# Identifying the Causal Effect of Truth Revelation Procedures on the Quality of Democratic Representation

Milena Ang, Genevieve Bates, and Monika Nalepa
The University of Chicago

This version March 23, 2019

**Abstract**

Does transitional justice hinder or help democracy? This is a question hard to address methodologically because countries embarking on transitional justice may be the same ones that would have had a successful pathway to democratization. Hence the problem of identifying the causal relationship between transitional justice and quality of democracy. To resolve it, we leverage one of the problems associated with coding transitional justice events. In 2016 that Onur Bakiner criticized one of the leading assumptions in the transitional justice literature: that mechanisms for dealing with the past can be assigned a discrete implementation date. This assumption is unwarranted because scholars have difficulty assigning a fixed year of implementation. To take truth commission as an example, qualitative research associates two to four years for the operation of truth commissions (Bakiner 2016). We address the criticism by organizing data on all truth revelation procedures (truth commissions *and* lustrations) as a time series of events. Not only is this more accurate/faithful to the process on the ground, but it allows us to implement a diff-in-diff research design to identify the casual effect of truth revelation procedures to the quality of democracy. We use quality of democracy indicators from V-Dem and merge them with our Global Transitional Justice Dataset to find convincing evidence that truth revelation procedures and truth commissions in particular, decrease the level of political corruption and help decrease the influence of former authoritarian elites.

The authors are grateful to Onur Bakiner, Charles Crabtree, Justin Grimmer, Margaret Hanson, Nahomi Ichino, Viivi Jarvi, Holger Kern, Cyanne Loyle, Anne Meng, Susan Osterman, Jasmine Serra, Rachel Riedl, and Tesalia Rizzo for comments. Ipek Cinar, David Krosnin, Yonatan Litwin, Ji Xue, and Eddie Zhang provided stellar research assistance. All mistakes are the authors' responsibility.

# 1 Introduction

Does transitional justice have an effect on the quality of democratic representation 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 deal with authoritarian elites and their collaborators, or perpetrators of human rights violations and their victims (Vinjamuri & Snyder 2004, Pinto 2008, Bakiner 2016, Loyle & Appel 2017, Kaminski & Nalepa 2006). Transitional justice can take many forms,[1] but this paper focuses mainly on one aspect of what the literature has referred to as "truth revelation procedures," which include both truth commissions and lustration (Nalepa 2008).

Lustration, a somewhat obscure category, is also referred to as 'TJ vetting' or 'screening.' Lustration aims at uncovering politicians running for office who might have collaborated clandestinely with the secret police apparatus or other regime enforcement institutions. Based on the information unearthed by the process of lustration, proven collaborators may be barred from holding office (Bates, Cinar & Nalepa forthcoming). The most cited example (Kaminski & Nalepa 2006, Nalepa 2010*a*, Nalepa 2012, Letki 2002, Williams, Fowler & Szczerbiak 2005, Szczerbiak 2002) comes from the Polish 1997 lustration law, which requires all persons holding or running for public office to declare in advance whether or not they had collaborated with the secret police during the authoritarian period. Information from declarations admitting collaboration is put on the ballot, and voters themselves decide whether to cast their vote on a former collaborator. Negative declarations are sent to a special division of the Institute for National Remembrance, where they are verified against information assembled in the archives of the former secret political police. Proven collaborators who lied on their declarations are banned from running for office for 10 years. A more typical lustration law, however, carries with it an explicit sanction for anyone who is proven to have worked for the secret police as an informer (as in

[1] Broadly speaking, transitional justice policies are categorized as: (1) truth revelation procedures, (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).

Hungary) or who fails to provide evidence of his or her innocence (as in the Czech Republic). Like truth commissions, lustration is a truth revelation procedure, though lustration focuses on collaboration of political elites with the regime, whereas truth commissions deal with broad patterns of abuse among the general public.

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).[2] 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.

A classic example of a truth commission is the South African Truth and Reconciliation Commission (Hayner 2011, Gibson 2006). The Commission was formed in 1995 to investigate crimes committed against the South African people during the apartheid regime (1960-1994), covering human rights violations committed by both the state and various liberation movements.[3] The Commission's mandate provided it with the ability to offer amnesty to those who fully participated in the process and truthfully confessed the full extent of their crimes. It released a five-volume final report to then-President Nelson Mandela in 1998. The report detailed the abuses committed by the apartheid-era National Party government, the African National Congress (ANC)—the state opposition turned ruling

[2]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).

[3]Specifically, it was established via the Promotion of National Union and Reconciliation Act, passed by the South African parliament in July 1995 (United States Institute of Peace 2011).

party—and other "leading political figures on both sides of the anti-apartheid struggle" (Keesing's Record of World Events 1998).

Despite the pressing question of how to deal with former collaborators and members of the authoritarian regime or those involved in conflict, implementing transitional justice raises some crucial issues. Scholars of normative transitional justice are skeptical that transitional justice can help successful democratization without jeopardizing the rule of law. Some authors even single out lustration for undermining the principle of non-retroactivity: *nulla poena sine lege*, which translates from Latin to "no crime without a law" (Holmes 1994, Osiatynski 1994, Osiatynski 1992).[4] These skeptics have argued that lustration cannot possibly offer a legal foundation for new democratic states, because it violates principles of rule of law by discriminating against citizens who were acting legally. In the words of Halmai, Scheppele & McAdams (1997) "living well is the best revenge."

Most of these arguments castigating "personnel transitional justice" (David 2003), however, have focused on the immediate aftermath of the transition. Recent work Ang & Nalepa (2019*a*) 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 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*b*).

So what *are* the effects of truth revelation procedures on the quality of democracy? We suggest that truth revelation procedures should improve the quality of democracy in two ways. First, by removing "skeletons in the closet," these procedures remove the leverage that authoritarian networks have over elites in the democratic period. Second, by actively sanctioning "bad" politicians—those who participated in human rights violations or engaged in secret collaboration with the authoritarian regime—truth revelation procedures

[4]The work of Colleen Murphy (2017) is an important exception in this regard.

may restore to victims of the former autocracy or civil war ridden state a sense of justice and increase their trust in democratic institutions (Horne 2017).

The next section of this paper explores this argument in detail, leading to a more nuanced prediction about the effects of lustrations and truth commissions on the prospects for democratic consolidation. It concludes with a summary of theoretical expectations regarding the effects of truth revelation procedures on democratic outcomes. Section 3 reviews how existing research on transitional justice has sought to identify the relationship between transitional justice and dependent variables related to peace and democratic stability. We then present the research design used in this paper and discuss the operationalizations of the dependent and independent variables. Section 4 is devoted to data analysis and interpretation of the results. We find that truth commissions in particular are across all specifications robust in improving the quality of democracy.

## 2 The logic of personnel transitional justice

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. Revealing the truth about collaboration and the associated bans on holding public office imposed on secret collaborators can have a different effect than punishing former elites and perpetrators of human rights whose collaboration in the old regime or activity in the conflict was widely known. In the case of *unknown* collaborators, absent truth revelation procedures, politicians who worked for the authoritarian regime or committed atrocities on its behalf in secret can be blackmailed with the threat of revealing these actions by those with credible access to such "skeletons in the closet" (Ang & Nalepa 2019*a*, Ang & Nalepa 2019*b*). Needless to say, if the public still pays attention to what happened in the past, the revelation of such "skeletons" could end a politician's career. In return for their silence, individuals in possession of such credible evidence of "skeletons in the closet" can demand rents or policy concessions. Regardless of the currency in which the ransom is paid out by the blackmailed politician, the quality of democracy suffers.

Under the right conditions, however, truth revelation procedures can address these concerns. By making known the previously unknown behavior of elites, truth revelation procedures remove the opportunity for blackmail, while also either (1) providing specific sanctions against wrongdoers, or (2) providing the public with the opportunity to sanction wrongdoers indirectly. By removing opportunities for blackmail and, at times, by removing "dishonest" politicians from office, we expect that truth revelation procedures should increase the quality of democracy. While all truth revelation should have this effect, we believe it is reasonable to expect that truth commissions will be more effective because of their ability to cast a wider net and expose more secrets. Although it seems that lustration laws ought to be more effective because of their focus on political elites, they face more obstacles precisely because they are politically threatening to parties. While it is true that truth commissions, by making public past wrong-doing by elites, leave it up to voters to sanction officials, once the secret is out, whoever could have been blackmailed with the threat of revealing it is now immune to such threats and may safely go about representing voters. Hence, we conjecture that the sanction itself is not an essential element of truth revelation procedures to work properly.

