

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 of 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

Countries that transitioned from authoritarian rule or that are recovering from civil conflict are confronted with the dilemma of whether to reckon—and how—with their violent past. Historical accounts show that victims of oppressive regimes—and citizens in general—have long demanded such reckoning. For example, after the US War of Independence, the Patriots organized ceremonies featuring puppets in cages as a way to reckon with collaborators who had fled to Britain. The puppets represented high-ranking defectors, like Benedict Arnold, and were paraded through the village and hanged in an act of revenge (Brumwell 2018). Similarly, in late 18th century Poland, Polish nationalists staged mock trials accusing three aristocrats of treason. The accused—who had conspired with Catherine the Great to divide the country among Prussia, Russia and the Hapsburg Empire—were physically absent from the trials, their portraits standing instead. After being found guilty and sentenced *in absentia*, they would be hanged in effigy (Walek 1988).

In the 20th century, these spectacles of symbolic accountability have been substituted, or even complemented, with additional transitional justice (TJ) measures that deal with authoritarian elites, their collaborators, or perpetrators of human rights violations (Bakiner 2016, Loyle & Appel 2017, Zvobgo 2019, Balcells, Palanza & Voytas 2020, Capoccia & Pop-Eleches 2020). The popularity of TJ, and the variety of institutions, practices, and policies that it encompasses, have raised some fundamental questions regarding the relationship between transitional justice and democracy. Classical scholars of democratization and regime transitions are skeptical about whether one can punish perpetrators of human rights abuses fairly, referring to certain forms of transitional justice as “witch hunts” (Holmes 1994) or “ritual sacrifices” (Cepl 1992). Others are also concerned about stability, arguing 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). Even Samuel Huntington insists that sometimes “amnesty [...] is necessary to establish a new democracy on a solid

basis” (Huntington 1993, 214).

These concerns, fundamental as they are, reveal that the classic literature has understood transitional justice primarily as a *punitive process*, overlooking the importance of the *disclosure* of egregious abuses and the names of those who committed them. After all, not all forms of authoritarian dominance and abuse are as transparent as repression (Tyson 2016), and in many cases, the very acts that sustained the authoritarian regime were secret collaboration (Blaydes 2010), cooptation (Magaloni 2006), and sabotage (Dragu & Przeworski 2019). In this way, transitional justice can deal with a violent past by punishing abusers but also by revealing the truth about the abuses committed.

Consider as an example the South African Truth and Reconciliation Commission (TRC) (Hayner 2010, Gibson 2006), formed in 1995 to investigate crimes against the South African people during the apartheid regime (1960-1994).¹ The TRC could investigate human rights violations committed by the state and various liberation movements, and it had the prerogative to offer amnesty to those who fully participated in the process and truthfully recounted their crimes, thus incentivizing perpetrators to come forward to share their story. In 1998, after touring the country for a couple of years, conducting hearings that were nationally broadcast on public radio, and collecting testimonies, the Commission released a five-volume final report. 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.” (Keesing’s Record of World Events 1998, 42536)

The TRC exemplifies how transitional justice can reveal the importance of truth revelation, particularly in two respects. First, the Commission held hearings for a number of perpetrators that were known offenders, but many of the specific instances of abuses that were revealed had been hidden from public disclosure and would have remained that way had it not been for the TRC. Recording these abuses was essential for reckoning with the

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

violence experienced during the Apartheid. Secondly, the Commission documented secrets that were particularly embarrassing and that could compromise former perpetrators of repression and/or their collaborators. As we argue in this paper, making this information public was crucial to avoid blackmail: absent the Commission's incentives to publicize abuses, people could threaten to denounce participation of others in egregious and violent acts unless they gave in to their economic or policy demands.

This example underscores the understudied, and underestimated, effects of truth commissions on the quality of democracy. As noted before, existing literature tends to focus on the punitive aspect of transitional justice; only recently have scholars begun to theorize and empirically illustrate the importance of disclosure and transparency for democracy. Horne & Levi (2004), for example, argues that by naming those who participated in human rights violations, transparency regimes can restore a sense of justice and increase victims' trust in democratic institutions. More recently, Ang & Nalepa (2019) argue that former dissident-turned-politicians can be blackmailed by former authoritarian elites, distorting political representation and worsening the quality of democracy. Lustration—another form of transitional justice that screens individuals interested in participating in political life—can render this threat null by disclosing embarrassing information.²

This paper contributes to the evolving literature on the effects of transparency regimes by focusing on truth commissions. We begin by arguing that truth commissions improve the quality of democracy in two ways. First, by exposing “skeletons in the closet,” truth commissions effectively eliminate the leverage that authoritarian networks have over elites in the democratic period. Second, truth commissions can dismantle authoritarian networks by weakening the clandestine ties that underpin such networks. Truth commissions, we argue, are important also relative to other transparency regimes, such as lus-

²We suggest, 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.

trations³ because they cast a wider net. Contrary to lustration, truth commissions extend beyond public figures and cover a greater range of crimes committed on behalf of the regime. As such, truth commissions should be exceptionally poised to deliver democracy-enhancing results.

