Identifying the Effect of Truth Commissions on the Quality of Democracy

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Abstract

Does transitional justice hinder or help democracy? This simple question poses a challenge because countries choosing to embark on transitional justice may be exactly the ones that would have had a successful pathway to democratization in the first place. We resolve it by measuring transitional justice as a series of events rather than one-shot instances of dealing with the past. Among transitional justice mechanisms, we focus on truth commissions, one of the most popular mechanisms of dealing with authoritarian pasts in the last fifty years. We propose one possible mechanism through which truth commissions can enhance democratic quality, arguing that *transparency regimes*, such as truth commissions, reveal "skeletons in politicians' closets" that could be used as kompromat to extract policy concessions from compromised politicians in exchange for silence. According to this logic, transparency regimes result in less corrupt politics and better quality of democratic representation. To corroborate this claim, we disaggregate the truth commission process into a series events that advance or hinder the process of disclosing human rights abuses that took place in the past. This method of coding not only allows us to measure their impact on democratic representation, but is also more faithful to the operation of truth commissions on the ground. We then use a difference-in-difference set-up to identify the causal link between truth commissions and political corruption on a panel dataset of 81 countries that transitioned to democracy since 1946.

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1   Introduction

Does transitional justice improve the quality of democracy in the countries that implement it? Transitional justice (TJ) refers to the ways in which countries that recently transitioned to democracy or that are recovering from civil conflict reckon with authoritarian elites and their collaborators, or perpetrators of human rights violations and their victims (Zvobgo 2019, Bakiner 2016, Loyle & Appel 2017, Balcells, Palanza & Voytas 2020, Capoccia & Pop-Eleches 2020).

The demand from the public for such reckoning can be overwhelming. Following the US War of Independence, high ranking defectors, such as Benedict Arnold, fled to Britain. To placate demand for reckoning with such collaborators, the Patriots would organize ceremonies with a puppet in a cage standing for the defector. The puppet would be paraded through the village and hanged in an act of revenge, only because, for instance, Arnold himself was not available (Brumwell 2018). Similar acts of symbolic justice took place in late 18th century Poland, after three aristocrats conspired with Katherine the Great to have the country divvied up among three empires: Prussia, Russia and the Hapsburg Empire. Following these, so-called, Partitions, Polish nationalists across the country would stage mock trials with the traitors' portraits standing in for the accused. After being sentenced in *absentia*, they would be hanged in effigy (Walek 1988).

In the 20th century, such spectacles of "folk" transitional justice have given way to other extrajudicial, but sometimes equally spectacular forms of dealing with perpetrators of human rights abuses and their collaborators. Consider as an example the South African Truth and Reconciliation Commission (TRC) (Hayner 2010, Gibson 2006). Formed in 1995 to investigate crimes committed against the South African people during the apartheid regime (1960-1994), it covered human rights violations committed by both the state and various liberation movements.[1] The Commission's mandate provided it with the ability

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[1]Specifically, it was established via the Promotion of National Union and Reconciliation Act, passed by the South African parliament in July 1995 (Gibson & Gouws 1999).

to offer amnesty to those who fully participated in the process and truthfully confessed the full extent of their crimes, hence incentivizing many perpetrators to come forward to share their story. Eventually, in 1998, it released a five-volume final report. To collect testimonies, however, it toured the country for a couple of years, conducting hearings that were nationally broadcast on public radio. The report detailed the abuses committed by the apartheid-era National Party government, the African National Congress (ANC)—the opposition-turned-ruling party—and other "leading political figures on both sides of the anti-apartheid struggle." Neither side was completely satisfied with its findings and recommendations, but the report was much more palatable given how familiar South Africans had become with the idea of the TRC.

Despite the popular demand for dealing with former members and collaborators of the authoritarian regime or perpetrators of human rights violations involved in civil conflict, scholars of democratization and regime transitions in general, remain mistrustful of whether or not these new democracies can go about punishing perpetrators of human rights abuses committed in the past without jeopardizing the rule of law. Jack Snyder argues that "the prosecution of perpetrators of atrocities according to universal standards risks causing more atrocities than it would prevent, because it pays insufficient attention to political realities" (Snyder & Vinjamuri 2004, 5). Stephen Holmes calls some forms of transitional justice "witch hunts" (Holmes 1994). Jon Elster refers to such acts as "ritual sacrifices"(Cepl 1992). Even Samuel Huntington maintains that sometimes "amnesty"(...) "is necessary to establish a new democracy on a solid basis" (Huntington 1993, 214). Implicit in this classic literature is the understanding of transitional justice as a punitive process. Hence the use of the term "retroactive justice" as synonymous with transitional justice.

Yet not all forms of authoritarian dominance are as transparent as repression (Tyson 2016). In many instances, the very acts that sustained the authoritarian regime were secret collaboration (Blaydes 2010), cooptation (Magaloni 2006), and sabotage (Dragu &

Przeworski 2019). Hence, another important way of dealing with the past is through the disclosure of such actions and the revelation of the identities of perpetrators.

Consider again the example of the TRC from above. Although some of the perpetrators that appeared before the Commission were known offenders, many of the facts they revealed in their hearings disclosed new crimes that but for the TRC, would have remained secret. Moreover, some of the secrets that came to light thanks to the TRC were so embarrassing that they had the potential to compromise their subjects. Consequently, those in possession of this information could have used it to blackmail the former perpetrators of repression and their collaborators. In some instances such blackmail would result in rents; in others, where those blackmailed were in positions of power—in policy concessions. The latter would contribute to political corruption and poor quality of democratic representation.[2]

In light of this discussion, we argue that truth commissions constitute an important class of *transparency regimes*. Transparency regimes are notoriously left out of the classical critiques of transitional justice. When Huntington writes that "[e]ven if a moral and legal argument could be made for prosecution, this would fall before the normative imperative of creating a stable democracy" he is implicitly assuming that transitional justice cannot be democracy-enhancing. In this paper we show that transparency regimes improve the quality of representation by preventing the blackmail of those former members and/or collaborators of the ancien régime that have embarrassing things to hide.

Bolstering our argument, recent work by Ang & Nalepa (2019) has posited the theoretical possibility that "doing nothing" may not produce immediate negative consequences, but over time may strengthen the power of authoritarian networks, particularly the net-

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[2]Ang & Nalepa (2019) write about the such blackmail of politicians with such "skeletons in the closet" and the potential of lustration—another transitional justice mechanism—to prevent it. Our mechanism is similar to lustration in that it can reveal secret information about the perpetrators of repression and their collaborators, but unlike lustration, truth commissions extend their focus beyond politicians. They cast a wider net both in terms of extending to a larger set of potential collaborators, and in terms of covering a greater range of crimes committed on behalf of the regime. In light of this, we believe truth commissions act faster and are more effective in delivering democracy-enhancing results than lustrations are.

works involving secret legacies of the authoritarian regime. Damaging information collected by the former authoritarian secret police for the benefit of authoritarian elites may turn elected politicians into clients of agents who threaten to reveal these politicians' "skeletons in the closet" (Nalepa 2010).

So what *are* the effects of truth commissions on the quality of democracy? We suggest that truth commission should improve the quality of democracy in two ways. First, by exposing "skeletons in the closet," these procedures remove the leverage that authoritarian networks have over elites in the democratic period. Second, truth commissions are able to dismantle authoritarian networks, particularly the clandestine ties underpinning those networks.[3]

The next section of this paper explores this argument in detail, leading to a more nuanced prediction about the effects of truth commissions on the prospects for democratic consolidation. It concludes with a summary of theoretical expectations regarding the effects of truth commissions on democratic outcomes. In Section 4, we first review existing research on identifying the causal relationship between transitional justice and dependent variables related to peace and democratic stability. Second, we present our own research design and explain how the dependent and independent variables are operationalized. Section 5 is devoted to data analysis and interpreting the results. We find that truth commissions are across all specifications robust in improving the quality of democracy. Section 6 concludes.

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[3]There is also an argument made in the context of Post-Communist Europe by (Horne & Levi 2004) that by exposing those who participated in human rights violations transparency regimes may restore to victims of the former autocracy or civil war ridden state a sense of justice and increase their trust in democratic institutions. We agree with this argument but are not in a position to test it, as it would require access to at least cross-sectional public opinion data.

2 How transparency regimes and truth commissions work

Among elites who sustained the former authoritarian regime are persons whose involvement in it is known, such as high ranking officials of authoritarian parties, and those whose identity is unknown, such as secret police informers and people who spied on their friends, family, and co-workers. While, broadly speaking, transitional justice can take many forms, most of these fall into (1) transparency regimes (2) purges, which can be thorough or restricted to the leadership of the authoritarian elite of military organization, (3) criminal trials, and (4) victim compensation (Elster 2004). In this paper, we focus on transparency regimes, and within transparency regimes, on truth commissions (Hayner 1994).

We borrow our understanding of truth commissions from Hayner (1994), seeing them as state-sanctioned "bodies set up to investigate a past history of human rights abuses in a particular country, which can include violations by the military or other government forces or armed opposition forces" (14).[4] Onur Bakiner offers more nuance to this definition by distinguishing truth commissions from "similar investigatory, judicial, or commemorative practices and institutions, such as parliamentary human rights commissions, courts, monitoring institutions and NGO's truth finding efforts" (Bakiner 2016, 11). Departing from Bakiner's distinctions, we do not exclude commissions of inquiry that examine human rights violations committed in more specific events than an entire period of authoritarian rule or civil war; we consider this inclusion justified as we also include commissions that only partially completed their mandate.