# 3 Research Design

The question this article seeks to answer is "What is the effect of truth revelation procedures on the quality of democratic representation?" The straightforward nature of the question is misleading, however, because a relationship like this is notoriously difficult to identify. In other words, establishing whether or not transitional justice improves the quality of representation 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 and programmatic representation as measured by expert surveys such as the Democratic Accountability and Linkages Project, as in (Ang & Nalepa 2019*a*). 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 TJ institutions in regime transitions 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[5] decrease the likelihood of conflict recurrence. As an additional robustness check to identify the determinants of post-conflict justice implementation, Loyle & Appel (2017) also employ a strategy of matching on observables. The results are consistent with their instrumental variable analysis.

Alyssa Prorok (2017) also uses an instrumental variable approach, proposing three plausible instruments to estimate the causal effects of International Criminal Court (ICC) investigations on conflict duration. Using a civil war state's affinity with the permanent five members of the UN Security Council, a civil war state's affinity with neighboring

[5]What these authors refer to as "motivation post-conflict justice."

states, and the number of neighboring states that have ratified the Rome Statute as instruments, 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 impact, however, decreases as the number of atrocities committed by one party to a conflict increases relative to other parties.

Loyle & Appel (2017) and Prorok (2017) have made important inroads 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 mechanisms affect the long term quality of democracy.

The robustness of democratic institutions, however, is one of the questions motivating Capoccia & Pop-Eleches (n.d.). 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 the prospects of democratic consolidation in post-WWII 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 democratization prospects. 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 (n.d.) 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 the broader effect transitional justice mechanisms may have for countries different than postwar Germany in important respects. Nor can this analysis tell us much about the effect of lustration and truth commissions, because although they were present in the post-WWII time period, Capoccia & Pop-Eleches (n.d.) do not disaggregate vetting into purges and lustrations to allow for the uncovering of such a relationship.

Our research builds on these initial approaches, recognizing that understanding the effects of truth revelation procedures requires isolating the effect of these 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. We thus attempt to identify the relationship between the two transitional justice mechanisms described above and the quality of democratic representation with a difference-in-difference design, which leverages the power of panel data to better understand the mechanisms by which transitional justice produces desirable democratic outcomes.

The classic explanation of this method relies on comparisons of time trends in countries that have been treated with the independent variable of interest—in our case a truth revelation procedures—and those that have not received treatment. As long as the "treated" and "control" countries have been matched in a way 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 the 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.

## 3.1 Measuring Truth Revelation as a Time Series

The dataset by Bates et al. (forthcoming) separates dealing with known collaborators of the former regime or perpetrators of human rights violations, such as purges from dealing with unknown collaborators of the former regime (lustration) or perpetrators of human rights violations (truth commissions). The dataset is constructed as an annual panel that records progressive and regressive TJ events. A progressive event is defined as the submission of a TJ proposal to the floor of the legislature, the passage of such legislation, the upholding of such legislation as constitutional by a supreme court, or the overturning of a presidential veto against such legislation. In the case of truth commissions, the publication of the commission's report(s) and the extension of the commission's mandate are also considered to be progressive TJ events. A regressive transitional justice event, in contrast, is defined as the voting down, vetoing, or striking down by the constitutional court of a transitional justice proposal or law. Similarly, expanding the set of persons targeted by TJ or broadening the set of "offenses" (where "offense" is defined in light of the TJ procedure in question) to include more past or present positions constitutes a progressive transitional justice event, whereas attempts to narrow the set of targets or "offenses" are coded as regressive TJ events. The guiding principle in determining if an event is regressive or progressive is whether it advances the TJ process forward or shifts it backward. The Global Transitional Justice Dataset panel covers 84 countries as the cross section and time since transition as the temporal dimension. Figure 12, included in the Empirical Appendix, shows such disaggregated event data on truth revelation procedures from the Global Transitional Justice Dataset. Following Bates et al. (forthcoming) we present it as a time trend (right) as well as a trend according to years lapsed since transition (left).

In their paper introducing the dataset, Bates et al. (forthcoming) argue that the time series structure of their data allows for the creation of many different measures of personnel transitional justice. The three measures they propose, however—urgency/delay, severity, and volatility—all collapse the time trends that make the Global Transitional Justice Events dataset so novel. This includes their measure of TJ severity, which is defined as the total number of progressive events over the total number of events.

Severity of specific TJ mechanisms is particularly relevant for testing our theory, which predicts that more severe forms of truth revelation procedures should increase the values of democracy indicators. Thus, we modify the Bates et al. measure of severity in a way that allows it to vary over time. What we sacrifice in turn is the continuous aspect of the measure, as to express it as a "treatment" we have to collapse the range of this measure into an indicator variable. Hence, in the regressions below, we will be using:

1. A minimalist measure, which codes the first progressive transitional justice event as $1$ and assigns all the country-years that follow a $1$, regardless of the subsequent transitional justice trajectory:

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

2. A measure that assigns 1 if cumulative net events up to a given year are positive and 0 otherwise:

$$TJ^2_{i,t} = \begin{cases} 1 & \text{if } \sum_1^{t}(P_{i,t} - N_{i,t}) > 0; \\ 0 & \text{if } \sum_1^{t}(P_{i,t} - N_{i,t}) \leq 0; \end{cases} \tag{2}$$

3. A measure that assigns 1 if net events in a given year are positive and 0 otherwise:

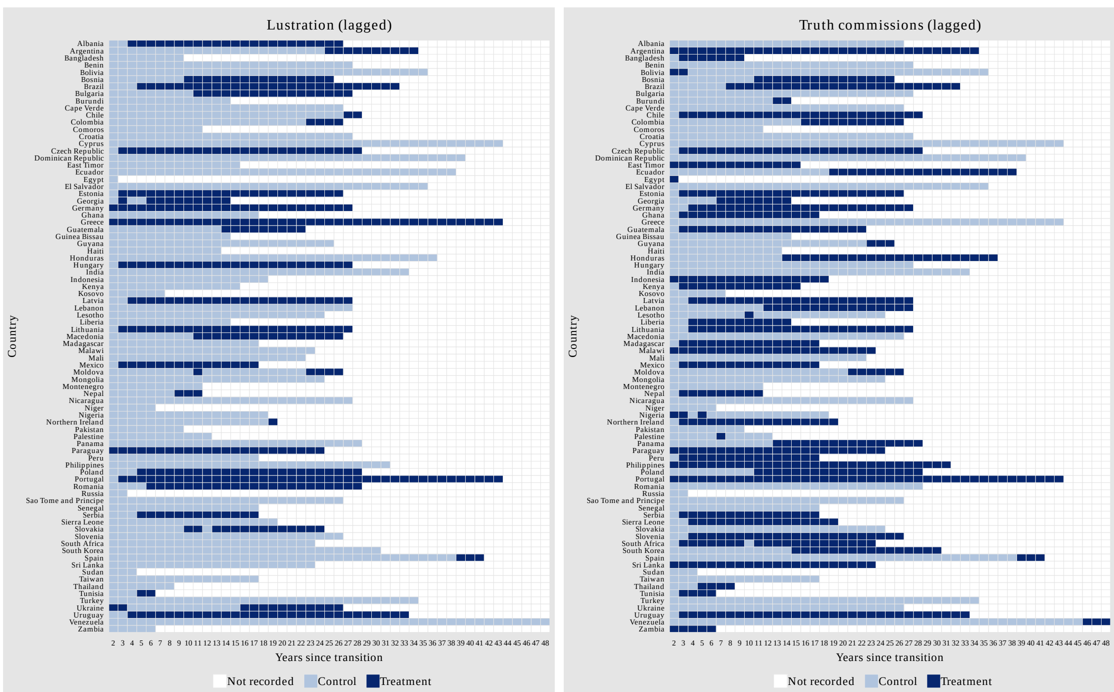
$$TJ^3_{i,t} = \begin{cases} 1 & \text{if } P_{i,t} - N_{i,t} > 0; \\ 0 & \text{if } P_{i,t} - N_{i,t} \leq 0; \end{cases} \tag{3}$$

These measures are suitable for use in a diff-in-diff research design because they are dichotomous. Beyond the first, minimalist measure, measure 2 identifies the number of progressive events net of the number of regressive events that have occurred in country $i$ up to period $t$. Since this measure assigns a new value to each post-authoritarian or post-conflict year a democracy has experienced, it is structured as a country trend. It assumes 1 when there are more cumulative progressive TJ events in a given year than cumulative regressive events and 0 otherwise.

Measure 3 modifies the original measure by identifying the number of progressive events net of the number of regressive events that have occurred in country $i$ in period $t$. It assumes 1 when there are more progressive TJ events than regressive TJ events in a given year and 0 otherwise. Since it assigns a new value to each post-authoritarian or post-conflict year a democracy has experienced, it is also structured as a country trend.

