

# Truth commissions and Democracy: Testing the Links

Geoff Dancy\* & Oskar Timo Thoms†

August 26, 2019

Paper to be presented at the APSA Annual Meeting, Washington, DC, September 1, 2019.

**Please do not cite or circulate without author permission!**

## Abstract

This paper distinguishes institutions from practices in order to revisit the question of truth commission impacts on democratic governance. According to quantitative analyses of countries undergoing transitions between 1970 and 2015, which address the endogeneity of truth commissions to democracy, these commissions help improve democratic participation and physical integrity rights but undermine institutions of liberal democracy and accountability by judiciaries. This contradicts a key mechanism of truth commission (TC) influence in the transitional justice literature.

---

\*Associate Professor, Tulane University, New Orleans.

†Simons Research Fellow in International Law and Human Security 2018-19, Simon Fraser University, Vancouver.  
From September 2019: Lecturer, Norman Paterson School of International Affairs, Carleton University, Ottawa.

# 1 Introduction

When truth commissions (TCs) as we know them were invented in the early 1980s, many understood them to be a middle-ground solution for handling serious human rights violations. Instead of criminal prosecution on one side, or blanket amnesty on the other, the state would set up a commission to provide a record of the truth. However, this “third way” between vengeance and forgiveness, as Bishop Tutu in South Africa referred to it,<sup>1</sup> increasingly became *the* way to do transitional justice (TJ).<sup>2</sup> The numbers are surprising. In all, since 1970, 87 TCs have operated for over 500 total years in 61 countries. Over half of these, 47, were established in the 21st Century; eight are now ongoing.

One reason for the rise of TCs is that their public image changed. In the early 1990s, practitioners had to rationalize the choice to substitute truth for criminal justice. Because TCs were originally seen as a surrender of strongly held convictions that wrongdoers should be punished, commentators were forced to reassure justice-minded activists.<sup>3</sup> They claimed that TCs were rooted in an “ethic of responsibility” or a “logic of consequences.”<sup>4</sup> By the end of the 1990s, though, no more justification was needed. TCs were suddenly a favorite for human rights advocates. There were no longer a sell-out option, but a moral end in themselves.<sup>5</sup> Settling for the truth became upholding a right to truth.

Partly behind this shift from TC apology to promotion was the global popularity of the South African Truth and Reconciliation Commission (TRC). However, theory may have also played a role. Transitional justice scholars started to argue that TCs possessed the power to catalyze democratic norms in uncertain times.<sup>6</sup> This idea proliferated through the world of practice. The United Nations and transnational NGOs like the International Center for Transitional Justice championed TCs, and helped spread them around the world. Yet, twenty years after the TC-democracy nexus was dreamed up, there remains very little evidence that it is real. Can it be shown that these temporary bodies, which operate on average for just under 2.5 years, exert any sort of independent impact on democratic governance?

In this article, we answer this question using the best quantitative evidence available. First, though, we must do some unpacking. The current literature on TC impact exhibits a number of methodological issues that need resolving. Among these are the analysis of widely different cases, the inclusion of disparate sets of TCs, lack of research on TC processes, and under-specification of the dependent variable, which may be any measure of democracy, rule of law, or human rights. In short, every study is very different from all of the others. To compound these problems, scholars have still not satisfactorily handled the issue of endogeneity. It is highly probable that the same factors affecting democratic governance also affect the design and implementation of TCs. To study impact, then, we must account for selection effects.

The second problem we must address is theory. While researchers have proposed various mechanisms that link TCs to democratic practices, these mechanisms are not often grounded in rich conceptual work from the field of comparative democratization. Empirical research tends to use off-the-shelf measures of democracy that poorly match the concerns of TJ theory. What *kinds* of

---

<sup>1</sup>Chapman and Ball 2001.

<sup>2</sup>Daly 2008; Dancy, Kim, and Wiebelhaus-Brahm 2010.

<sup>3</sup>Mamdani 2002; Wilson 2001.

<sup>4</sup>Zalaquett 1992; Snyder and Vinjamuri 2003.