Authoritarian regimes sustain themselves thanks to agents who repress citizens of the authoritarian state either publicly or secretly. Following the democratic transition, both those who openly violated human rights and those who did so in secret may be held accountable for their abuses and secret collaborators of the regime might be revealed as per-

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[4]Truth commissions should (1) not focus on ongoing human rights abuses as a human rights ombudsman might; (2) examine a pattern of human rights abuses over time rather than a specific event; (3) be temporary; and (4) have an official sanction from the state to carry out its operations (Hayner 1994, 14).

petrators of atrocities. We argue that truth commissions, to the extent that they disclose atrocities committed in secret on behalf of the authoritarian regime, improves the quality of the newly inaugurated democratic regime.

This positive impact is achieved through two related mechanisms. First, unearthing secret collaborations prevents the blackmail of former collaborators or enforcers-turned politicians. Note that *unknown* or *secret* collaborators of former regimes might want to take part in the new regime. Yet, absent truth revelation procedures, they are susceptible to blackmail with the threat of revealing their 'skeletons in the closet', i.e. revealing that they participated in atrocious acts. These threats, especially when they come from those with credible access to such evidence, can distort policy and representational outcomes (Ang & Nalepa 2019, Nalepa & Sonin 2020). The threat of revealing kompromat holds sway over former elites, because the public likely cares about past abuses. Thus, revealing such 'skeletons' could chastise a public official, and end his or her career. In return for not denouncing them, blackmailers can demand rents or policy concessions, either of which negatively impacts the quality of democracy.

Nalepa & Sonin (2020) further elaborate on this blackmail mechanism, and show that blackmail can be directed at candidates who did not participate in the regime. The authors argue that the mere presence of secret authoritarian legacies enables blackmailers to pressure candidates into running on a non-representative platform (that is, a platform that diverges from the general voting population). This argument is presented in a formal model in which a politician who was a collaborator in the previous regime can tarnish the reputation of other candidates, leading to the reelection of sometimes extreme incumbents. In the same paper, the authors show that transitional justice procedures that reveal past atrocities, such as truth commissions, benefit voters regardless of the voters' preferences for normative transitional justice. In other words, the beneficial effects of transitional justice, in particular the effects of revealing skeletons in the closet, take hold regardless of– and sometimes despite–the public's demand for retribution.

We build on these already identified dynamics to hypothesize a second mechanism connecting uncovering secret collaborators and democratic quality: transparency regimes can upend existing authoritarian networks that linger on even after the transition to democracy. Former secret collaborators, if undisclosed can be manipulated by former authoritarian elites if those elites threaten to reveal information about their collaboration. In South Africa, such information was extremely dangerous. Even prior to the transition, the ANC would hold so-called "People's Courts" against suspected Apartheid collaborators. Those found guilty would be subjected to the excruciating torture of having a burning tire placed over their head (Price 1991).[5] Truth commissions can disempower authoritarian networks by:

1. providing the public with the opportunity to vote out wrongdoers via elections;

2. resulting in investigation that is actionable in a court of law;

3. providing specific sanctions against proven wrongdoers.

In outing and removing former secret collaborators, covert authoritarian networks are less likely to survive the transition to democracy.

In sum, truth commissions remove opportunities for blackmail–which may threaten the quality of democratic representation–and provide voters with information necessary for removing 'dishonest' officials–who were plausibly connected to former authoritarian elites. Importantly, these effects are brought about by the transparency aspect of the transitional justice mechanism, an effect that we argue is theoretically and empirically distinct from the criminal punishment of former members and collaborators of the ancien régime who abused human rights publicly. This leads us to conclude that truth commissions will contribute to improved quality of democracy in two directly observable ways. We hypothesize that by revealing abuses by secret agents of repression *truth commissions will reduce*

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[5]In this context, one sees how having ones name published in the TRC report constituted a considerable improvement over the horrific "doughnut death" as the burning tire sentence was called (Price 1991)

political corruption (by undercutting the blackmail mechanism described above) and *reduce the power of authoritarian networks*.

Before explaining how we design our research to test these two hypotheses, a small caveat is in order. Our discussion thus far has suggested that any kind of transparency regime including lustration can trigger this network-dismantling mechanism. Indeed, some might argue that lustration should be the most effective policy in dismantling authoritarian networks, precisely because it is so focused on political elites. We believe, however, that this focus might be counterproductive by placing constraints on the reach of transparency that lustration has to offer. In order to be subject to lustration, one has to actually run for office or occupy a public position. Truth commissions, meanwhile, cast a wider net both in terms of extending to a larger set of potential collaborators, and in terms of covering a greater range of crimes committed on behalf of the regime. In light of this, we believe truth commissions act faster and are more effective in delivering democracy-enhancing results than lustrations are.

3 Previous work identifying the effect of transitional justice

In the previous section, we have discussed how truth commissions investigate and reveal information about secret collaboration with the previous regime, and we have argued that the mere transparency aspect of this transitional justice mechanism can be beneficial for democracy. Non-transparency regimes allow authoritarian elites to threaten to expose secrets as a way of extracting policy concessions. Voters, meanwhile, are kept in the dark about whose interests are politicians really representing. First, by disclosing secret authoritarian legacies, truth commissions fulfill democratic goals because voters are better informed about candidates allowing them to choose the "honest" politicians. Second,

truth commissions can also undermine authoritarian networks and render them obsolete.[6] Politicians that have been named and shamed as collaborators by truth commissions can continue to try to represent voters, but such politicians will be more transparent to voters as their actions in office are less likely to be driven by the interests of potentially blackmailing authoritarian elites. Our hypotheses therefore predict that the intensification of truth commissions will decrease political corruption and eliminate authoritarian networks.

Our argument above, that truth commissions will improve the quality of representation because they reveal important information, requires empirical evidence beyond simply a correlational analysis. That is, we cannot simply argue that a positive relationship between truth commissions and measures of democratic quality are evidence of our theory because the identifying the effect of truth commissions is made hard by the fact that the same factors that cause states to embark on transitional justice may lead them to have better quality of representation down the line. Take as an example the relationship between lustration policies (that is, policies verifying if persons running for public office had secretly collaborated with the authoritarian political police) and programmatic representation as measured by expert surveys such as the Democratic Accountability and Linkages Project, as in Ang & Nalepa (2019). There is a real concern that the same factors that might promote lustration might also increase the extent to which experts perceive parties as running on identifiable and salient platforms that party members identify with ideologically. Given the multitude of drivers of programmatic representation, any relationship established in a cross-sectional research design between programmaticness at a given point in time and the severity of lustration may well be spurious.

Previous scholarship on the effects of justice institutions in transitional and post-conflict settings has used several techniques to address endogeneity concerns. In their work on the relationship between post-conflict justice processes and civil war recurrence, Loyle &

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[6]Note that truth commissions can also enable the application of punishment of former secret collaborators, for example, by creating a file that can be used in a criminal justice trial. But since our argument focuses on the effect of uncovering the truth, the sanction itself is neither necessary nor sufficient for the mechanism that we have highlighted here.

Appel (2017) take an instrumental variable approach, using the presence of transitional justice institutions in the region as an instrument. Assuming that neighbors with and without such institutions are assigned to any given post-conflict country at random and assuming that transitional justice institutions diffuse, these authors find that truth commissions, reparations, amnesties, and comprehensive trials—which they call "motivation post-conflict justice"— decrease the likelihood of conflict recurrence. As an additional robustness check to identify the determinants of post-conflict justice implementation, these authors also employ a strategy of matching on observables.[7] The results are consistent with their instrumental variable analysis.

Prorok (2017) also uses an instrumental variable approach, identifying three plausible instruments to estimate the causal effects of International Criminal Court (ICC) investigations on conflict duration in civil wars. Using as instruments (1) a state's affinity with the permanent five members of the UN Security Council, (2) a state's affinity with its neighbors, and (3) the number of neighboring states that have ratified the Rome Statute, Prorok finds that ICC involvement in a conflict setting significantly reduces the probability of conflict termination when government and rebel groups have committed similar levels of atrocities. The Court's counterproductive impact decreases as the number of atrocities committed by one party to a conflict increases relative to that committed by other parties.

Loyle & Appel (2017) and Prorok (2017) have made important contributions to understanding the causal effects of national and international-level transitional justice processes on conflict termination, an important prerequisite to having a healthy robust democracy. Their research, however, stops short of telling us how these mechanism affect the long term quality of democracy.

Balcells, Palanza & Voytas (2020) investigate the long terms effect of memorialization efforts, such as the erection of museums devoted to victims of authoritarian repression, on

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[7]This robustness check is an important improvement because an additional potential problem with an identification strategy that uses neighboring units as instruments is it's failure to satisfy the exclusion restriction, as demonstrated by Betz, Cook & Hollenbach (2019).

political behavior. Concentrating on Chile, whose regime ended in 1984, Balcells and her coauthors focus on detecting effects even among generations not directly affected by the Pinochet regime. They use a field experiment with visitors randomly assigned to visit a museum devoted to victims of military rule in Chile and an alternative museum completely unrelated to politics. The authors find a significant effect of the museum visit compared to the control condition on evaluations of the former regime and on political behavior. At the same time, through follow-up surveys with the panel they were able to assess the relatively short-lived nature of the effects, almost in no instance exceeding 6 months. Field experiments, however, are not feasible for the assessment of the effects of personnel transitional justice, such as truth commissions.