Figure 1, below, shows the trajectories of different lustrations and truth commissions measured according to measure 2 in each of the 84 countries included in the dataset. It presents the countries as rows and time since transition on the horizontal dimension. The light blue cells represent years coded as control (for example, $TJ_{i,t} = 0$ for the left panel), and the dark blue cells depict years that are coded as treated ($TJ_{i,t} = 1$). Empty cells represent years that we have yet not observed.[6]

Figure 1: Countries and truth revelation treatments




Note: Transitional justice treatment takes the value of 1 if cumulative net transitional events up to a given year are positive, and 0 otherwise.

Figure 1 is useful because it illustrates the different trajectories of truth commissions and lustrations according to measure 2. It shows first that not all truth revelation policies were implemented at the same time, nor across the same countries. It is useful to contrast this figure with the representation of net trends in lustration and truth commissions in Figure 12 in the empirical appendix. That figure, recall, used net TJ events as opposed to

[6]Note that because countries transition to democracy at different times, this is an unbalanced panel.

an indicator variable, yet it also shows that the timing of lustrations and truth commissions differs.

## 3.2 Measuring Quality of Democracy

Over the last decade, scholars have created transitional justice datasets that span transitional justice implementation across the globe and over time (Olsen, Payne & Reiter 2010, Binningsbø, Loyle, Gates & Elster 2012, Van der Merwe, Baxter & Chapman 2009, Thoms, Ron & Paris 2010, Loyle & Binningsbø 2018). The dependent variables these scholars have looked at range from trust in governmental institutions (Horne 2017) to peace (Binningsbø et al. 2012) to democratic stability (Olsen et al. 2010). Yet none of these variables capture the degree to which politicians can implement policies that the voters desire as opposed to the policies favored by former autocrats and perpetrators of human rights abuses. Hence, it is fair to say that the scholarship on transitional justice to date has found no evidence that these mechanisms make a difference in preventing former autocrats from reasserting their political dominance.

Finding a dependent variable for a classical observational study of the relationship between transitional justice and the quality of representation 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. As noted above, we address this problem by implementing the difference-in-difference research design. The cost of implementing it is that both the dependent and independent variables of interest must be measured over time.

Fortunately, we can use existing data from the Varieties of Democracy (V-Dem) project to measure the quality of representation. 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 for measuring the quality of representation and the extent to which authoritarian elites reproduce, we settled on using two more specific expert evaluations that tap into the concept of quality of representation. Among V-Dem variables that stand out as particularly useful for this purpose are the Political Corruption Index (*v2x_corr*), and Power Distributed by Socioeconomic Status (*v2pepwrses*), which we discuss in turn below.

### 3.2.1 Political Corruption Index

Corruption is a phenomenon which is notoriously hard to measure (Apaza 2009) and some of the datasets which have been created do not span the time frame that is required of our dataset (our trend variable has to start as early as 1958, when the first transition to democracy in the Bates, Cinar & Nalepa dataset took place). This index is created out of six different questions regarding corruption in three different branches of government—

the legislature, judiciary and executive.[7] Its advantage is that within the executive it treats separately bribery and embezzlement and also that it distinguishes between corruption taking place in high levels of government ("grand corruption") and lower in the public sector (corresponding to "petty" corruption) (Coppedge et al. 2018). The index 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. Granted, the index question does not get directly at the the extent to which politicians are influenced by blackmail with secret police files, but it does measure how much they succumb to pressures that impede their ability to represent. The left panel of Figure 2 shows the trajectories of the public corruption index for countries represented in our dataset, with one country (Argentina) highlighted for illustration purposes.

#### 3.2.2 Political Power Distributed by Socioeconomic Status

While the political corruption index assesses the extent to which politicians are susceptible to external pressures in the form of blackmail, the second dependent variable we use taps directly into the ability of authoritarian elites to survive the transition. 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 in a clientelistic fashion to stay 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

[7] Specifically, it is made up of questions coded as variables *v2x_pubcorr*, *v2x_execorr*, *v2lgcrrpt*, *v2jucorrdc*, *v2excrptps*, and *v2exthftps*. For example, the question about executive corruption is"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?" (Coppedge, Gerring, Lindberg, Skaaning & Teorell 2018).

Figure 2: Outcomes of interest through time
Argentina highlighted for illustration

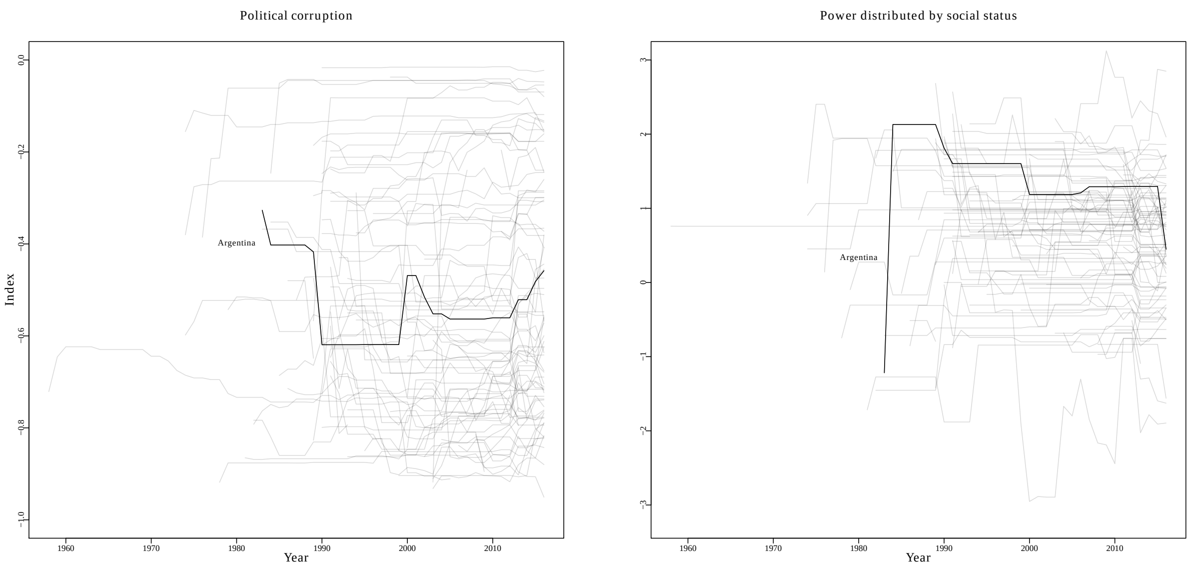



Figure 3: *Note*: In the case of the political corruption index, for ease of interpretation, we transformed the index so that it takes on values between -1 and 0, where lower values are worse for democracy.

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. Effective personnel 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 democratic representation. 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.

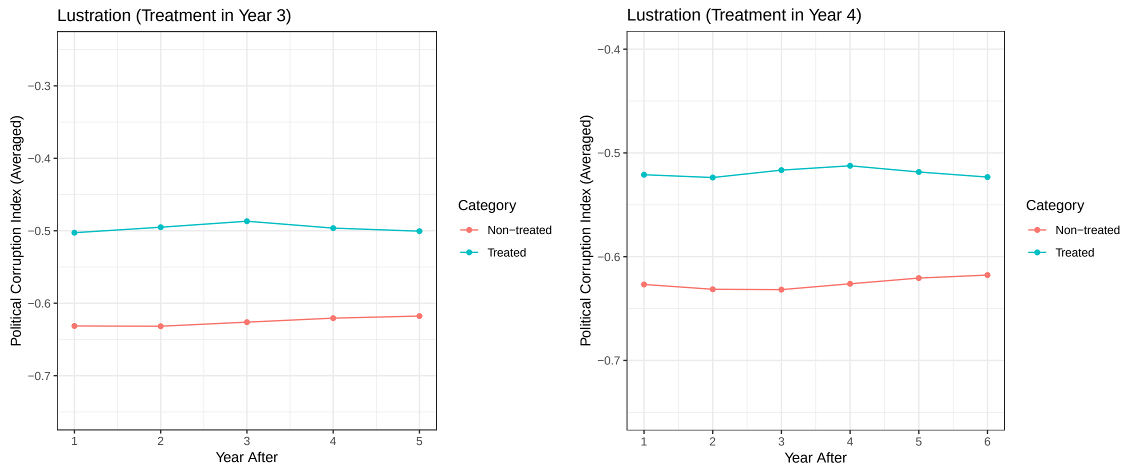
Called "Political Power Distributed by Socio-economic Status" (*v2pepwrses*), the variable is based on the following question posed to V-Dem experts: "is political power distributed according to socioeconomic position?"(Coppedge, Gerring, Lindberg, Skaaning, Teorell, Ciobanu & Saxer 2017).[8] In his 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).[9] 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 show this measure in the right-side panel of Figure 2, with Argentina highlighted for illustration purposes.

