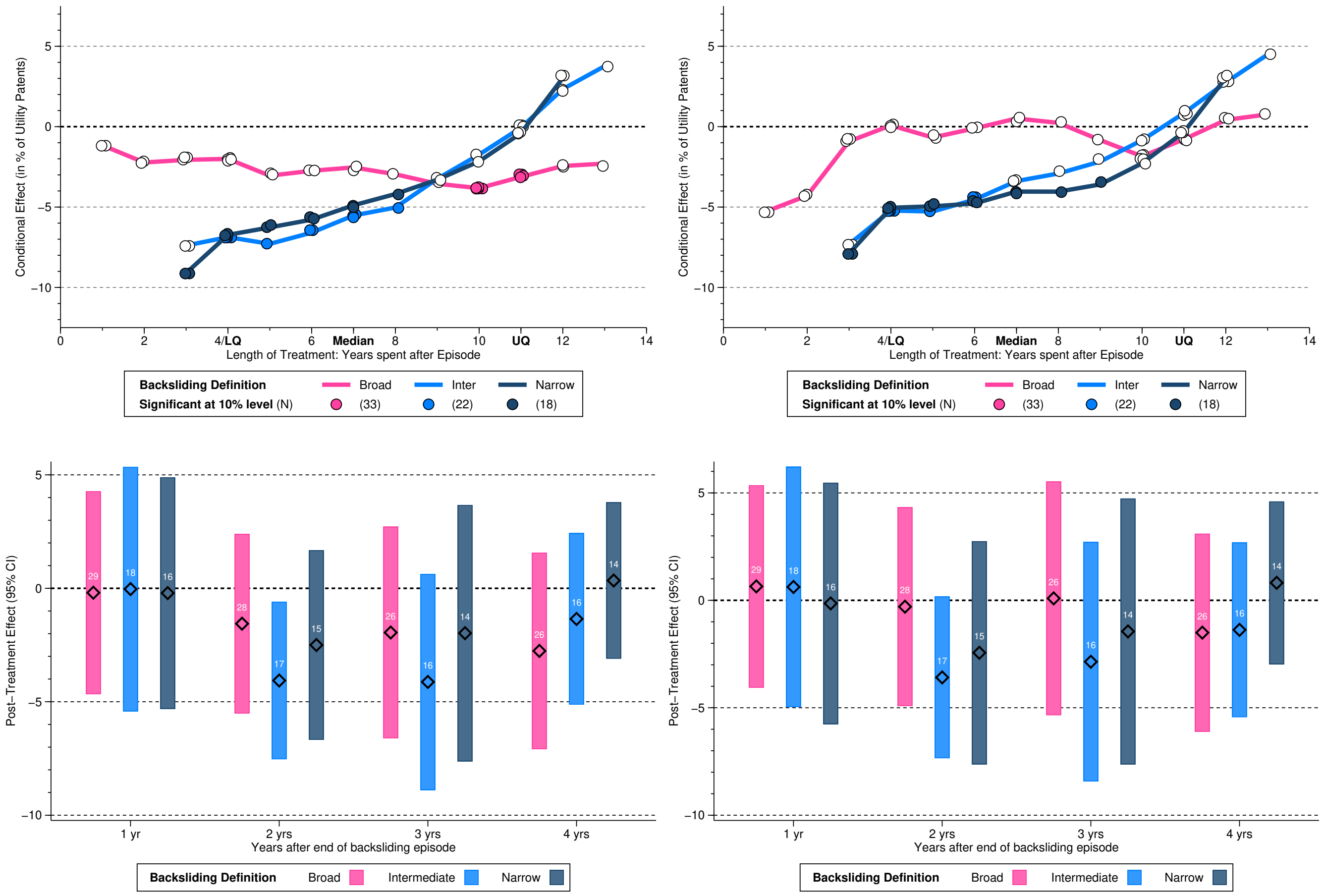
(f) Patent grants●●○: treatment estimates for 4 and 5 factors (top), post-treatment estimates



(Figure continued overleaf)

Figure F-1: Democratic Backsliding Episodes — Treatment and Post-treatment (cont'd)

(g) Utility Patents●●○: treatment estimates for 4 and 5 factors (top), post-treatment estimates