<sup>5</sup>Dimitrijević 2006.

<sup>6</sup>Gutmann and Thompson 2000.

democratic practices might TCs affect, and how? To answer this question, we must disaggregate the concept of democracy.

After we break down these issues, we rebuild a theory of TCs and democracy, focusing on four main avenues through which these mechanism could strengthen democracy. We then test those each of those four explanations using 45 years of evidence. Ultimately, after accounting for selection, we find that TCs might plausibly bolster democratic participation and lower the risk of violent repression, but they do not contribute to the enhancement of institutionalized liberal rights protections, nor do they strengthen judicial checks on the executive. In short, TCs may change behavior, but they do not seem to change institutions.

## 2 Literature Review

Truth commissions are temporary, officially sanctioned bodies with mandates to investigate specific periods of past state abuses.<sup>7</sup> They signal an official determination, genuine or ingenuous, to establish the truth about past abuses and to avoid their recurrence.<sup>8</sup> Subject to the creativity of their designers, TCs are able to provide official acknowledgement of abuses and rewrite history; focus on institutional and structural factors that permitted such abuses; make recommendations for legal and institutional reforms; provide information that is useful for subsequent prosecutions and vetting of abusive public officials; and create a basis for victim reparations and memorialization.<sup>9</sup> While some scholars are careful not to overstate the potential impacts of TCs, especially regarding their democratizing effects,<sup>10</sup> others claim that TCs are “increasingly deemed to be imperative for the consolidation of democracy.”<sup>11</sup>

There is a growing scholarly literature that assesses such causal claims, examining the correlation between TJ mechanisms and political violence, respect for human rights, rule of law, and indicators of democracy. We are concerned primarily with multi-country comparative studies — using quantitative or qualitative methods — rather than single-country case studies. Single case studies are well suited to theory-building and testing crucial cases, but they do not offer general conclusions about causal effects. They are ill-equipped to consider alternative explanations or counterfactuals based on negative cases, which must be considered in order to generalize.<sup>12</sup>

Given the relative youth of the empirical turn in TJ scholarship – now just over a decade old – it is not surprising controlled comparisons frequently produce contradictory results. For instance, while some studies find no systematic associations between TCs and the extent of democracy or democratization,<sup>13</sup> others find positive associations with democratization.<sup>14</sup> More disconcertingly, while one large-sample quantitative study finds a positive relationship between TCs and the protection of personal integrity rights,<sup>15</sup> several others find that the presence of TCs is, on average, associated

---

<sup>7</sup>Dancy, Kim, and Wiebelhaus-Brahm 2010.

<sup>8</sup>On ingenuous TCs, see Subotić 2009.

<sup>9</sup>Goldstone 1996; Kritz 1996; Méndez 1997; Minow 1998; Hayner 2001.

<sup>10</sup>Popkin and Roht-Arriaza 1995, 115–116; Minow 1998, 83; Hayner 2001, 8; Gibson 2009.

<sup>11</sup>Borer 2006, 17.

<sup>12</sup>Brahm 2007; Backer 2009.

<sup>13</sup>Wiebelhaus-Brahm 2010.

<sup>14</sup>Kenney and Spears 2005; Taylor and Dukalskis 2012.

<sup>15</sup>Kim and Sikkink 2010.

with *less* subsequent respect for these human rights.<sup>16</sup> The latter studies find that when TCs are used alone, they have negative impacts, but in combination with criminal trials and partial amnesties, they are associated with improved human rights practices. This finding is also supported by some in-depth case studies.<sup>17</sup> This leads the authors to conclude that TCs “[enhance] the human rights-promoting qualities of those mechanisms,” but by themselves, they raise expectations of truth and justice but fail to deliver.<sup>18</sup>

## 2.1 Problems

Thus far, there is little convincing systematic evidence that TCs exert strong positive or negative effects on democracy, respect for human rights, or the rule of law. Many of the state-level impacts of TJ policies are unclear.<sup>19</sup> The reason for this is in part methodological. Discrepancies in statistical findings on TCs impact likely result from different research designs and different data sources.