# 4 Difference in Difference

Recall from our discussion in Section 2 that we expect truth revelation procedures—that is, truth commissions and lustration—to increase the quality of democracy. We expect truth commissions to have the strongest effect on improving democratic indicators because they cast a wider net in revealing secrets of the former authoritarian regime than lustrations do. Truth commissions are both forward-looking in focus *and* extend to all citizens. They do not exclude everyone but the elite. Thus, we suggest that truth commissions may be more

[8] 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).

[9] 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 the implementation of transitional justice.




(a) treated after 2 years or more

(b) treated after 3 years or more

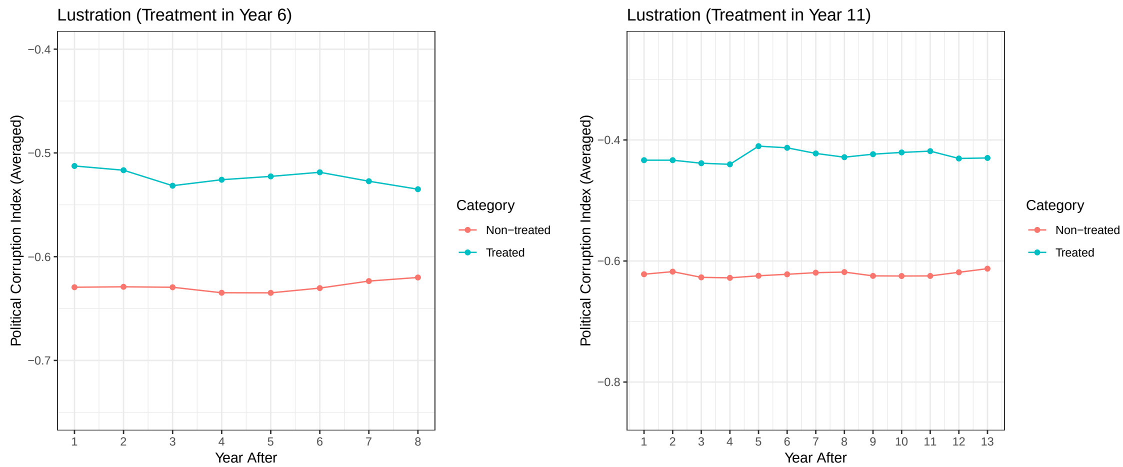
Figure 4: Parallel trends for political corruption index, treatment: lustration as positive net cumulative events (early adopters)

likely to bring to light the past misdeeds of political elites. Conversely, we expect the effect of lustrations to be more moderate.

One of the key assumptions that has to be satisfied before using a diff-in-diff research design is the assumption that had the treated group not been treated, the outcome variable would have followed the same course over time as the control group. This is known as the "parallel trends assumption" and its satisfaction is best illustrated graphically. The first series of figures below compares the average values of our first dependent variable—the political corruption index—in all countries that were not "treated" with lustration to all countries that received treatment in the $3^{rd}$ (top left), $4^{th}$ (top right) $6^{th}$ (bottom left) and $11^{th}$ (bottom right) year after the transition or later. The trends define treatment according to measure 2, that is, as an indicator for whether the net cumulative events were positive:

$$TJ^2_{i,t} = \begin{cases} 1 & \text{if} \sum_1^t (P_{i,t} - N_{i,t}) > 0; \\ 0 & \text{if} \sum_1^t (P_{i,t} - N_{i,t}) \leq 0; \end{cases}$$

As is clearly visible, countries treated with lustration do have less corruption on average than countries that are not treated in the same time frame. This observation alone is a good argument for trying to causally identify the effect of truth revelation procedures on




(a) treated after 5 years or more

(b) treated after 10 years or more

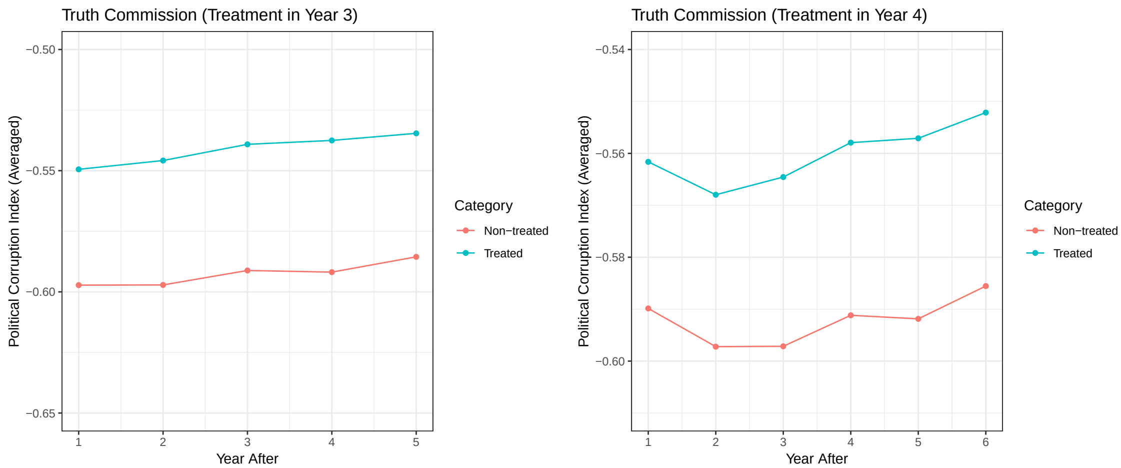
Figure 5: Parallel trends for political corruption index, treatment: lustration as positive net cumulative events (late adopters)

the quality of democracy. In terms of the parallel nature of the trends, there is no obvious way in which the trends in the treated countries diverge from the trends in the untreated countries prior to the treatment. In the case of lustration, there is also no obvious way in which they diverge after treatment either, although those effect might be visible later than just a few years out. Figures 5 and 6 show parallel trends for truth commissions.

Here, the gap in political corruption between the treated and not treated is considerably smaller: .05 between untreated and those treated in year 3 or later and it drops to even less following years 4, 5, and 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. Similarly to lustration, there does not seem to be a big difference resulting from the treatment. But the decision to use a diff-in-diff framework while advisable for lustration[10] is not so necessary to use here.

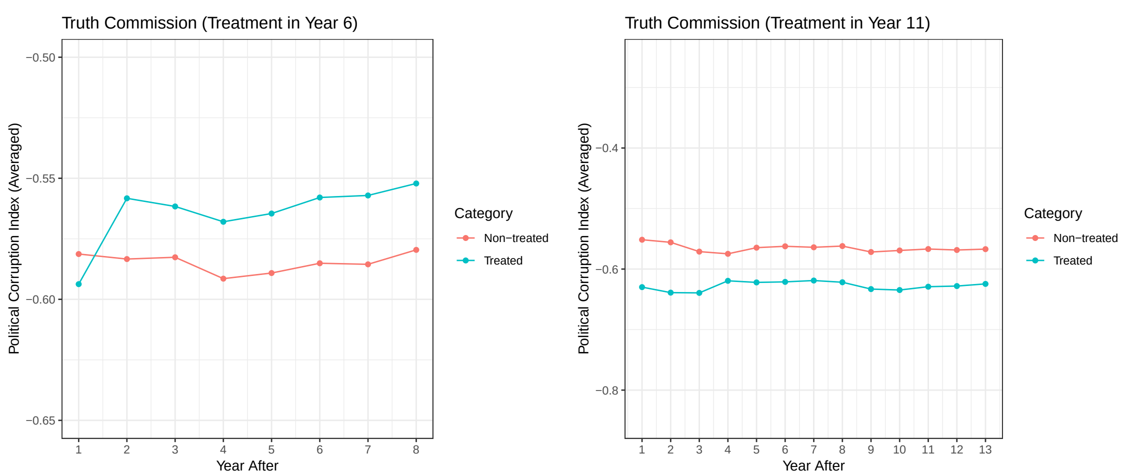
We now turn to discussing some adjustments that have to be made to the traditional diff-in-diff framework for it to work with our unbalanced panel.

[10]Note that parallel trends presented in figures 3 and 4 are consistent with the story "countries with less political corruption tend to implement lustration, but lustration does not decreases on its own political corruption." The trends in figures 5 and 6 less so.