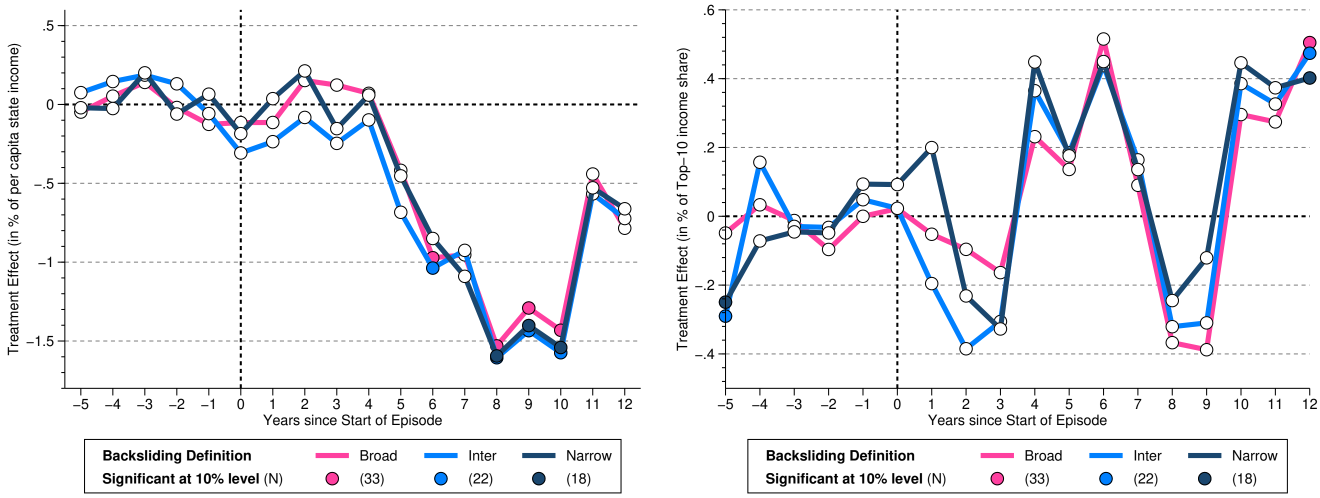


Notes: We present predictions from running line regressions of the underlying PCDID regressions where we add four post-episode dummies to the specification. In each panel, the first set of graphs are the treatment effects as presented before for PCDID augmentation with four or five factors. The second set of results is the related robust mean estimates for the four post-episode year dummies (along with their 90% confidence intervals), in each case for the broad, intermediate, and narrow definition of backsliding.