However, the robustness of democratic institutions is one of the questions motivating Capoccia & Pop-Eleches (2020). These authors make use of a natural experiment—the division of Allied-occupied Germany into four zones—to estimate the effects of transitional justice policies on democratic consolidation in post-WW II Germany. Treating the assignment to different denazification policies (trials and punishment) in each of the four occupation zones as exogenous, they estimate the effects of transitional justice policies on Germans' readiness to uphold democratic values. The dependent variable is measured with survey data from 1957. One key dependent variable probes public support for a one-party political system; a second dependent variable is constructed from a 1957 election survey. Capoccia & Pop-Eleches (2020) ultimately find that differences in the scope and severity of transitional justice implementation have a variety of differing effects on these two democratization indicators. This fascinating and innovative work is, however, unable to address questions about the broader effect that transitional justice mechanisms may have for countries that differ from postwar Germany.

Our research builds on these initial approaches, recognizing that understanding the effects of transitional justice requires isolating the effect of these mechanisms from factors that contributed to the implementation of transitional justice in the first place, as the latter

might be highly correlated with quality of democracy indicators.

4   Research design: Exploiting time and intensity of truth commissions

In order to test our predictions, we make use of the Global Transitional Justice Dataset Bates, Cinar & Nalepa (2020), which offers a panel of truth commission events for every country that transitioned to democracy between 1946 and 2016 (there are 81 such countries). The Dataset goes above and beyond of merely recording the presence or absence of a truth commission, as it records all events that at an institutional level pertain to truth commissions. The institutional level refers to the fact that the actor initiating the event must be a national government actor. This excludes members of the international community but also NGO and local authorities. Second, the event is classified so that it advances the work of a truth commission (positive events) or so that it inhibits it (negative events). For example, a proposal of a bill made to the floor of the legislature to create a truth commission is considered a positive event, while a presidential veto of bill to create a truth commission constitutes a negative event. Bates, Cinar & Nalepa (2020) also assign positive and negative status to events associated with the operation of the truth commission itself. So, for instance, the circulation of a report by the truth commission is classified as a positive event, while the stepping down of the chairperson of the commission prior to the end of the term or the defunding of the commission are classified as negative events. Finally, the GTJD team aggregated the positive and negative events annually, creating an unbalanced panel (unbalanced because states transitioned into democracies at different times and some transitioned out of democracy). A snapshot of the dataset, presented as trends of net (positive minus negative) truth commission events is presented in Figure 1 below. The figure on the left shows the disaggregated event data as function of years since transition (left) and as a function of objective time (right).

Figure 1: Disaggregated Truth Commission Data over Time
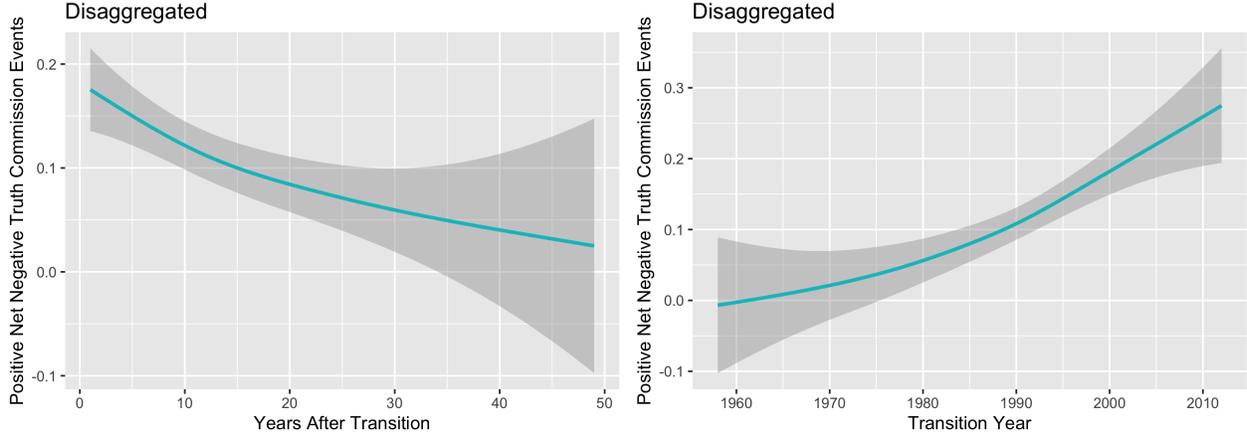
The GTJD team used numbers of positive and negative events as building blocks for the development of measures of, among other characteristics, truth commission severity. Here, in order to zero in on the the effects of truth commissions over time we use a slight modification of the measure of transitional justice severity developed by Bates, Cinar & Nalepa (2020), who measure the intensity of various transitional justice mechanisms using the total number of positive transitional justice events that occurred in country i over the total number of events plus one (one is added to the denominator to avoid dividing by zero). The way we modify the static measure is by assigning to each year a dummy variable that is defined as a function of severity up to that year.

More specifically, we first create the following "minimalist" measure:

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TJ^1_{i,t} = \begin{cases} 1 & \text{if } \sum_1^{t-1} P_{i,t} > 0; \\ 0 & \text{otherwise} \end{cases} \tag{1}
$$

where P_{it} represents the number of positive TJ events in country i in year $t-1$ and N_{it} represent the number of negative TJ events in country i in year $t-1$. This measure takes time into account very superficially by coding the year with the first positive transitional justice event as 1 and continuing to code as 1 every subsequent year, regardless of the ensuing TJ trajectory. The virtue of this measure is its simplicity, and we use it in all regressions

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below as a first model.

The next two measures focus on two different aspects of the unfolding of transitional justice trend. The first simply measures net events in country i in year $t-1$:

$$TJ_{i,t}^2 = \begin{cases} 1 & \text{if } P_{i,t-1} - N_{i,t-1} > 0; \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

The second measure uses the cumulative net events in year $t-1$ (measured as the total negative events in year t subtracted from the total positive events in year $t-1$), but assigns a 1 if that number is positive and 0 if is negative:

$$TJ_{i,t}^3 = \begin{cases} 1 & \text{if } \sum_1^{t-1} P_{it} - \sum_1^{t-1} N_{it} > 0; \\ 0 & \text{otherwise} \end{cases} \tag{3}$$

In one sense, these measures resemble the original severity measures developed by Bates, Cinar & Nalepa (2020), because they continuously capture the intensity of transitional justice. However, in contrast to their static measure, ours make use of the unfolding of transitional justice over time. In doing so, our measure better leverages the time series aspect of the Global Transitional Justice Dataset.

Figure 2 illustrates how this last measure captures truth commissions in the Global Transitional Justice Dataset. Countries are stacked on the vertical dimension while the horizontal dimension represents years since the transition. Dark blue cells illustrate years in which the cumulative net transitional justice events were positive ($\sum_1^{t-1} P_{it} - \sum_1^{t-1} N_{it} > 0$), and light blue cells illustrate years in which the cumulative net transitional justice events were negative or equal to zero ($\sum_1^{t-1} P_{it} - \sum_1^{t-1} N_{it} \leq 0$). White cells represent data that has not yet been revealed because the country in the corresponding row has not been democratic long enough.

All the measures presented above use dichotomous variations of the severity measures.

Figure 2: Truth commissions at a glance according to summary measure
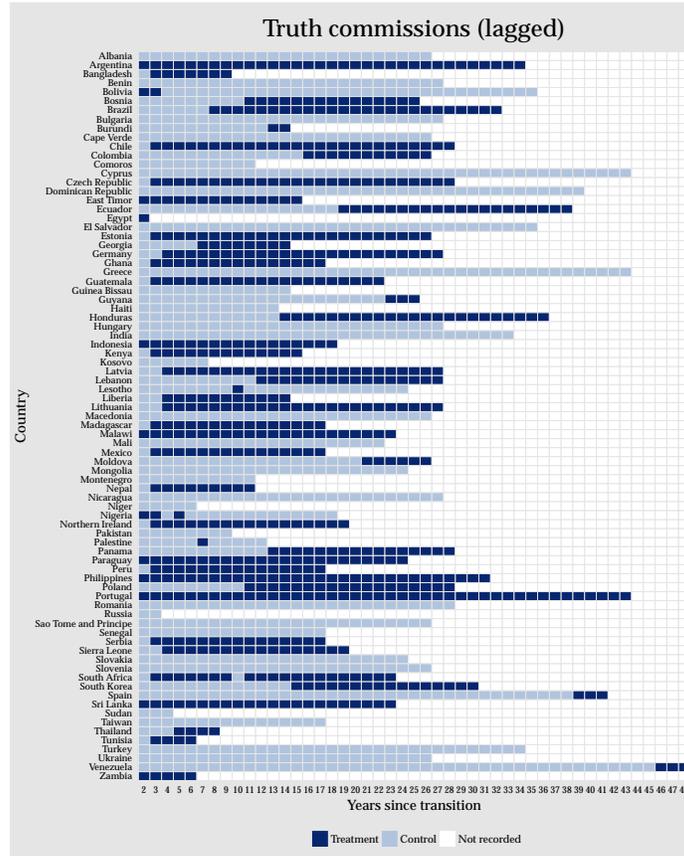
Dark blue cells represent years in which net cumulative truth commission events were positive; light blue cells represent years in which net cumulative truth commission events were negative; white cells represent censored data.