First, studies disagree on the relevant universe of cases to analyze. Some studies consider only cases that have had TCs,<sup>20</sup> while others consider all democratic and/or or post-conflict transitions,<sup>21</sup> and yet others include global samples, given available data.<sup>22</sup> Case selection should be guided by clear theory because these choices have important ramifications. Including only TC cases, for example, disallows comparison to non-TC contexts. Furthermore, including TCs in non-transitional contexts means possibly skewing results, by including cases where TCs may be neither likely nor relevant. TCs in consolidated democracies and consolidated autocracies have very different goals and political stakes; therefore, there is reason to suspect they have different impacts.<sup>23</sup>

Second, studies work with different definitions of TCs, and as a result, they compare different numbers.<sup>24</sup> When combined with case selection and modeling choices, this will produce widely divergent observations for analysis. As shown in Table 1, in the quantitative literature, the sample N ranges from 28 to 3581. Needless to say, such different research designs are unlikely to produce similar or comparable results.

Table 1: Select TC impact studies

Study	Case selection	TCs	Time-series	N
Kim and Sikkink (2010)	Trans Demo (1980-2004)	28	Yes	1314
Olsen, Payne, and Reiter (2010a)	Trans Demo (1970-2004)	59	No	38-67
Wiebelhaus-Brahm (2010)	All countries (1981-2005)	29	Yes	3581
Taylor and Dukalskis (2012)	TC Countries (1970-2009)	28	No	28

Third, with the exception of Taylor and Dukalskis (2012), there is too little analysis of variation in the implementation of TCs. Impact studies have almost always treated TCs as uniform, binary

<sup>16</sup>Olsen, Payne, and Reiter 2010a; Olsen, Payne, Reiter, and Wiebelhaus-Brahm 2010; Wiebelhaus-Brahm 2010.

<sup>17</sup>Wiebelhaus-Brahm 2010.

<sup>18</sup>Olsen, Payne, Reiter, and Wiebelhaus-Brahm 2010, 469.

<sup>19</sup>Thoms, Ron, and Paris 2010.

<sup>20</sup>Taylor and Dukalskis 2012.

<sup>21</sup>Kim and Sikkink 2010; Olsen, Payne, and Reiter 2010a.

<sup>22</sup>Wiebelhaus-Brahm 2010.

<sup>23</sup>Bakiner 2014, 10; Loyle and Davenport 2016.

<sup>24</sup>Dancy, Kim, and Wiebelhaus-Brahm 2010; Bakiner 2014, 6.

treatments. While scholars often acknowledge that TCs differ greatly in their mandates, compositions, resources, practices, and completion of their work, this variation has rarely been taken into account.<sup>25</sup> In part, this is due to limited data availability for some commissions, which hampers the development of more sophisticated measures that can be applied across the universe of cases.<sup>26</sup>

Fourth, there are significant data limitations of large-sample quantitative studies, not just with respect to TJ mechanisms, but also with respect to outcome measures. For instance, the leading cross-national quantitative indicators of state-level human rights performance, such as the Political Terror Scale and the CIRI human rights indices are problematic because they suffer from incomplete data across countries and years; are coded from potentially biased source materials; and at times may make abuses appear “stickier” than they actually are.<sup>27</sup>

This problem is particularly acute with respect to democratization. Large-sample studies of the effects of TCs on democratic governance or democratization invariably employ one or both of two leading measures of democracy, the Freedom House indices and the Polity score, which also have significant conceptual and measurement problems. While the Polity data fulfills important scholarly requirements of transparent methodology and reproducibility, the Freedom House data do not, raising doubts about its validity and internal consistency over time. Moreover, both suffer from problems of conceptual logic, such as using one-dimensional indices to measure the multidimensional concept of democracy, and for differently weighting and aggregating their components without theoretical justification for considering certain attributes more important than others.<sup>28</sup>