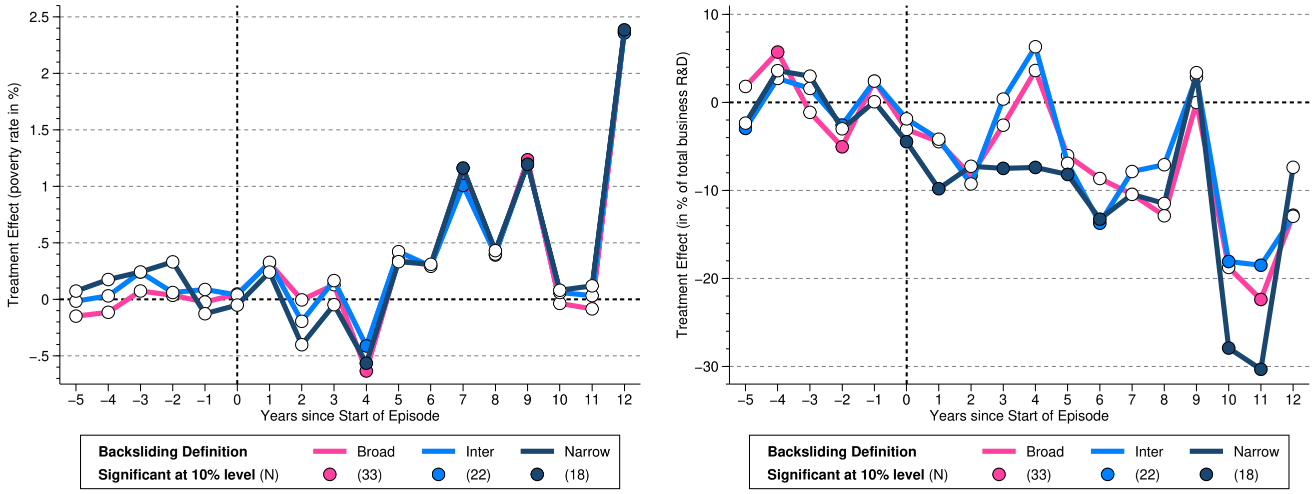
G Counterfactual Approaches

Figure G-1: Democratic Backsliding Episodes — Counterfactual Estimators

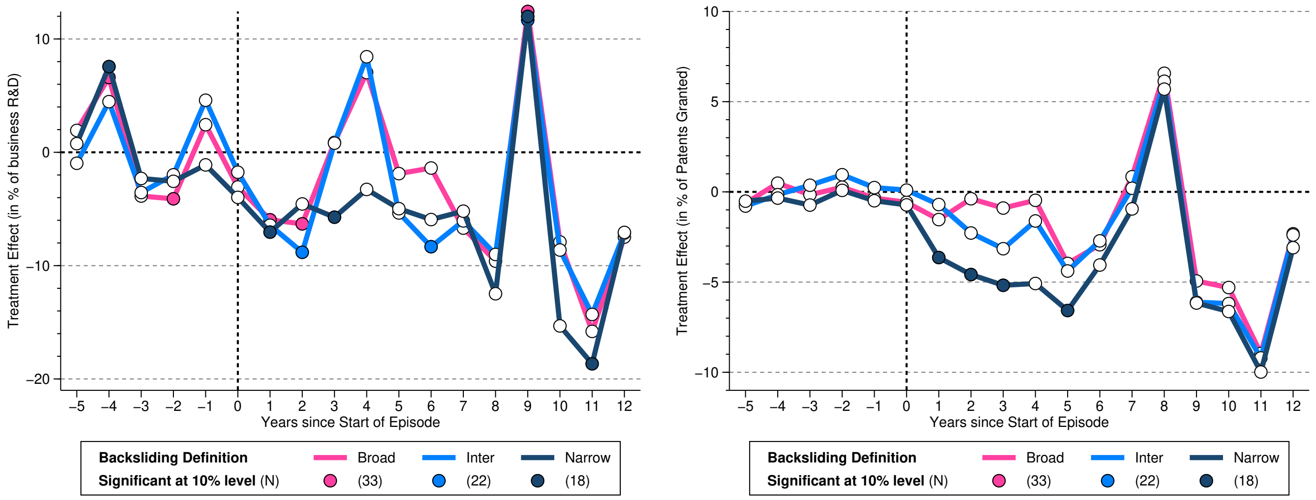
(a) State income per capita (left) and top-10% income share (right)



(b) Poverty rate (left) and Total R&D Expenditure by Businesses(right)



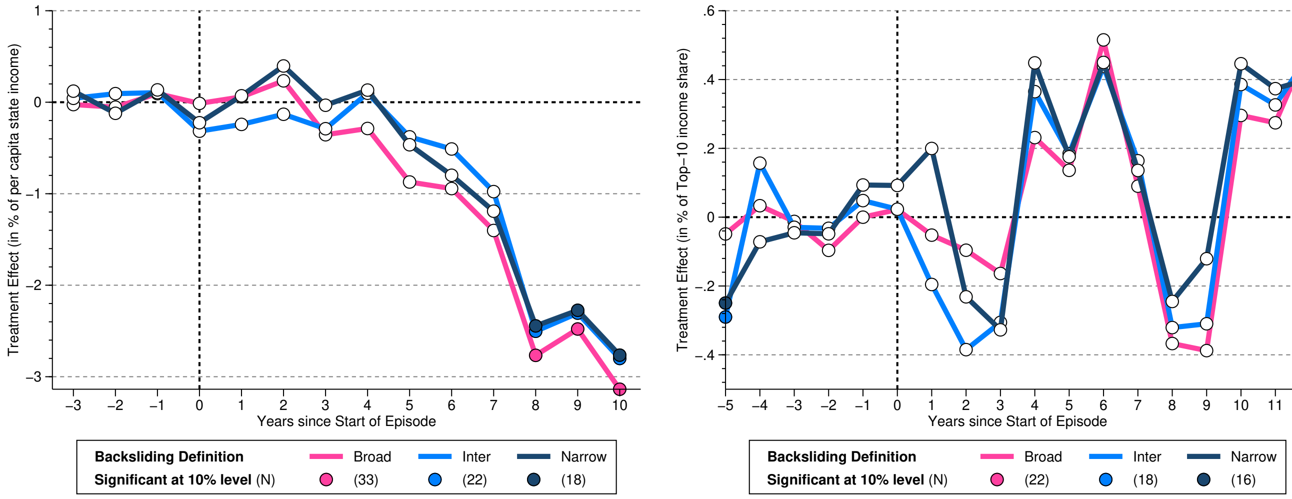
(c) Business Own R&D Expenditure (left) and Total Patents Granted (right)