This is because indicator variables are easier to interpret in a diff-in-diff framework. All the regressions we perform used one year lags of these newly created measures, although figures 12 and 13 in the appendix summarize the coefficients obtained from running the regressions on ten different lags. The use of lags is justified by the fact that any plausible model of the effect of transitional justice on the quality of democracy should allow for some lapse of time between the occurrence of transitional justice and outcome, which in our case is the quality of democracy variable.

4.1 The identification strategy

Finding dependent variables for a classical observational study of the relationship between transitional justice and the quality of democracy is very challenging because the implementation of transitional justice is endogenous to phenomena that are used as building blocks of so many democratic indicators, ranging from rule law to freedom from discrimination to freedom to run for office. We address this problem by implementing the difference-in-difference research design.

The classic explanation of the diff-in-diff method relies on comparisons of time trends in countries that have been treated with the independent variable of interest—in our case, truth commissions—and those that have not received treatment. As long as the the "treated" and "control" countries have been matched to ensure that their pre-treatment trends on the dependent variable are similar enough (satisfying the parallel trends assumption), such a comparison is warranted. The case without the treatment serves as a counterfactual to the case with the treatment. Given appropriate data, this method can be generalized to panel data and incorporated into a regression framework. The data requirements, however, are quite stringent, as they require both the dependent and independent variables to vary across countries and over time.

Fortunately, we could take advantage of the Varieties of Democracy (V-Dem) project's indicators of political corruption and measures of authoritarian turnover, which match closely the aspects of quality of democracy influenced by truth commissions in our argument. V-Dem is a dataset created by interviewing country experts (approximately 5 independent experts per country) and coding subjectively a host of regime characteristics that are not directly observable over time. V-Dem is exceptional among expert surveys in that it corrects for how differences of opinion or mistakes cause experts to diverge in their evaluations. Traditionally, datasets report expert-coded data with means and standard deviations, ignoring the fact that expert reliability and the way in which experts apply an ordinal scale to ratings may systematically vary. V-Dem, however, uses item response

theory to model and adjust for differences in how experts apply scales (Pemstein, Marquardt, Tzelgov, Wang & Miri 2015). In order to allow for scaling the independent coding by country experts, V-Dem scholars also encouraged experts to "bridge code" a second or third country. Although experts have less expertise in evaluating these second and third countries than they have at evaluating the countries in which they have primary expertise, this effort allowed V-Dem methodologists to compare the use of the ordinal scales across coders and correct for systematic differences.

V-Dem researchers asked about three thousand country experts hundreds of questions to arrive at 5 general indexes - electoral, liberal, participatory, deliberative, and egalitarian. Since these indices are somewhat broad, we focused on two variables that measure distortions in the quality of representation and the extent to which authoritarian elites have been able to reproduce. The first V-Dem variable that we use for this purpose is the Political Corruption Index (*v2x_corr*); the second is Power Distributed by Socioeconomic Status (*v2pepwrses*). We discuss each in turn below.

4.1.1   Political Corruption Index

Building on our theoretical discussion, we note that blackmail with skeletons in the closet cannot be directly observed. What we can detect, however, are the consequences of blackmail: policy distortion created as a result of extracting concessions by blackmailers. One of the ways this will be observed is a as political corruption.

The V-Dem index of political corruption is a "meta-index" in that it is made up of several component indices. Among them are the executive corruption index (*v2x_execorr*),[8] public sector corruption (*v2x_pubcorr*),[9] and judicial corruption (*v2x_jucorrdc*).[10] The in-

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[8]This variable was created from expert answers to the question "How routinely do members of the executive or their agents grant favors in exchange for bribes, kickbacks, or other material inducements, and how often do they steal, embezzle, or misappropriate public funds or other state resources for personal or family use?"

[9]This variable was created from answers to the question "To what extent do public sector employees grant favors in exchange for bribes, kickbacks, or other material inducements, and how often do they steal, embezzle, or misappropriate public funds or other state resources for personal or family use?"

[10]This variable was created from answers to the question "How often do individuals or businesses make

dex takes on values between 0 and 1, but we have transformed it to take values between -1 and 0 so that higher values correspond to higher quality of democracy and to ensure its directionality is the same as that of our second dependent variable. While the index in question does not get directly at the the extent to which politicians are influenced by blackmail with secret police files, it does measure how much they succumb to pressures that impede their ability to represent.

In order to use the diff-in-diff framework, we must assume that absent treatment, political corruption trends would develop according to a similar pattern as in countries that were never treated with transitional justice. This assumption is commonly referred to as the "parallel trends assumption."

Figures 3 and 4 compare the average political corruption index of all countries that never received treatment with average values of the political corruption index that received treatment after the 2, 3, 5, and 10 years following their democratic transition.

(a) treated after 2 years or more
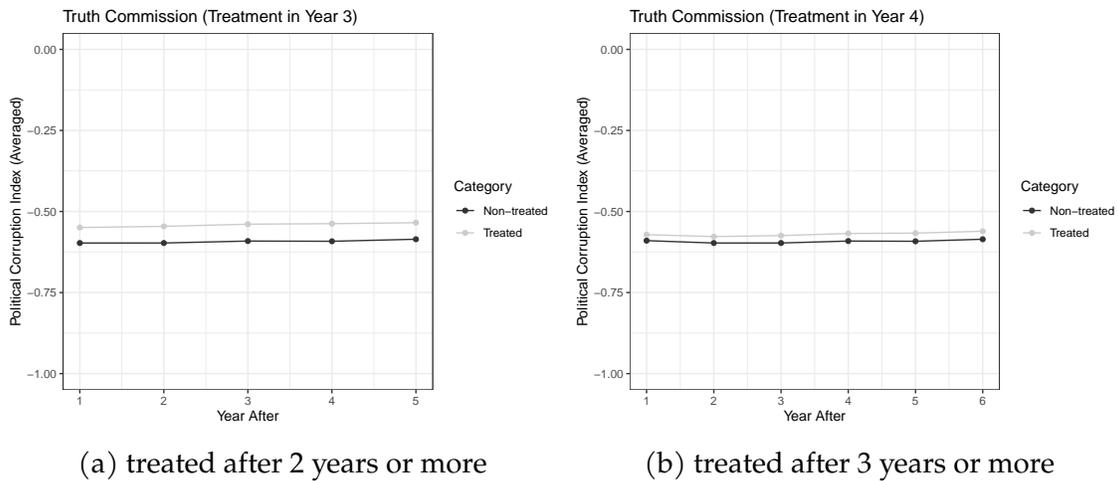
(b) treated after 3 years or more

Figure 3: Parallel trends for political corruption index, treatment: truth commissions as positive net cumulative events (early adopters)

In all these graphs, we interpret "being treated by a truth commission" as taking the value of 1 in the cumulative net events measure described in equation 3.

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undocumented extra payments or bribes in order to speed up or delay the process or to obtain a favorable judicial decision?"

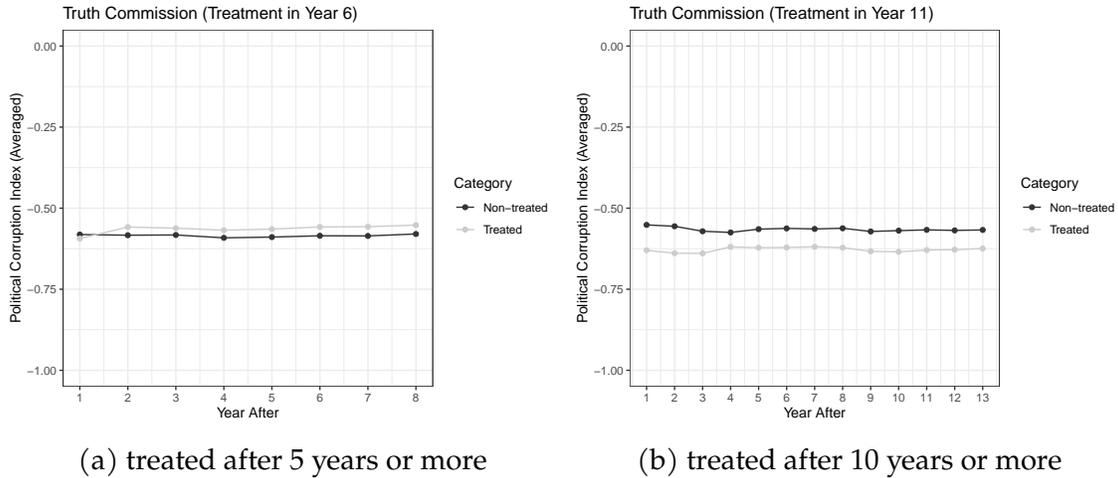(a) treated after 5 years or more   (b) treated after 10 years or more

Figure 4: Parallel trends for political corruption index, treatment: truth commissions as positive net cumulative events (late adopters)

The first apparent observation here is that countries treated with truth commissions, regardless of when the treatment took place, have less political corruption than countries not treated with truth commissions, but the gap between the treated and untreated narrows with time. More concretely, the gap between untreated countries and those treated in year 3 or later is .05, but about .4 and only .1 by year 10. The trends are mostly parallel with the exception of the beginning of the trend for the treatment in year 5 or later, where they cross. At the same time, there does not seem to be a big difference in the values of the dependent variables resulting from the treatment. Namely, there is no "break" in the trend line around year 2 in the top left figure or year 10 in the bottom right. At the same time, there does seem to be a small uptick in the treated trend relative to the untreated in the top right figure around year 3 and in the bottom left around year 5. Recall, this means that countries whose first year with more positive than negative truth commission events was year 3 (or 5) seem to experience less political corruption relative to countries that did not have more positive truth commission events than negative truth commission events by that time. It is worth noticing that in all of the parallel trends figures above not only the treatment group but also the comparison group changes: the control group is always made up of the compliment set to the treated countries **within the group of countries that**

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had a net cumulative value of 0 up to the year illustrated in the figure. Consequently, the figure in the top left panel uses many more cases than the figure in the bottom right panel.