A fifth concern is the thorny problem of endogeneity. Scholars argue that TCs can strengthen democratization if they help discredit previously unaccountable institutions, but this seems to only occur if TCs are established in countries where democratization is already well underway.<sup>29</sup> Similarly, one study finds that TCs only enhance post-conflict peace duration when the analysis is restricted to reasonably established democracies.<sup>30</sup> These findings suggest that only already reasonably democratic states can generate the stability and legitimacy needed for TCs to be accepted as a form of justice and to have positive impacts.<sup>31</sup> In other words, existing studies have not been able to determine whether TCs have an independent effect on democratization, or are a symptom of the democratization process. Some research investigating the interaction of different TJ mechanisms finds that TCs only have positive effects on democratization when they are combined with prosecutions and partial amnesties.<sup>32</sup> These causal relationships are difficult to disentangle because it may be that democratization and TC success are driven by the same underlying factors.

## 2.2 Moving Forward

Facing contradictory findings on TCs and democracy, and complicating methodological issues, one option is to abandon large-N research on big outcomes for clearer thinking about process and mid-range impacts. For example, Bakiner carefully “process-traces” the causal linkages from TCs

---

<sup>25</sup>Rotberg and Thompson 2000; Chapman and Ball 2001; Hayner 2001; Brahm 2007; Backer 2009; Bakiner 2014.

<sup>26</sup>Olsen, Payne, Reiter, and Wiebelhaus-Brahm 2010, 466.

<sup>27</sup>Hafner-Burton and Ron 2009; Thoms, Ron, and Paris 2010; Clark and Sikink 2013; Fariss 2014.

<sup>28</sup>Foweraker and Krznaric 2000; Munck and Verkuilen 2002.

<sup>29</sup>de Brito, Enríquez, and Aguilar 2001; Wiebelhaus-Brahm 2010.

<sup>30</sup>Lie, Binningsbø, and Gates 2007.

<sup>31</sup>Arenhövel 2008, 581.

<sup>32</sup>Olsen, Payne, and Reiter 2010a.

operations, outputs, and direct political effects, such as whether it received a public endorsement, led to institutional reforms, or received civil society attention.<sup>33</sup> At the end of this important work, he concludes that “...the normative impact of TCs on politics and society is undeniable,” but “future research should devise innovative and precise data collection and analysis tools to assess the magnitude, direction and specific causal mechanism....” The reason for this statement is that, despite a high level of systematicity, Bakiner’s work still cannot speak to the overall impact of TCs; it can only make an argument that various TCs have inspired reforms, and without comparison to negative cases. Indeed, few process-tracing studies rigorously assess alternative causal explanations for post-TJ outcomes or compare TJ experiences to political transitions without such policies, i.e., to “control” cases. However, comparisons involving negative cases are critical for establishing whether democratic or human rights outcomes would have been possible without TJ.

An example of mid-range work that accounts for negative cases is Ishiyama and Laoye (2016), which examines the relationship between TC practice and trust in courts in Africa.<sup>34</sup> Echoing earlier findings from South Africa,<sup>35</sup> the authors discover that subjects in countries with TCs are no more likely to trust courts. If anything, they become less trustful of judicial institutions. This is important because it counteracts claims that, by providing an unblemished record of the truth, these bodies would help societies “heal” or restore themselves to a condition of “political morality.”<sup>36</sup>

The move to the mid-range is useful primarily for alerting us to important variations within TCs, or to thinking about causal mechanisms. However, it still leaves many questions unanswered. Bakiner may hint that TCs have the power to nudge political society toward reform, but he cannot demonstrate the magnitude of that power. Ishiyama and Laoye show that there is reason to doubt TCs’ ability to change political attitudes, but they do not follow with how that might affect democracy as a whole. As in large-N, cross-national studies, we are left with contradictory findings and little sense of resolution.

### 3 Pathways of truth commission influence

A return to the systematic study of TC impact on democratic governance means starting with theory. In saying “democracy”, we refer to a regime type that consists of certain institutions, practices, and attitudes, specifically those that promote toleration of opposition, political contestation, public participation, and constraints on executive power.