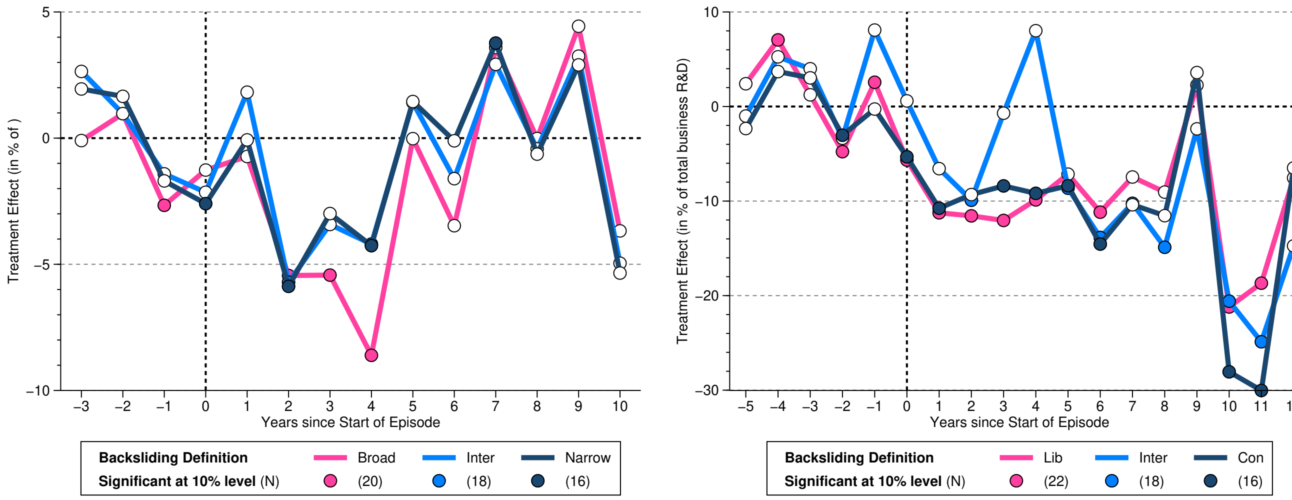
Notes: We compare results for the three variants of democratic backsliding using the Xu (2017) counter-factual estimators (fect). The plots represent event analyses.

Figure G-2: Democratic Backsliding Episodes — Counterfactual Estimators (restricted)

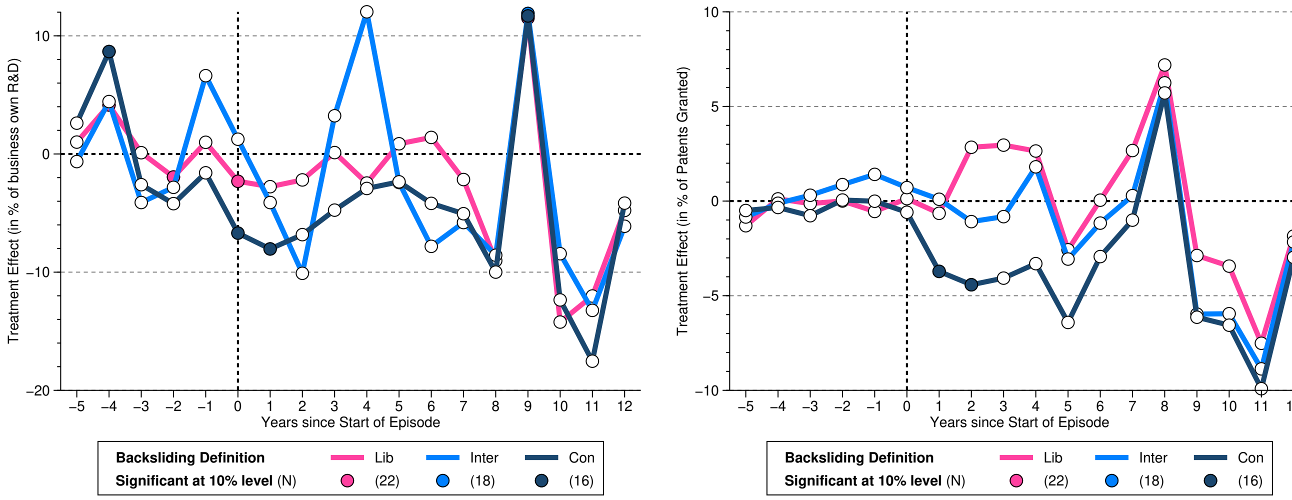
(a) State income per capita (left) and top-10% income share (right)



(b) Poverty rate (left) and Total R&D Expenditure by Businesses(right)



(c) Business Own R&D Expenditure (left) and Total Patents Granted (right)



Notes: We compare results for the three variants of democratic backsliding using the Xu (2017) counter-factual estimators (fect). The plots represent event analyses, where the treated sample is limited to states which experienced a single backsliding episode.