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

Figure 6: Parallel trends for political corruption index, treatment: truth commissions as positive net cumulative events (early adopters)




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

Figure 7: Parallel trends for political corruption index, treatment: truth commissions as positive net cumulative events (late adopters)

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

$$Y_{c,t} = \mu_c M_C + \lambda_t T + \gamma D_{c,t} + \beta X_{c,t} + \epsilon_i$$

where $D_c = 1$ if country $c$ experienced transitional justice of a given type and is $0$ otherwise. $M_c$ is the country dummy, which assumes $1$ when the TJ event is associated with country $c$[11]; $T$ is the time period dummy, meaning it is equal to 0 if $Y_{c,t}$ is measured at time $t = 0$ and $1$ if it is taken at time $t = 1$. In the classical 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, $\mu_c$ can be interpreted as the country intercept and $\lambda_t$ as the the period intercept in the model. $X_{c,t}$ represents the set of covariates, which the treatment effect is conditioned on. Note that a consequence of the above notation is that $D_{c,t} = M_c * T$.

There are two important adjustments one has to make in the model to reflect the panel structure of the data. First, each country got the treatment at a different time $t$ (that is, each country has a different year that marks the pre and post-treatment period). Second, we have multiple time periods recorded for each country. To account for this and correctly specify the DiD model with multiple time periods, we build on Angrist & Pischke (2008) and Besley & Burgess (2004)[12] and propose to estimate the model in the following form:

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

Note, that this is essentially, a two-way fixed effect regression[13] where $\mu_i$ will again give us country-specific fixed effects and $\lambda_t$ will provide "year-since-transition" specific fixed effects.[14] $X_{i,t}$ still represents the set of covariates, which the treatment effect is conditioned

[11]Note that there are as many dummies as countries -1.

[12]Also cited in stackexchange (http://tinyurl.com/y8cf6f8b). We also rely on notes by Oscar Torres Reyna (http://tinyurl.com/ybc4lm6v).

[13]We are grateful to Naoki Egami for pointing this out to us.

[14]Since transition from democracy took place in different calendar years for different countries, we use

on. In our case, we use a single covariate—GDP per capita. 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.

In order to use the difference-in-difference framework, we assume that absent treatment, the political corruption trends would develop according to a similar pattern as in the countries that were never treated with transitional justice. Yet there are many factors that effect political corruption trends aside from transitional justice, and key among them is economic wealth. Hence, we control in all our regressions for GDP per capita.

# 5 Results and Discussion

We begin by estimating the effect of the two truth revelation mechanisms—lustration and truth commissions—on the political corruption index. Next we estimate the effect of these two mechanisms on power distributed by socioeconomic status.

## 5.1 Political Corruption

The models presented Table 1 capture the effects of each truth revelation mechanism having at least one progressive event. Recall, that following our transformation of the data, higher values of the Political Corruption Index represent less corruption and hence a higher quality of democratic representation. It is important to keep in mind how to interpret these results. In models 1 and 2, the treatment represents having experienced at least one progressive truth revelation event of a given type (lustration in model 1 and truth commission in 2). Thus, the control group is defined by countries who never had an event of *that type*, though they could have experienced an event of the other type or any other type, for that matter. Model 3, on the other other hand includes both types of truth revelation procedures. Hence the control group here are countries that had no truth revelation

years since transition to label periods instead of the objective time period.

Table 1: Political corruption index and occurrence of truth revelation procedures

| | Political Corruption Index | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Lustration Occured | −0.012 | | −0.018* |
| | (0.008) | | (0.008) |
| Truth Commission Occured | | 0.028*** | 0.031*** |
| | | (0.006) | (0.006) |
| GDP per capita | 0.018 | 0.018 | 0.020 |
| | (0.011) | (0.011) | (0.011) |
| Observations | 1,740 | 1,740 | 1,740 |
| $R^2$ | 0.062 | 0.073 | 0.076 |
| Adjusted $R^2$ | −0.010 | 0.001 | 0.004 |
| *Note:* | *p<0.05; **p<0.01; ***p<0.001 | | |
| | *Lagged TRP (at least one event occurred), country + year fixed effects* | | |

procedures whatsoever.

Another thing to keep in mind is that the minimalist measure is very simple, perhaps too much so. An event could be merely proposing a bill in the legislature, even if this proposal goes absolutely nowhere. If one country accumulated considerably more progressive truth revelation events than another, this measure is not able to pick up this difference.

Keeping these caveats in mind, we see that lustration severity measured in this way has a significant negative effect on reducing corruption relative to countries that did not engage in any lustration events. Truth commissions, on the other hand have a significant and positive effect on reducing corruption relative to countries that had no progressive truth commission event whatsoever. From Model 3, we see that effect is almost twice as strong as the negative effect of having at least one lustration event. It is very plausible that because truth commissions are non-exclusionary in nature compared to lustration, they have the ability to reveal more secrets than lustration alone.

These findings are partially consistent with our expectations, in that we expected truth commissions to improve the quality of democratic representation. Of course, in this model we are not in a position to control for the possible modifiers of lustration effects discussed in (Ang & Nalepa 2019*a*) such as the free media and the costs of revealing skeletons. It is also possible that one event is simply too little to decrease perceptions of corruption. In order to remedy this, we turn to our second measure, assigning 1 to the country years in which cumulative progressive events exceeded regressive event of a given type. These are reported in Table 2 below.

Table 2: Political corruption index and cumulative truth revelation procedures

| | Political Corruption Index | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Lustration Net Cum Pos. | $-0.013^{*}$ | | $-0.013$ | | $-0.014$ |
| | (0.007) | | (0.011) | | (0.011) |
| Truth Commission Net Cum Pos. | | $0.025^{***}$ | | $-0.018$ | $-0.016$ |
| | | (0.006) | | (0.022) | (0.022) |
| Lustration Occured | | | $-0.001$ | | $-0.005$ |
| | | | (0.013) | | (0.013) |
| Truth Commission Occured | | | | $0.046^{*}$ | $0.046^{*}$ |
| | | | | (0.023) | (0.023) |
| GDP per capita | 0.019 | 0.018 | 0.019 | 0.018 | $0.021^{*}$ |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) |
| Observations | 1,740 | 1,740 | 1,740 | 1,740 | 1,740 |
| $R^2$ | 0.063 | 0.071 | 0.063 | 0.073 | 0.077 |
| Adjusted $R^2$ | $-0.009$ | $-0.001$ | $-0.010$ | 0.001 | 0.004 |
| *Note:* | $^{*}$p<0.05; $^{**}$p<0.01; $^{***}$p<0.001 | | | | |
| | *Occurred and Net Cum Positive TRP, country + year fixed effects* | | | | |

Note again the control groups in these models. In the first and second models, the control groups are countries that have never experienced a positive event related to the truth revelation procedure of a given type, but also those that had more progressive events than regressive ones, but where the trend switched. In these models, it appears that as before, lustration fails to decrease corruption, but truth commissions succeed at it. However, one may feel uneasy about lumping into one control group countries that experienced no transitional justice events with countries that had them, but had just more regressive events or with countries that used to have more progressive than regressive events but then the trend reversed. For this reason, Table 2 also presents, in models 3-5, regressions with the minimalist measure included. This allows us to evaluate the effects of truth revelation procedures for a more carefully disaggregated control group.

First, it is clear that the positive effect of truth commissions on reducing corruption persists: relative to countries that had at least one progressive lustration event, having more cumulative progressive events than negative events decreases corruption by almost three percentage points (.046-.018). Relative to countries that had no progressive events at all, it decreases corruption by almost 5 percentage points. These effects persist in magnitude even after taking lustration into account. Moreover, from models 1 and 3 we see that once lustration events are counted more carefully than with just the presence of at least once progressive event they stop mattering in a significant way and their relevance is dwarfed by truth commissions.

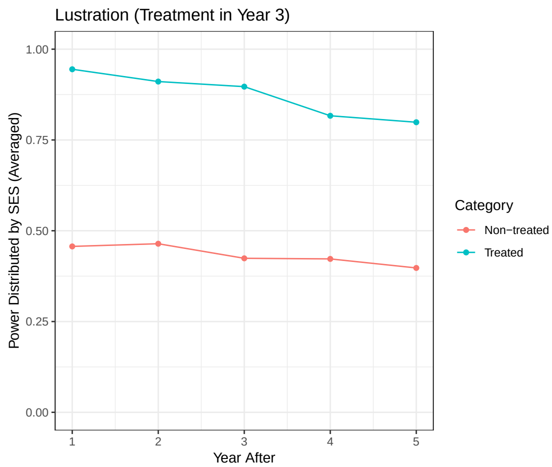
At some level, this measure could also be criticized for "double-counting" time. Since diff-in-diffs explicitly model time, accounting for cumulative lustration and truth commissions may seem redundant. For this reason, as a robustness check, we also include models with the simple measure of net events in each year (measure 3). These results are presented in Table 3 below.