Our contribution is also empirical. We use a panel dataset to leverage variation across countries and within countries across time to identify the causal effect of truth commissions on quality of democracy. This dataset enables us to record the advancement, or delay, of truth commissions through time. In doing so, our data acknowledges that truth commissions are the result of a politically contested process, and provides a more accurate metric of truth commissions. The dataset also allows us to estimate a modified difference-in-differences model, which helps us provide better evidence of the causal effect of truth commissions on the quality of democracy.

The paper begins by discussing our theoretical argument in detail, and from this discussion we develop a series of detailed predictions and theoretical expectations about the effects of truth commissions on democratic consolidation. In Section 3, we review existing research on identifying the causal relationship between transitional justice and dependent variables related to peace and democratic stability. We then present our own research design and explain how we operationalize the dependent and independent variables. Section 4 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 5 concludes.

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

³lustration is the process of disclosing and sometimes removing from public office proven secret collaborators of the authoritarian regime (Nalepa 2010)

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, it primarily falls into one of several categories, including: (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 specifically (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 perpetrators of atrocities. We argue that truth commissions, to the extent that they disclose atrocities committed in secret on behalf of the authoritarian regime, improve the quality of the newly inaugurated democratic regime.

⁴Truth 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).

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 transparency regimes, 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 agents 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 lack of transparency can distort representation even when only “innocent” candidates are running for office. 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 the possibility of a collaborator of the previous regime running for office can tarnish the reputation of other candidates, leading to the reelection of 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. This is because such information can be extremely dangerous. For example, even prior to the transition in South Africa, 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 In outing and removing former secret collaborators, covert authoritarian networks are less likely to survive the transition to democracy.

Without a transparency regime in place, former authoritarian elites can 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. By disclosing secret authoritarian legacies, truth commissions fulfill democratic goals by informing voters about candidates and allowing them to choose “honest” politicians. 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. Truth commissions also undermine authoritarian networks and render them obsolete.⁶

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 authoritarian

⁵In this context, one sees how having one’s name published in the TRC report constituted a considerable improvement over the horrific “doughnut death” as the burning tire sentence was called (?)

⁶Note 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.

regime who abused human rights publicly. Therefore, truth commissions will improve the quality of democracy in two directly observable ways. First, by revealing abuses committed by secret agents of repression truth commissions will undercut the blackmail mechanism described above and reduce political corruption. Second, by revealing this compromising information to voters about the true characters of persons who may potentially continue occupying public office, truth commissions will reduce the power of authoritarian networks.

3 Previous work identifying the effect of transitional justice

Our claim that truth commissions will improve the quality of representation requires empirical evidence beyond a simple correlational analysis. A positive relationship between truth commissions and measures of democratic quality alone is insufficient evidence for our theory because the same factors that cause states to embark on transitional justice may lead them to have better quality of representation later.

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, which is discussed in Ang & Nalepa (2019). Given the multitude of drivers of programmatic representation, any relationship established in a cross-sectional research design 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 & 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 political behavior. Concentrating on Chile, Balcells and her coauthors focus on detecting effects even among generations not directly affected by the former authoritarian regime. They use a field experiment with visitors randomly assigned to visit a museum devoted

⁷This 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).

to victims of military rule in Chile and an alternative museum completely unrelated to politics. The authors find that the museum visit compared to the has a significant effect on evaluations of the former regime, as well on political behavior more broadly.

While field experiments are not feasible for the assessment of the effects of personal transitional justice, the robustness of democratic institutions is also 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. 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 the importance of isolating the effects of transitional justice 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 (GTJD) Bates, Cinar & Nalepa (2020), which includes 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 NGOs and local authorities. Second, the event is classified as advancing the work of a truth commission (a positive event) or as inhibiting it (a negative event). 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 such a bill constitutes a negative event Bates, Cinar & Nalepa (2020). Moreover, we assign positive or negative status to events associated with the operation of truth commissions. For example, the circulation of a report by a truth commission is classified as a positive event, while the stepping down of a commission's chairperson prior to the term's expiration or the de-funding of a 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). Figure 1 presents a snapshot of the dataset as a trend of net (positive minus negative) truth commission events.