Another point to keep in mind while interpreting these figures is that there are many factors (economic wealth key among them) in addition to transitional justice that affect political corruption trends. Since the diff-in-diff design calls for time varying covariates, many factors—such as historical legacies—are impossible to control for. However, we can control in all regressions for GDP per capita, implying that the parallel trends assumption must only hold conditional on GDP per capita. At the same time, because presenting parallel trends graphs conditional on continuously-measured variables is very difficult to do, the graphs of parallel trends presented above do not take GDP per capita into account.

4.1.2 Political Power Distributed by Socioeconomic Status

The second dependent variable we use taps directly into the ability of authoritarian elites to survive the transition and flourish. To understand its suitability, it is useful to consider why the power of former authoritarian elites may extend beyond the life span of an authoritarian regime. Autocrats may be well positioned to capture state resources at the time of democratic transition, which they can then use to entrench themselves in power (Brun & Diamond 2014, Haggard & Kaufman 2016, Albertus & Menaldo 2014).

The outgoing autocrats' access to resources can be cut off if they or their successors are voted out of office following the transition to democracy. Various cases from around the world demonstrate, however, that this removal may only be temporary (Kitschelt 1999). Grzymala-Busse (2002), for instance, attributes the revival of successor authoritarian parties to the organizational advantage authoritarian parties hold over parties that are new to the party system. This organizational advantage allows them to make better use of state resources when they eventually do find themselves in government. Transitional justice institutions are often portrayed as the last resort to curb autocrats' unfair advantage. Indeed,

scholars of transitional justice have argued that its mechanisms should undercut the privileged position of members or parties of the former autocrats, their collaborators, or their enforcement apparatuses (Stan et al. 2009, David 2011, Vinjamuri & Snyder 2004, Escriba-Folch & Wright 2015).

In light of this argument, transitional justice institutions may plausibly be interpreted as mechanisms preventing former authoritarian elites from holding on to such economic resources. Therefore, a variable measuring the association between economic wealth and political power is an ideal candidate for a dependent variable operationalizing the effects of transitional justice on the quality of democracy. Additionally, given the temporal nature of our data, an ideally suited dependent variable also measures this association over time. Fortunately, the V-Dem Expert Survey contains such a measure.

Called "Political Power Distributed by Socio-economic Status" (*v2pepwrses*), the variable is based on the following question posed to V-Dem experts: "is political power distributed according to socioeconomic position?"(Coppedge, Gerring, Lindberg, Skaaning, Teorell, Ciobanu & Saxer 2017).[11] In his clarification note, John Gerring elaborates that the measure was designed to gauge the extent to which inequalities translate into political power (Coppedge et.al. 2017b). Other scholars of non-democratic regimes have noted that the ability of economic elites to lock in political power is one of the marks of high capacity authoritarian states. In the case of some non-democracies, as illustrated by Hollenbach (2019), the link between political and economic power can be fully institutionalized.[12] Power Distributed by Socioeconomic Status is also a particularly reasonable

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[11]Answers to the question were distributed along a 5-point scale. The possible answers included (0)"Wealthy people enjoy a virtual monopoly on political power. Average and poorer people have almost no influence"; (1)"Wealthy people enjoy a dominant hold on political power. People of average income have little say. Poorer people have essentially no influence"; (2)"Wealthy people have a very strong hold on political power. People of average or poorer income have some degree of influence but only on issues that matter less for wealthy people"; (3)"Wealthy people have more political power than others. But people of average income have almost as much influence and poor people also have a significant degree of political power"; and (4)"Wealthy people have no more political power than those whose economic status is average or poor. Political power is more or less equally distributed across economic groups" (Coppedge et.al. 2017b).

[12]In his article on elite interests and public spending in 19th century Prussia, Hollenbach explains how in the empire's cities, a portion of the male population contributing a third of the tax revenue would have the same voting power and the portion of the male population contributing the second third of the revenue

measure of quality of democracy for our purposes because while it measures an important aspect of democracy, it is unlikely to be correlated with rule of law, which could also affect the implementation of transitional justice. If the goal of personnel transitional justice is to undermine the privileged position of authoritarian elites, this score should increase with the severity of the transitional justice mechanism in question.

We again start with investigating the parallel trends assumption when using PdSES as the outcome variable. Truth commissions have been operationalized as a treatment in exactly the same way as in Figures 3 and 4:

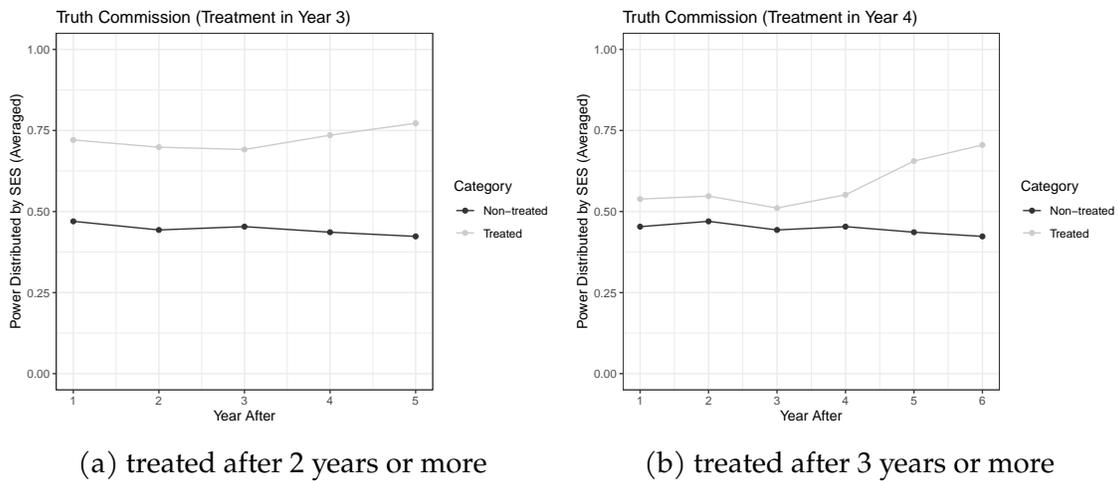(a) treated after 2 years or more   (b) treated after 3 years or more

Figure 5: Parallel trends for Power distributed according to SES, treatment: truth commissions as positive net cumulative events (early adopters)

Comparing the PdSES trends of countries treated and untreated with truth commissions reveals that for all but very late adopters, the trends before treatment are parallel, and there is a very clear uptick in quality of democracy following treatment. These trends clearly indicate that a diff-in-diff framework is an appropriate approach to investigating the effect of truth commissions. They also suggest that truth commissions may help eliminate vestiges of authoritarian rule to the extent that these vestiges are captured by PdSES.

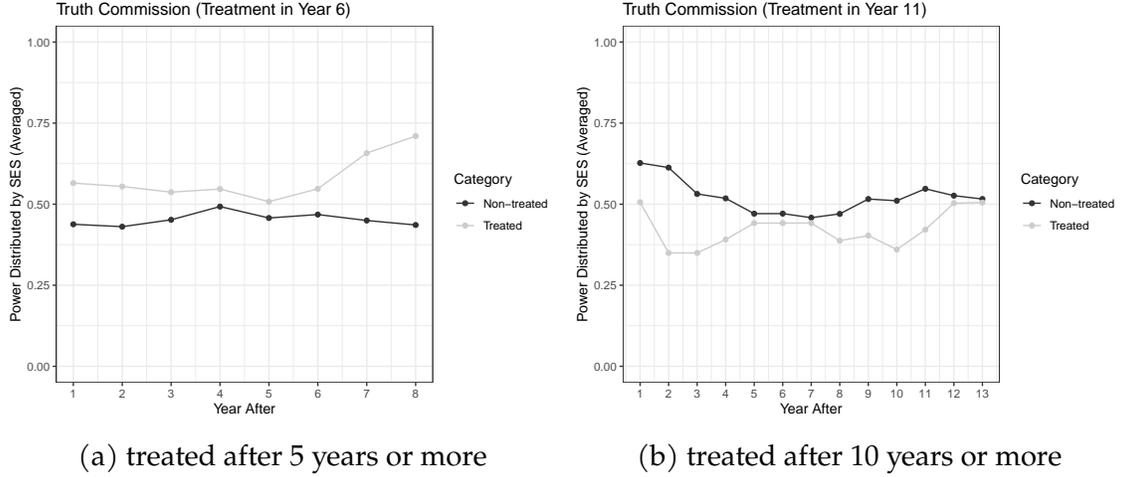and the bottom third of the tax revenue (Hollenbach 2019).