The first thing to note is that lustration is no longer significant anywhere regardless of how the control group is put together. Second, truth commissions do overall decrease the level of political corruption relative to countries that did not have a single progressive event by 2.5 percentage points and relative to countries that had at least a positive event,

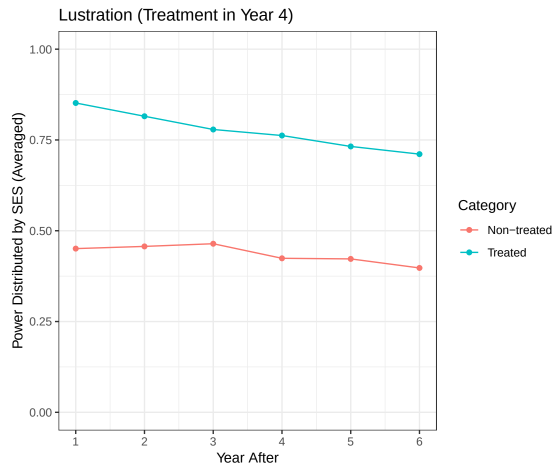
Table 3: Political corruption index net TRP

| | Political Corruption Index | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Lustration Net | −0.010 | | −0.008 | | −0.008 |
| | (0.006) | | (0.006) | | (0.006) |
| Truth Commissions Net | | 0.012* | | 0.005 | 0.005 |
| | | (0.005) | | (0.006) | (0.006) |
| Lustration Occurred | | | −0.010 | | −0.016* |
| | | | (0.008) | | (0.008) |
| Truth Commissions Occurred | | | | 0.026*** | 0.029*** |
| | | | | (0.006) | (0.007) |
| GDP per capita (logged) | 0.016 | 0.017 | 0.017 | 0.018 | 0.019 |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) |
| Observations | 1,740 | 1,740 | 1,740 | 1,740 | 1,740 |
| $R^2$ | 0.062 | 0.064 | 0.063 | 0.073 | 0.077 |
| Adjusted $R^2$ | −0.010 | −0.008 | −0.010 | 0.001 | 0.004 |

*Note:* *p<0.05; **p<0.01; ***p<0.001
*TRP occurred and net events (country and year fixed effects)*




(a) treated after 2 years or more




(b) treated after 3 years or more

Figure 8: Parallel trends for Power distributed according to SES, treatment: lustration as positive net cumulative events (early adopters)

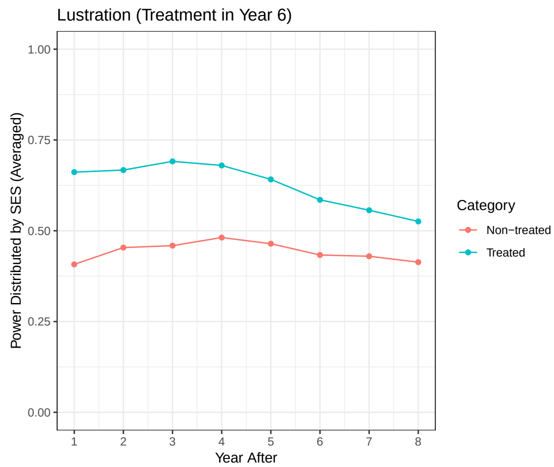
by 2%. Relative to countries that had at least one progressive lustration event, it decreases corruption by 4 percentage points.

The next subsection parts with looking at corruption altogether and presents results from similar regressions as the ones above, but using the association between economic and political status as the dependent variable.

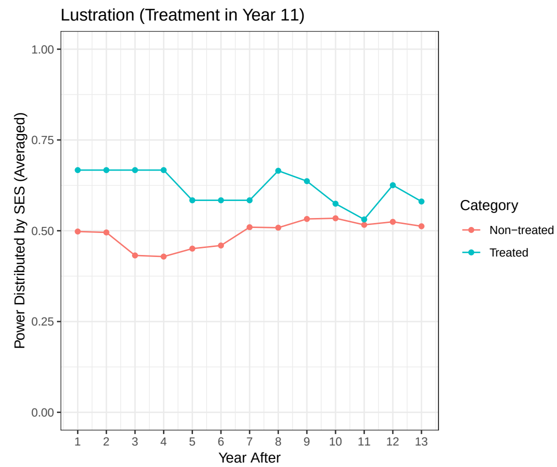
## 5.2 Power Distributed by Socio-economic Status

Here we use data on truth revelation procedures to predict Power Distributed by Socio-Economic Status, which was described in detail in Section 3.2.2, above. First, consider the figures below illustrating whether the parallel trends assumption is satisfied.

The trends in power distributed by socio-economic status for lustration, similarly to the ones for political corruption indicate that the quality of democracy according to this measure is higher for the treated countries than the untreated, particularly for the comparison with early adopters in Figure 7 (the difference is around .3 on a four point scale). With the exception of the very late adopters, the trends seem to be quite parallel to each other. The figure for the latest adopter (the right hand side of Figure 7) suggests that lustrations




(a) treated after 5 years or more



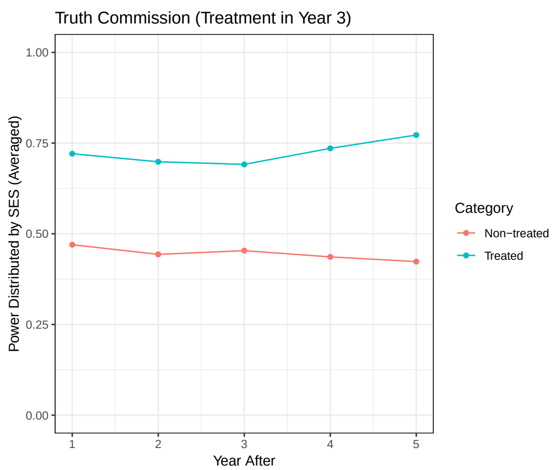

(b) treated after 10 years or more

Figure 9: Parallel trends for Power distributed according to SES, treatment: lustration as positive net cumulative events (late adopters)

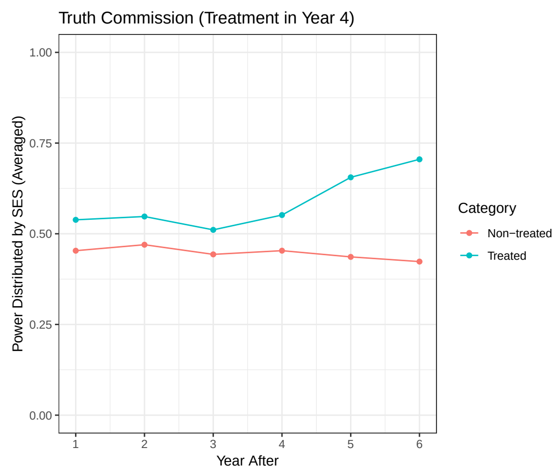
are adopted at the time of a downswing in Power Distributed by Socioeconomic Status, however this is just driven by a couple of observations. At the same time, in neither of the panels is there a clearly visible uptick or downtick following treatment.

Moving on to the illustrations for truth commissions we see a very different picture emerge. First, note that for all but very late adopters, the trends before treatment are parallel and there is a very clear uptick in quality of democracy according to the Power Distributed by Socioeconomic Status measure following treatment. Thus, there is a clear indication, even from eye-balling the parallel trends that the diff-in-diff framework will show that truth commissions help eliminate vestiges of authoritarian rule, to the extent that they are captured by Power Distributed by Socioeconomic Status.