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 the measure of truth commission severity.⁸ In order to focus on the effects of truth commissions over time we modify slightly the measure of transitional justice severity by assigning to each year a dummy variable that is defined as a function of severity up to that year.

More specifically, we create three such alternative measures. The first, is a "minimalist"

⁸Severity is defined as 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)

measure:

$$TJ_{i,t}^1 = \begin{cases} 1 & \text{if } \sum_1^{t-1} P_{i,t} > 0; \\ 0 & \text{otherwise} \end{cases} \quad (1)$$

where P_{it} represents the number of positive truth commission events in country i in year $t - 1$ and N_{it} represents the number of negative truth commission 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 trajectory. The virtue of this measure is its simplicity, and we use it in all regressions below as a first model.

Our second measure reflects net events in country i in year $t - 1$:

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

Our third 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 $t - 1$):

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

All three 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 truth commission events over time. In doing so, our measure better leverages the time series aspect of the Global Transitional Justice Dataset.

Figure 2 illustrates how the third—net cumulative—measure captures truth commissions in the GTJD. Countries are stacked on the vertical dimension, while the horizontal

dimension represents years since the democratic transition. Dark blue cells illustrate years in which the cumulative net transitional justice events were positive ($\sum_1^{t-1} P_{it} - \sum_1^{t-1} N_{it} > 0$); light blue cells illustrate years in which the cumulative net transitional justice events were negative or equal to zero ($\sum_1^{t-1} P_{it} - \sum_1^{t-1} N_{it} \leq 0$); and white cells represent censored data stemming from the fact that the country in the corresponding row has not been democratic long enough.

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.

We use dichotomous severity measures because such indicator variables are easier to interpret in a diff-in-diff framework. Our regressions use one year lags of these measures, although figures 14 and 15 in Appendix A.5 summarize the coefficients obtained from running the regressions on ten different lags. We motivate the use of lags noting 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 our outcome, which in our case is a measure of quality of democracy.

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. Such comparisons are warranted as long as the “treated” and “control” countries are matched to ensure that their pre-treatment trends on the dependent variable are similar enough (satisfying the parallel trends assumption). 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.

For our dependent variables we use indicators of political corruption and measures of authoritarian turnover from the Varieties of Democracy (V-Dem) project. Both variables match closely the aspects of quality of democracy shaped by truth commissions according to our argument. V-Dem is a dataset based on interviews with approximately 5 independent experts per country. The experts’ subjective answers to questions on a host of regime characteristics over time are coded. These characteristics are not directly observable, hence experts’ subjective answers cannot be validated by relying on observational data. Yet 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 ordinal scales 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 we cannot directly observe blackmail with skeletons in the closet. What we can detect, however, are the consequences of blackmail: policy distortions that result from blackmail and these distortions can be observed 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*),⁹ public sector corruption (*v2x_pubcorr*),¹⁰ and judicial corruption (*v2x_jucorrdc*).¹¹ Originally, the index takes on values between 0 and 1, but we have transformed it to take values between -1 and 0 ensuring 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 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 voters.

⁹This 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?”

¹⁰This 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?”

¹¹This variable was created from answers to the question “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?”

Figure 3: Parallel Trends: Political Corruption Index

An assumption of the diff-in-diff framework, is that absent treatment, political corruption trends would develop in countries that were never treated with truth commission events according to a similar pattern as countries that were treated. This assumption is commonly referred to as the “parallel trends assumption.” In our case, since treatment can occur at any time following the transition to democracy, we need to be specific about defining treatment when demonstrating the parallel trends assumption.

Figure 3 compare the average political corruption index of countries that never received treatment with average values of the political corruption index of treated countries within 2, 5, and 10 years following their democratic transition. The control group is constant for all these interpretations of treatment and includes countries that were never treated, that is, never had more positive than negative truth commission events.

We first observe that countries treated with truth commissions are, in general, less politically corrupt than countries that were not treated. Second, the gap between the treated and untreated widens with time. More concretely, the gap between untreated countries and those treated by year 2 is .015, but about .05 by year 5 and .1 by year 10. The trends are mostly parallel with the exception of the beginning of the trend for the treatment by year 3, where they cross. At the same time, in treatment by year 5, there does not seem to be a big difference in the values of political corruption resulting from the treatment. Namely, there is no “break” in the trend line around year 5 in the top right figure and in the figure for treatment by year 10 in the bottom right, the break seems to occur earlier, around year 8. However, there does seem to be a small uptick in the treated trend relative to the untreated in the top left figure at the treatment by year 3 mark. Recall, this means that countries that first experienced more positive than negative truth commission events by year 3 seem to experience less political corruption relative to countries that never had more positive truth commission events than negative truth commission events.