(a) treated after 5 years or more (b) treated after 10 years or more

Figure 6: Parallel trends for Power distributed according to SES, treatment: truth commissions as positive net cumulative events (late adopters)

4.2 The statistical model

A traditional difference-in-difference framework with just one pre-treatment and one post-treatment period would estimate:

$$Y_{i,t} = \mu_i M_I + \lambda T + \gamma D_{i,t} + \beta X_{i,t} + \epsilon_i$$

where $D_i = 1$ if country i experienced transitional justice of a given type, and $D_i = 0$ otherwise. M_I is the country dummy and assumes 1 when the TJ event is associated with country i.[13] In the classic difference-in-difference set-up, there are only two periods: $t = 0$ for the pre-treatment period and $t = 1$ for the post-treatment period. Consequently, μ_i can be interpreted as the country intercept and λ as the post-treatment intercept. $T = 0$ in the pre-treatment period, and $T = 1$ in the post-treatment period. $X_{i,t}$ represents the set of covariates upon which the treatment effect is conditioned. Note that a consequence of the above notation is that $D_{i,t} = M_i * T$.

The panel structure of our data calls for two important adjustments to the model.

First, each country receives the treatment at a different time t (that is, each country

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[13]Note that there are one fewer dummies than there are countries.

has a different year that marks the pre- and post-treatment period). Second, each country appears in the dataset for multiple periods. To correctly specify the diff-in-diff model with multiple time periods, we build on Angrist & Pischke (2008) and Besley & Burgess (2004), and propose to estimate the following model:

$$Y_{i,t} = \mu_i M_i + \lambda_t T_t + \gamma D_{i,t-1} + \beta X_{i,t} + \epsilon_i \tag{4}$$

This is essentially a two-way fixed effect regression where μ_i represents country-specific fixed effects and λ_t represents year-since-transition specific fixed effects. T is the time period dummy and is equal to 0 in every year lapsed since the transition except for T in row $Y_{i,T}$. Note that in the regression framework above, we use $D_{i,t-1}$, the treatment from the year preceding the year in which the dependent variable was recorded. $D_j, t-1$ can also be written as $\mu_i * \lambda_{t-1}$; it takes the value of 1 when country i was treated in period $t-1$. $X_{i,t}$ represents the set of covariates conditioning the treatment. Here, we use a single covariate: GDP per capita.

In a paper from 2018, Andrew Goodman-Bacon shows that this general difference-in-difference estimator is a weighted average of all possible two-group and two-period diff-in-diff estimators in the data, with the greatest weights given to treatments that occurred close to the "middle of the panel" (Goodman-Bacon 2018). This is because the weights are directly proportional to the treatment variances.[14]

4.3   Results and Discussion

We begin by estimating the effect of severity of truth commissions on the political corruption index. In the following section, we estimate the effect of truth commissions on Power Distributed by Socioeconomic Status. All models presented here control for GDP per capita (logged).

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[14]In our case, these would be the cases treated after at least three years after the transition compared to those that were never treated.

4.3.1 Political Corruption

Table 1: Political Corruption and Truth Commission Events

|	Political Corruption		
	(1)	(2)	(3)
Minimalist Truth Commission Events	0.034*		
	(.015)		
Net Truth Commission Events		.012*	
		(.0061507)	
Cummulative Net Truth Commission Events			0.030*
			(.015)
GDP per capita (logged)	.015	.018	.016
	(0..038)	(.039)	(.038)
Country FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	1,759	1,759	1,759
R^2	0.101	0.088	0.099

Note: $^*p<0.05; ^{**}p<0.01; ^{***}p<0.001$
All Truth Commission events are lagged

Table 1 presents three models, summarized also in Figure 7, each pertaining to one of the three measures of truth commission events. First, it is clear that truth commissions, regardless of how they are measured, have a positive effect on reducing corruption: relative to countries with no positive events at all, having one positive event decreases corruption by almost 3.5 percentage points. Years following years with more positive than negative events see an uptick of almost 12 percentage points and years following a cumulative number of positive events exceeding negative events see political corruption decreasing by 30 percentage points.

While political corruption is a category that ought to tap into the extent to which persons in public office can be blackmailed with the threat of revealing secrets from their authoritarian past, the appendix replicates this analysis for legislative corruption ("v2xlgcrrpt"), judicial corruption ("v2jucorrdc"), public sector corruption ("v2xpubcorr"), and execu-

Figure 7: Diff-in-Diff regression of Political Corruption on Three Measures of Truth Commission Events (TCE) with Country Clustered SE
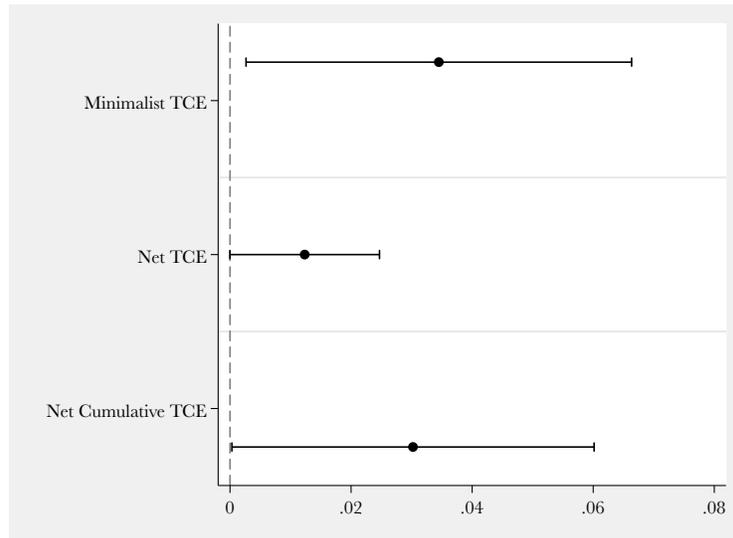
tive corruption ("'v2xexecorr").[15]

These components can be understood as casting a narrower net and focusing on the acts of bureaucrats and administrators of the state and the extent to which these actors are willing to take bribes. None of these alternative variables, however, produces results that depart significantly from what we uncovered in the case of the general political corruption index.

The next subsection presents results from regressions similar to the ones above, but uses the association between economic and political status as the dependent variable. Recall that the association between economic and political power is intended to capture legacies of the former authoritarian state that are still alive and well, as in authoritarian states, economic and political power are closely associated with one another.

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[15]Specifically, these subcomponents are operationalized as follows: public sector corruption is constructed out of answers to: "To what extent do public sector employees grant favors in exchange for bribes, kickbacks, or other material inducements, and how often do they steal, embezzle, or misappropriate public funds or other state resources for personal or family use?"; executive corruption codes answers to: "How routinely do members of the executive, or their agents grant favors in exchange for bribes, kickbacks, or other material inducements, and how often do they steal, embezzle, or misappropriate public funds or other state resources for personal or family use?"; judicial corruption is constructed out of answers to: "How often do individuals or businesses make undocumented extra payments or bribes in order to speed up or delay the process or to obtain a favorable judicial decision?" and legislative corruption codes answers to: "Do members of the legislature abuse their position for financial gain?"

4.3.2 Power Distributed by Socioeconomic Status

The second V-Dem variable we seek to predict with data on truth commissions is "Power Distributed by Socioeconomic Status," described in detail in section 4.1.2. This variable, recall, measures the ability of former authoritarian elites to resist turnover and persist in their ability to accumulate political and economic wealth. It taps into a very different aspect of quality of democracy than corruption, which is why it complements the analysis well. Moreover, as remarked before, it is conceptually unrelated to rule of law and concerns for accountability—two concepts so closely related to transitional justice that it would be problematic to place them on opposite sides of a regression.

The table below shows the results from the diff-in-diff regressions of PdSES on truth commission events, again using three different measures of truth commission events: minimalist, net events per year, and cumulative net events per year.

Table 2: The Effect of Truth Commissions on power distributed by socioeconomic status

	Power distributed by socio-economic status		
	(1)	(2)	(3)
Minimalist Truth Commission	.213*		
	(.1044532)		
Net Truth Commissions		.0531499	
		(.0304614)	
Cumulative Net Truth Commissions			.204*
			(.1000583)
GDP per capita (logged)	.279	.297	.285
	(.274)	(.280)	(.275)
Country FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	1,759	1,759	1,759
R^2	0.067	0.052	0.067

Note: $^*p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$
All Truth Commission events are lagged

The models indicate that truth commission events decrease the extent to which political power is coupled with economic power. Recall that the quality of democracy indicator

Figure 8: Diff-in-Diff regression of Power Distributed by Socioeconomic Status on Three Measures of Truth Commission Events with Country Clustered SE
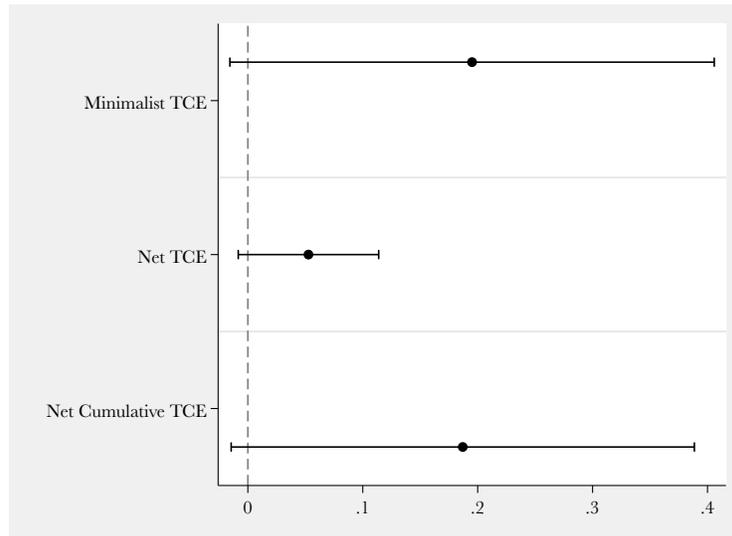
here is measured on a 5-point scale and so the effects of truth commission events are not as large as in the case of political corruption. The strongest effects—for the minimalist and cumulative net measures—are only associated with changes of 4 percentage points. These changes, however, are statistically significant and in the predicted direction. Thus, the data analysis carried out in this section within a difference-in-difference framework supports the main expectation described previously: truth commissions are an effective tool for improving the quality of democracy.