Again, models (1) and (2) of Table 4, below, use a single progressive truth revelation event as the treatment, meaning that the comparison group is defined by having no truth revelation events of that type at all. Here the relationship between thus defined lustration and the quality of democracy is significantly negative, while the effect of truth revelation procedures is significantly positive. When the comparison group changes to countries that had at least one progressive lustration event, the effect of truth commissions is also posi-

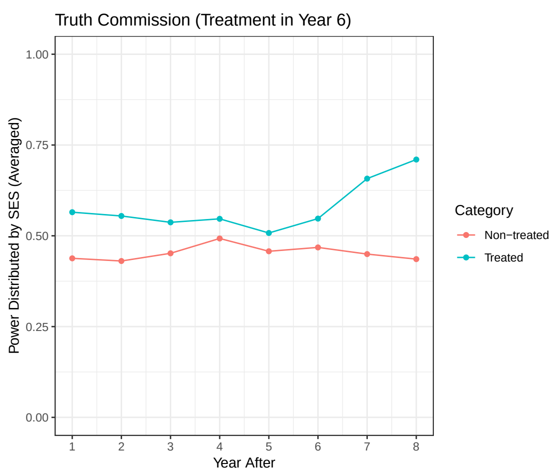



(a) treated after 2 years or more

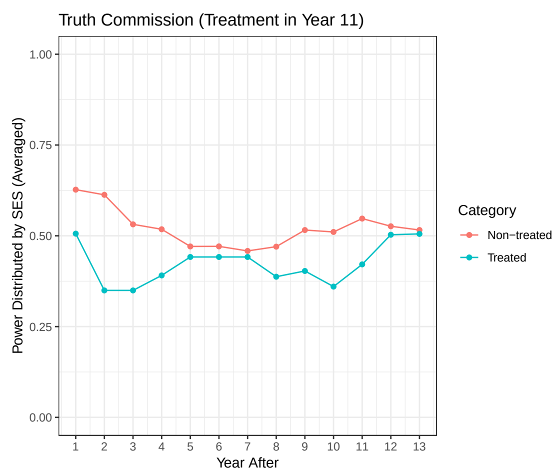



(b) treated after 3 years or more

Figure 10: Parallel trends for Power distributed according to SES, treatment: truth commissions as positive net cumulative events (early adopters)




(a) treated after 5 years or more




(b) treated after 10 years or more

Figure 11: Parallel trends for Power distributed according to SES, treatment: truth commissions as positive net cumulative events (late adopters)

Table 4: Power, socioeconomic status, and occurrence of truth revelation procedures

| | Power distributed by socio-economic status | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Lustration Occured | $-0.155^{***}$ | | $-0.183^{***}$ |
| | (0.046) | | (0.046) |
| Truth Commissions Occured | | $0.125^{***}$ | $0.148^{***}$ |
| | | (0.037) | (0.037) |
| GDP per capita (logged) | $-0.218^{***}$ | $-0.229^{***}$ | $-0.209^{***}$ |
| | (0.063) | (0.063) | (0.063) |
| Observations | 1,740 | 1,740 | 1,740 |
| $R^2$ | 0.059 | 0.059 | 0.069 |
| Adjusted $R^2$ | $-0.013$ | $-0.013$ | $-0.004$ |

*Note:* $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

*TRP severity measured as at least one event that occurred in the past (country and year fixed effects)*

tive, although not strong enough to offset the effect of cumulative net lustrations. Power distributed by socioeconomic status is measured on a scale from -2 to 2, which affects our interpretation of these coefficients. Even though they seem larger than the political corruption ones, to arrive at the treatment effect, one must divide the coefficient by four. Hence, for instance in model 3, the effect of experiencing a progressive truth commission event on increasing quality of democracy is a little less than four percent relative to a country that never experienced any truth revelation procedures.

Table 5: Power, socioeconomic status, and truth revelation procedures

| | Power distributed by socio-economic status | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Lustration net cum pos. | $-0.149^{***}$ | | $-0.107$ | | $-0.114$ |
| | (0.041) | | (0.066) | | (0.066) |
| Truth Commissions net cum pos. | | $0.123^{***}$ | | 0.094 | 0.117 |
| | | (0.035) | | (0.133) | (0.132) |
| Lustration Occurred | | | $-0.060$ | | $-0.083$ |
| | | | (0.075) | | (0.075) |
| Truth Commissions Occurred | | | | 0.031 | 0.033 |
| | | | | (0.138) | (0.137) |
| GDP per capita (logged) | $-0.205^{**}$ | $-0.228^{***}$ | $-0.207^{**}$ | $-0.228^{***}$ | $-0.195^{**}$ |
| | (0.064) | (0.063) | (0.064) | (0.063) | (0.064) |
| Observations | 1,740 | 1,740 | 1,740 | 1,740 | 1,740 |
| $R^2$ | 0.061 | 0.060 | 0.061 | 0.060 | 0.071 |
| Adjusted $R^2$ | $-0.012$ | $-0.012$ | $-0.012$ | $-0.013$ | $-0.002$ |

*Note:* $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

*TRP measured with indicator for net cumulative events per year being positive (country and year specific fixed effects)*

In the next step, we look at using the cumulative net events to measure truth revelation procedure treatment. These results are reported in Table 5, above. Net cumulative lustration has a negative impact relative to countries that either never had any truth revelation events involving lustration or had more negative events than positive ones, but once that control group is changed to include only countries that had at least one lustration event, the significance goes away (see model 3). Hence it is hard to put much stock in the results involving this more sensitive measure. The case is similar for truth commissions, although there the coefficient was generally positive.

Finally, we redo the analyses with the minimalist measure and net events. The results are presented below in Table 6.

Table 6: Power, socioeconomic status, and net truth revelation procedures

| | Power distributed by socio-economic status | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Lustration Net | $-0.012^{*}$ | | $-0.010^{*}$ | | $-0.009$ |
| | (0.005) | | (0.005) | | (0.005) |
| Truth Commissions Net | | 0.004 | | $-0.006$ | $-0.006$ |
| | | (0.006) | | (0.007) | (0.007) |
| Lustration Occurred | | | $-0.143^{**}$ | | $-0.169^{***}$ |
| | | | (0.046) | | (0.046) |
| Truth Commissions Occured | | | | $0.140^{***}$ | $0.158^{***}$ |
| | | | | (0.040) | (0.041) |
| GDP per capita (logged) | $-0.206^{**}$ | $-0.240^{***}$ | $-0.196^{**}$ | $-0.220^{***}$ | $-0.179^{**}$ |
| | (0.064) | (0.064) | (0.064) | (0.064) | (0.065) |
| Observations | 1,740 | 1,740 | 1,740 | 1,740 | 1,740 |
| $R^2$ | 0.056 | 0.053 | 0.062 | 0.060 | 0.071 |
| Adjusted $R^2$ | $-0.016$ | $-0.020$ | $-0.011$ | $-0.013$ | $-0.002$ |
| *Note:* | $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$ | | | | |
| | *TJ severity measured as net events (country and year fixed effects)* | | | | |

The results reported here reveal that net lustration has a marginally significant but substantively extremely small effect on the quality of democracy measured with Power Distributed by Socioeconomic status. This effect is .3 percentage points if the control group includes both countries that never experienced a truth revelation procedure (either lustration or truth commission) and those that experienced it but where there were more regressive than progressive events. If the control group is changed to include countries that had at least one progressive event, having more progressive events in the previous year than regressive events contributes to an almost 3 percentage points drop in the quality of democracy as measured by Power Distributed by Socioeconomic Status. And for countries with no lustration experience whatsoever, that effect is closer to 4%. In the case of truth commissions, the net of truth commission events in the previous year does nothing to the quality of democracy, but the mere occurrence of a progressive event has a positive impact in the order of about 4 percent.

How can these relatively weak results, particularly in the case of lustration, be justified? We suggest that there are several reasons. The first is how lustration and truth commission severity are ultimately measured. After discussing at length the benefits of disaggregating transitional justice events into a time series, for the purposes of using diff-in-diff and operationalizing a treatment variable, most of this information is discarded. This is the price we pay for addressing the inability to account for trends in the data or the unobserved covariates.

# 6 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*a*) 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 progressive events relative to regressive 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 et al. (forthcoming) 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.

# References

Albertus, M. & Menaldo, V. (2014), 'Gaming democracy: elite dominance during transition and the prospects for redistribution', *British Journal of Political Science* **44**(3), 575–603.

Ang, M. & Nalepa, M. (2019*a*), 'Can transitional justice improve the quality of representation in new democracies?', *World Politics* **71**.

Ang, M. & Nalepa, M. (2019*b*), 'What can quantitative and formal models teach us about transitional justice'.

Angrist, J. D. & Pischke, J.-S. (2008), *Mostly harmless econometrics: An empiricist's companion*, Princeton university press.

Apaza, C. R. (2009), 'Measuring governance and corruption through the worldwide governance indicators: Critiques, responses, and ongoing scholarly discussion', *PS: Political Science & Politics* **42**(1), 139–143.

Bakiner, O. (2016), *Truth Commissions: Memory, Power, and Legitimacy*, University of Pennsylvania Press.

Bates, G., Cinar, I. & Nalepa, M. (forthcoming), 'Accountability by numbers: Evidence from a global transitional justice events dataset (1946-2016)', *Perspectives on Politics* .

Besley, T. & Burgess, R. (2004), 'Can labor regulation hinder economic performance? evidence from india', *The Quarterly journal of economics* **119**(1), 91–134.

Binningsbø, H. M., Loyle, C. E., Gates, S. & Elster, J. (2012), 'Armed conflict and post-conflict justice, 1946–2006: A dataset', *Journal of Peace Research* **49**(5), 731–740.

Brun, D. A. & Diamond, L. (2014), *Clientelism, social policy, and the quality of democracy*, JHU Press.