It is worth keeping in mind while interpreting these figures 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 socio-economic position?” (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Ciobanu & Saxer 2017).¹² 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 markers 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.¹³ Power Distributed by Socioeconomic Status is also a particularly reasonable 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

¹²Answers 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).

¹³In 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 had the same voting power as the portion of the male population contributing the second third of the revenue and as the portion of the male population contributing the bottom third of the tax revenue, even though, obviously, these groups were numerically unequal (Hollenbach 2019).

Figure 4: Parallel Trends: PdSES

the implementation of transitional justice. If the goal of 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 using PdSES as the outcome variable. Truth commissions have been operationalized as a treatment in exactly the same way as in figure 3, so using net cumulative events.

Comparing the PdSES trends of countries treated and untreated with truth commissions reveals that for all but very early adopters, the trends before treatment are largely parallel; Regardless of how the treatment is defined (whether by year 2, 5 or 10) there does seem to be an 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.

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 .¹⁴ 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

¹⁴Note that there are one fewer dummies than there are countries.

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 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:

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

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.¹⁵

¹⁵In our case, these would be the cases treated within the first five years after the transition compared to those that were never treated.

Figure 5: Effect of Truth Commission Events (TCE) on Political Corruption

Truth Commission Events are lagged.

All models include country and year after transition intercepts, and standard errors clustered by country

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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).

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 (0.038)	.018 (.039)	.016 (.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 6, 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.

Figure 6: Diff-in-Diff regression of Political Corruption on Three Measures of Truth Commission Events (TCE) with Country Clustered SE

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 executive corruption (“v2xexecorr”).¹⁶

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. Re-

¹⁶Specifically, 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?”

Figure 7: Effect of Truth Commission Events (TCE) on PdSES

Truth Commission Events are lagged.

All models include country and year after transition intercepts, and standard errors clustered by country

Figure 8: Diff-in-Diff regression of Power Distributed by Socioeconomic Status on Three Measures of Truth Commission Events with Country Clustered SE

call 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.

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.

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 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.

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 (.274)	.297 (.280)	.285 (.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

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 and 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 others 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 our 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-

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

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

October2020Version/Data/SynthPdSES.pdf

cur.¹⁷ 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 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

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. overlooking the political work that the truth-revelation aspect of TJ can have. 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 au-

¹⁷Alternatively, 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.

thoritarian networks, particularly the networks 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).

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 three 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. Finally, in the third section, 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.

A.1 Table of Results

A.2 OLS with pooled data

We begin by presenting the table below, which shows the results of a pooled linear regression model. This table shows our estimates of the association between truth commission events (TCE) and the two outcomes of interest, including GDP logged as a covariate. The results below show a positive association between the minimalist measure of truth commission events and the net cumulative measure and political corruption and PdSES. When we use the measure of net TCE, however, the association is negative although statistically indistinguishable from 0. Of course, these results use a panel dataset without proper fixes to account for non-independence, and without controlling for selection into treatment. As such, these results are illustrative of a general trend but cannot be treated as conclusive.

Figure 11: Removing one country at a time

Figure 12: Removing one country at a time

A.3 Removing one country at a time

We also estimated the same models but removing one country at a time to make sure that our results are robust.

A.4 Decomposing the Political Corruption Index

Given that the political corruption index is composed by 4 sub-components—executive, public, judicial, and legislative corruption—we also present a similar set of estimations than in the paper (equation 4) but using each of these sub-components as outcomes of interest. The results are summarized in the following plot:

Figure 13: Coefficients of TCE on the Four Components of the Political Corruption Index on Truth

All models include country and year after transition intercepts, and standard errors are clustered by country

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.5 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 14 presents the results for political cor-

Figure 14: Diff-in-Diff regression coefficients for 10 lags, Net Cumulative TCE (political corruption)

Figure 15: Diff-in-Diff regression coefficients for 10 lags, Net Cumulative TCE (PdSES)

ruption and Figure 15 illustrates the coefficients for power distributed by socioeconomic status. As can be seen in both figures, our decision to lag our truth commission variable by one year in all of the above analyses was reasonable.