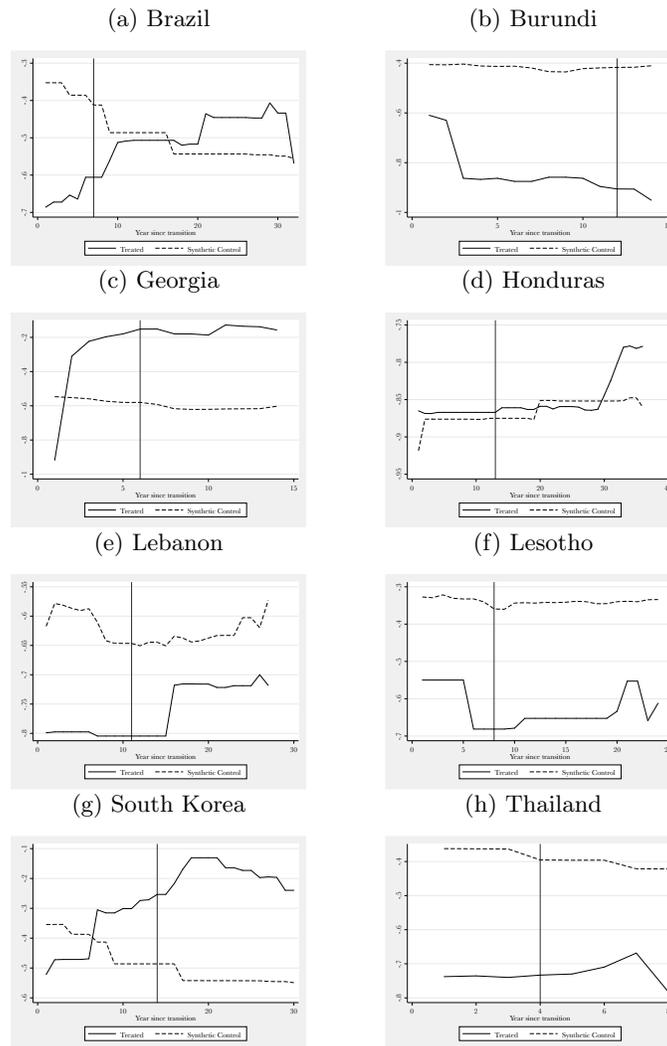## 4.4 Robustness check: Synthetic matching

Before concluding, as a robustness check, we also present a series of synthetic matching estimations. The underlying logic of synthetic matching is to borrow information from a number of untreated units to create a synthetic counterfactual that can be compared to the trajectory of the treated unit. Effectively, the observed untreated units are compared on a number of covariates to the treated unit during the pre-treatment period. From that comparison, a series of weights are assigned to each of the untreated units to create the synthetic control: a weighted average of the outcome variable that spans both the pre-

and post-treatment period. Following this process, we are able to compare the trajectory of the dependent variable of the treated unit to the synthetic match (Abadie, Diamond & Hainmueller 2015).

In its classic presentation, synthetic matching requires a balanced panel and a single treated unit. In addition, the method estimates the weights based on the trajectory pre-treatment, so one should be able to observe the treated unit a number of time periods before the treatment is administered. Our panel, however, is not balanced and some countries experience truth commission events immediately upon transition. Despite this, we were able to process the data to conduct a conservative synthetic matching robustness check as follows: We begin by selecting only the countries that treated with a Truth Commission Event (TCE) 4 years or more following the transition transition. In total, our dataset had eight such countries. The control groups for each one of these countries, were created out of untreated countries that survived at least as many following the transition as the treated country. We used GDP per capita as the matching covariate. Thanks to this operation, we were able to balance the panel and estimate the synthetic matching parameters.

In the Figures 9 an 10 below we compare the 8 treated countries with their corresponding synthetic control according to the two outcome variables we focus on in this paper. Starting with the political corruption variable, we see that all countries begin their trend in the quality of democracy indicator lower than their synthetic control, but some, as we argue as result of the treatment overtake the control (as Georgia, Brazil and South Korean), whereas other close the gap to the synthetic control. The only exception to this is Burundi, which experienced its first TCE very late, however (12 years following the transition to democracy). The vertical line marks the year following the transition to democracy in which the country first experienced a truth commission event. Since or theory is ambivalent regarding how long it takes for a truth commission to reduce political corruption, this kind of presentation, allows us to observe when divergence in trajectories might oc-

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Figure 9: Synthetic matching for selected countries (outcome: Political Corruption)

(a) Brazil

(b) Burundi

(c) Georgia

(d) Honduras

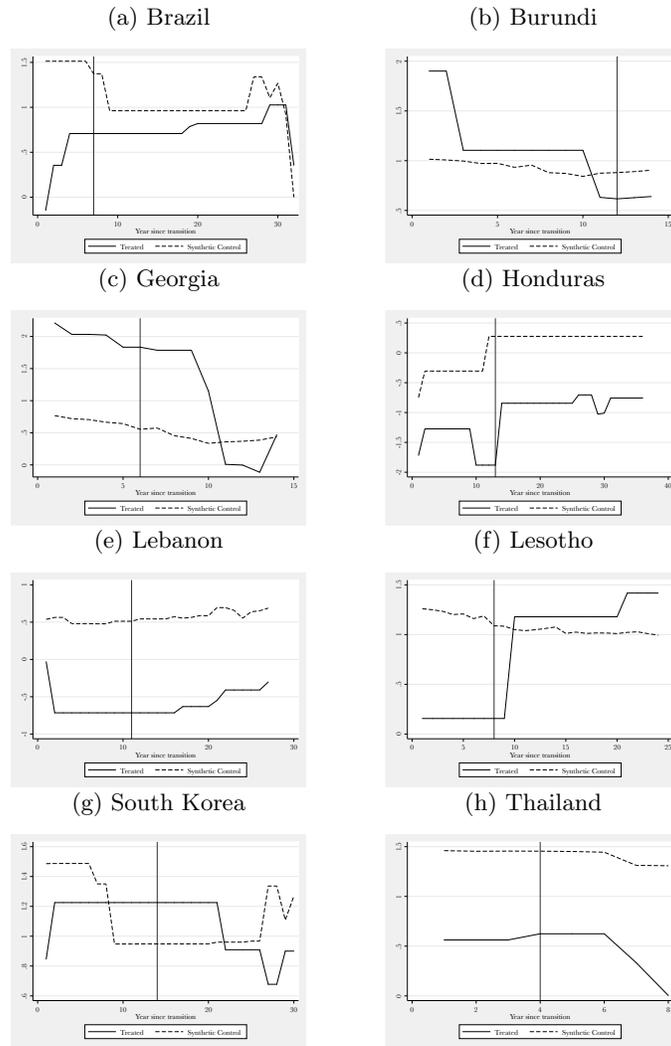(e) Lebanon

(f) Lesotho

(g) South Korea

(h) Thailand

cur.[16] Since each of these treated countries are observed for different periods of time, and thus have a different feasible set of countries to compare them to, the synthetic matches shown below are not comparable across figures.

Figure 10, summarizing the comparisons of treated countries with their synthetic matches according to the second outcome variable, PdSES, is less consistent with our hypotheses. Only in the case of Lesotho is there a clear response in the the predicted direction. Taken

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[16]Alternatively, the appendix to this paper presents the dif-in-dif regressions for alternative lags, but for the specific eight countries portrayed here, the exact moment when an uptick by TCE is induced is particularly easy to observe.

Figure 10: Synthetic matching for selected countries (outcome: PdSES)

(a) Brazil

(b) Burundi

(c) Georgia

(d) Honduras

(e) Lebanon

(f) Lesotho

(g) South Korea

(h) Thailand

together, thus, these results increase our confidence in the ability of truth commissions to decrease political corruption, though not necessarily to decrease the association between political and economic wealth.

5 Conclusion

This paper is a first attempt at testing a causal theory of transitional justice with a global dataset that disaggregates transitional justice events across time and by mechanism. The ability to distinguish between different mechanisms allows us to theorize about the differing effects of truth revelations procedures. The general expectation is that more truth revelation should lead to better democratic outcomes. This is because revealing the truth about the past misdeeds of elites prevents authoritarian or conflict-era networks from extorting policy concessions from elected politicians. Absent truth revelation, former agents of the secret police, for example, could blackmail collaborators who have assumed political office and threaten to reveal "skeletons in their closets" (Ang & Nalepa 2019) were the blackmailed politicians to refuse responding to former agents' demands.

We further suggested that truth commissions should be more effective at improving the quality of democracy than lustration because truth commissions cast a wide net in society, thus providing more greater opportunities for the past to come to light. Lustration, on the other hand, is often restricted in its reach to public officials and thus faces more obstacles. Its effects may therefore take longer to observe.