Capoccia, G. & Pop-Eleches, G. (n.d.), 'The consequences of punishment: Transitional justice and democratic support in post-war germany', *Comparative Political Studies* .

Coppedge, M., Gerring, J., Lindberg, S. I., Skaaning, S.-E., Teorell, J., Ciobanu, V. I. & Saxer, L. (2017), 'V-dem country coding units v7'.

Coppedge, M., Gerring, J., Lindberg, S., Skaaning, S. & Teorell, J. (2018), 'v-dem codebook v8. varieties of democracy (vdem)'.

David, R. (2003), 'Lustration laws in action: the motives and evaluation of lustration policy in the czech republic and poland (1989–2001)', *Law & Social Inquiry* **28**(2), 387–439.

David, R. (2011), *Lustration and transitional justice: Personnel systems in the Czech Republic, Hungary, and Poland*, University of Pennsylvania Press.

Elster, J. (2004), *Closing the books: Transitional justice in historical perspective*, Cambridge University Press.

Escriba-Folch, A. & Wright, J. (2015), 'Human rights prosecutions and autocratic survival', *International Organization* **69**(02), 343–373.

Gibson, J. L. (2006), 'Overcoming apartheid: Can truth reconcile a divided nation?', *The Annals of the American Academy of Political and Social Science* **603**(1), 82–110.

Grzymala-Busse, A. M. (2002), *Redeeming the communist past: The regeneration of communist parties in East Central Europe*, Cambridge University Press.

Haggard, S. & Kaufman, R. R. (2016), *Dictators and democrats: masses, elites, and regime change*, Princeton University Press.

Halmai, G., Scheppele, K. L. & McAdams, A. J. (1997), *Transitional Justice and the Rule of Law in New Democracies*, University of Notre Dame Press Notre Dame, IN.

Hayner, P. (2011), *Unspeakable Truths: Transitional Justice and the Challenges of Truth Commissions*, Routledge: Auflage, New York, NY and Abington.

Hayner, P. B. (1994), 'Fifteen truth commissions-1974 to 1994: a comparative study', *Hum. Rts. Q.* **16**, 597.

Holmes, S. (1994), 'The end of decommunization', *E. Eur. Const. Rev.* **3**, 33.

Horne, C. (2017), 'Vetting, purges, and lustration: Measurement choices and empirical implications'.

Kaminski, M. M. & Nalepa, M. (2006), 'Judging transitional justice: A new criterion for evaluating truth revelation procedures', *Journal of Conflict Resolution* **50**(3), 383–408.

Keesing's Record of World Events (1998), 'South africa'.

Kitschelt, H. (1999), *Post-communist party systems: competition, representation, and inter-party cooperation*, Cambridge University Press.

Letki, N. (2002), 'Lustration and democratisation in east-central europe', *Europe-Asia Studies* **54**(4), 529–552.

Loyle, C. E. & Appel, B. J. (2017), 'Conflict recurrence and postconflict justice: Addressing motivations and opportunities for sustainable peace', *International Studies Quarterly* **61**(3), 690–703.

Loyle, C. E. & Binningsbø, H. M. (2018), 'Justice during armed conflict: A new dataset on government and rebel strategies', *Journal of Conflict Resolution* **62**(2), 442–466.

Murphy, C. (2017), *The Conceptual Foundations of Transitional Justice*, Cambridge University Press.

Nalepa, M. (2008), 'To punish the guilty and protect the innocent comparing truth revelation procedures', *Journal of Theoretical Politics* **20**(2), 221–245.

Nalepa, M. (2010*a*), 'Captured commitments: an analytic narrative of transitions with transitional justice', *World Politics* **62**(2), 341–380.

Nalepa, M. (2010*b*), *Skeletons in the closet: Transitional justice in post-communist Europe*, Cambridge Studies in Comparative Politics, Cambridge University Press.

Nalepa, M. (2012), 'Tolerating mistakes: How do popular perceptions of procedural fairness affect demand for transitional justice?', *Journal of Conflict Resolution* **56**(3), 490–515.

Olsen, T. D., Payne, L. A. & Reiter, A. G. (2010), 'The justice balance: When transitional justice improves human rights and democracy', *Human Rights Quarterly* **32**(4), 980–1007.

Osiatynski, W. (1992), 'Agent walesa', *E. Eur. Const. Rev.* **1**, 28.

Osiatynski, W. (1994), 'Decommunization and recommunization in poland', *E. Eur. Const. Rev.* **3**, 36.

Pemstein, D., Marquardt, K. L., Tzelgov, E., Wang, Y.-t. & Miri, F. (2015), 'The v-dem measurement model: Latent variable analysis for cross-national and cross-temporal expert-coded data'.

Pinto, A. C. (2008), 'Political purges and state crisis in portugal's transition to democracy, 1975-76', *Journal of Contemporary History* **43**(2), 305–332.

Prorok, A. K. (2017), 'The (in) compatibility of peace and justice? the international criminal court and civil conflict termination', *International organization* **71**(2), 213–243.

Stan, L. et al. (2009), *Transitional justice in Eastern Europe and the former Soviet Union: Reckoning with the communist past*, Routledge.

Szczerbiak, A. (2002), 'Dealing with the communist past or the politics of the present? lustration in post-communist poland', *Europe-Asia Studies* **54**(4), 553–572.

Thoms, O. N., Ron, J. & Paris, R. (2010), 'State-level effects of transitional justice: what do we know?', *International Journal of Transitional Justice* **4**(3), 329–354.

United States Institute of Peace (2011), 'Truth commission: South africa'.

Van der Merwe, H., Baxter, V. & Chapman, A. R. (2009), *Assessing the impact of transitional justice: Challenges for empirical research*, US Institute of Peace Press.

Vinjamuri, L. & Snyder, J. (2004), 'Advocacy and scholarship in the study of international war crime tribunals and transitional justice', *Annual Review of Political Science* **7**, 345–362.

Williams, K., Fowler, B. & Szczerbiak, A. (2005), 'Explaining lustration in central europe: a 'post-communist politics' approach', *Democratization* **12**(1), 22–43.

# A Empirical Appendix

Figure 12 shows the disaggregated event data for truth revelation procedures (lustration and truth commissions) as both a general time trend (right) and trend in years since transition (left).

Figure 12: Disaggregated Truth Revelation Data over Time

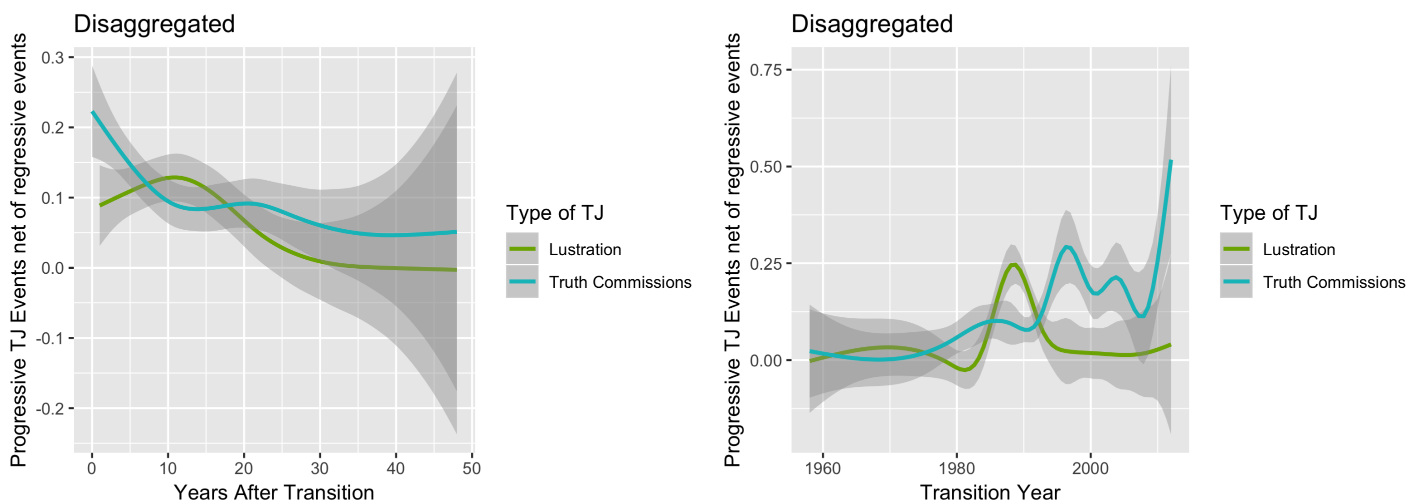



Figure 13 shows the pair-wise relationship of different transitional events per year.

Figure 13: Pair-wise transitional events per year