Our data analysis, carried out within a difference-in-difference framework, supports the main expectation described above, namely that truth revelation procedures should improve the quality of democracy. We find moderate evidence in support of the more specific expectation that truth commissions are an effective tool for improving the quality of democracy. To some extent, these moderate results can be explained by the detailed information lost while creating time-varying measures of transitional justice severity.

An explanation for the lustration results in particular may hinge on the way that lustration operates. Perhaps, compared to truth commissions, lustration requires a larges mass of positive events relative to negative events and thus, a measure picking up such intensity would lead to different outcome. As indicated earlier, the diff-in-diff framework compelled us to force a count measure of transitional justice events into an indicator variable. An alternative specification would use the original severity measure developed by Bates, Cinar & Nalepa (2020) in an HLM framework. Future work can address theoretical expectations about truth commissions and lustration policies more specifically, exploring in detail the mechanisms by which each truth revelation procedure operates to increase or decrease the quality of democracy.

Finally, we underscore our paper's empirical contribution to the growing research agenda on transitional justice datasets and democratic outcomes. Recent years has seen a number of transitional justice datasets (Binningsbø, Loyle, Gates & Elster 2012, Van der Merwe, Baxter & Chapman 2009, Thoms, Ron & Paris 2010, Loyle & Binningsbø 2018). The literature's main focus has been used on identifying the effects of a variety of TJ measures on variables like trust in governmental institutions (Horne 2017), peace (Binningsbø et al. 2012), or democratic stability (Olsen, Payne & Reiter 2010). Therefore, our approach contributes in three specific ways. First, it accounts for the dynamic nature of truth-commission implementation. Second, it proposes an empirical approach to test a mechanisms that is notoriously hard to detect, Finally, we propose two observable variables that can plausibly inform us whether truth commissions can prevent former autocrats from reasserting their political dominance in the new regime.

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A Empirical Appendix

This appendix is composed of four sections. In the first, we conduct a simple OLS regression on the pooled data, just as a benchmark for our diff-in-diff analysis. In the second, we run diff-in-diff analysis on all the subcomponents of the political corruption index. In the third, we explore the first ten lags of truth commissions events (as opposed to just the first lag) used in the same diff-in-diff framework. Finally in the fourth section, to justify our use of lustration events in the main text, we also present evidence that the parallel trends assumption is satisfied for lustration events.

A.1 OLS with pooled data

The results show a positive association between truth commission events (with measured with the minimalist measure or with the net cumulative measure for both political corruption and PdSES. However, as discussed in the main text without controlling for selection into treatment, as a diff-in-diff framework allows us to, we do not not know if this relationship is spurious. It is also worth noting that there is not significant relationship between net truth commission events and the two quality of democracy indicators examined here.

Table 3: OLS regressions of Political Corruption and PdSES on Truth Commissions

	(1) PdSES	(2) PdSES	(3) PdSES	(4) Pol Corruption	(5) v2x.corr	(6) v2x.corr
Minimalist TC	0.124** (3.12)			0.0465*** (3.88)		
Net TC Events		0.00247 (0.03)			0.00614 (0.29)	
Ne Cumulative TC Events			0.0844* (2.11)			0.0730*** (6.12)
Log GDP	0.0626*** (6.24)	0.0680*** (6.85)	0.0641*** (6.38)	0.0373*** (12.39)	0.0392*** (13.15)	0.0360*** (12.00)
Years Since Transition	0.00835*** (3.82)	0.00867*** (3.93)	0.00851*** (3.88)	0.00185** (2.82)	0.00199** (3.00)	0.00184** (2.82)
Constant	−1.054*** (−4.43)	−1.131*** (−4.76)	−1.073*** (−4.49)	−1.486*** (−20.79)	−1.514*** (−21.19)	−1.464*** (−20.59)
Observations	1747	1747	1747	1747	1747	1747

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 11: Coefficients from diff-in-diff Regressions of Components of Political Corruption Index on Truth Commission Events
Note: All regressions are using country clustered SE and net cumulative events to measure the intensity of Truth Commissions
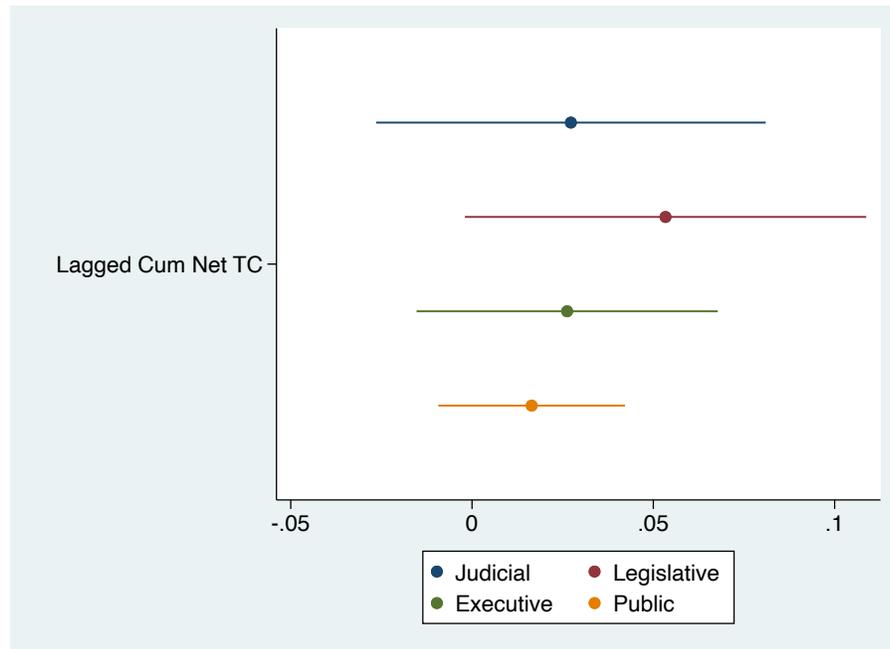
A.2   Decomposing political corruption

In the next step, we decompose the political corruption index into its sub-components: executive, public, judicial, and legislative corruption. Here we use the statistical model described in equation 4 4. The results are summarized in the following coefficient plot.

Our measure of cumulative net truth commissions has a positive effect on reducing each of the components of corruption, although some—most notably Public Corruption and Executive corruption—have much narrower confidence intervals. Note, that even though the subcomponents were originally measured on different scales, they have been normalized for comparability to a 0-to-1 scale.

A.3   Robustness check: Expanding the lags in the diff-in-diff

Last but not least, we consider, as a robustness check, models that include lags for different numbers of years. We do this because our previous analyses rest on the assumption that a one-year lag is sufficient to capture the start-up costs of truth commissions. It may be presumptuous to think that truth commissions start having an effect as early as one year after the first truth commission event occurs. Therefore, in this section, we report the coefficients on ten different lags used in our diff-in-diff framework. We use our measure of net cumulative truth commission events. Figure 12 presents the results for political corruption and Figure 13 illustrates the coefficients for power distributed by socioeconomic status. As can be seen in both figures, our decision to lag our truth commission variable

Figure 12: Diff-in-Diff regression coefficients for 10 lags, Net Cumulative TCE (political corruption)
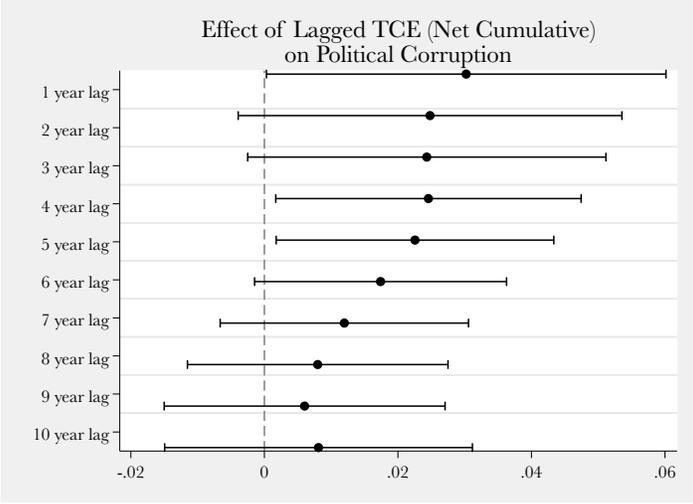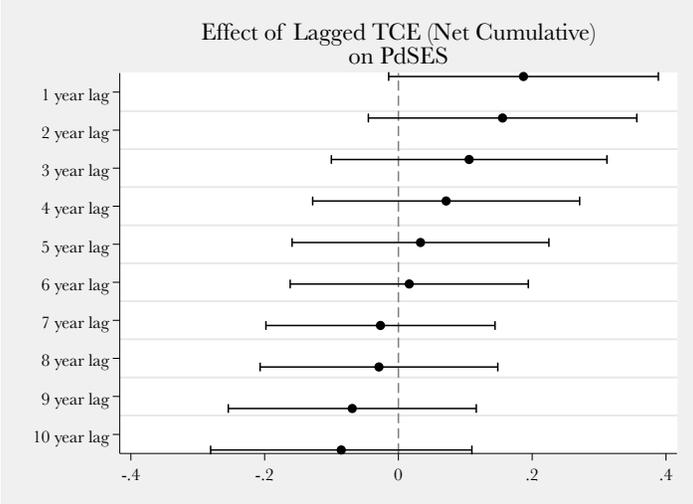

Figure 13: Diff-in-Diff regression coefficients for 10 lags, Net Cumulative TCE (PdSES)

by one year in all of the above analyses was reasonable.