A great deal of theory has devoted its attention to whether TCs can change individual or collective *attitudes* in society. Some scholars argue that TCs contribute to individual healing and reconciliation,<sup>37</sup> thereby helping to break “the cycle of revenge and hatred between former enemies.”<sup>38</sup> It is no surprise that scholars have zeroed in on the promise of reconciliation. This link was intentionally manufactured by political leaders, who started to write the word “reconciliation” into the name of many commissions starting in the 1990s.<sup>39</sup>

---

<sup>33</sup>Bakiner 2014, 18–19.

<sup>34</sup>Ishiyama and Laoye 2016.

<sup>35</sup>Gibson 2004.

<sup>36</sup>Du Toit 2000, 125.

<sup>37</sup>Minow 1998.

<sup>38</sup>Hayner 2001, 154.

<sup>39</sup>Dancy, Kim, and Wiebelhaus-Brahm 2010.

There are a number of reasons to be suspicious, though, of theory linking TCs to feelings of harmony between former violent enemies. In religious form, reconciliation is built on the Christian notion of atonement; in secular form, reconciliation is more akin the deliberative democratic notions of ideal speech between political opponents.<sup>40</sup> Regardless, one can say that there are many “varieties of reconciliation”, making concrete observation difficult.<sup>41</sup> How does one measure atonement or ideal speech?

Beyond this conceptual problem is another more immediate concern: very little observable evidence indicates that TCs are measurably linked with personal healing, trust, or reconciliation, however defined.<sup>42</sup> In a systematic review of relevant psychological literature, Mendeloff (2009) finds that there is little proof that truth-telling processes satisfy victim needs for justice, ease emotional and psychological suffering, or dampen desires for vengeance.<sup>43</sup> Furthermore, in the South African context, researchers discovered no significant change in attitudes of trust a decade after the TRC completed its work,<sup>44</sup> leaving one scholar of public opinion to wonder whether the social “bargain” had “unraveled.”<sup>45</sup> These findings are supported by other case studies in countries like Sierra Leone,<sup>46</sup> and Africa-wide studies of TCs and trust, which turn up few notable results.<sup>47</sup>

It is even possible that TCs change attitudes in a negative direction. Skeptics argue that TCs may sharpen societal divisions, undermine peace settlements in ongoing conflicts, and even provoke backlash from spoilers, because establishing a record of past abuses may generate further resentment among both victims and perpetrators.<sup>48</sup> Commissions may in fact reinforce prevailing toxic political attitudes if they fail to gain legitimacy.<sup>49</sup> In this way, they may actually provide new grievances to be exploited by cynical political elites.

For these reasons, it might be prudent to relax theoretical expectations about the impact of TCs on political attitudes or values. Once formed, attitudes and values are difficult to change. Whether people believe in democracy is likely the *byproduct* of governance, rather than its cause. Therefore, we turn instead to conceptual foundations of democracy rooted in institutions and practices.

### 3.1 Democratic Institutions

Minimally, a democracy must have contested elections in which people participate. This is Robert Dahl’s formulation, which built on previous work by Schumpeter.<sup>50</sup> Maximally, a *liberal* democracy must also include checks on the executive, rule of law, and minority rights protections. These values can be promoted through able and properly functioning judiciaries. Thus, TCs could exert an impact on democratic *institutions* either by affecting the operation of fair and contested elections, or by empowering legal institutions to constrain majority or executive power.

---

<sup>40</sup>Bashir 2012.

<sup>41</sup>Meierhenrich 2008.

<sup>42</sup>Graybill and Lanegran 2004.

<sup>43</sup>Mendeloff 2009.

<sup>44</sup>Gibson 2004.

<sup>45</sup>Backer 2010.

<sup>46</sup>Millar 2011.

<sup>47</sup>Ishiyama and Laoye 2016.

<sup>48</sup>Snyder and Vinjamuri 2003; Mendeloff 2004.

<sup>49</sup>Subotić 2009.

<sup>50</sup>Dahl 1971.

At first blush, it is hard to conceive of how TCs might alter electoral institutions. Most TCs do not make recommendations concerning, say, the operation of electoral management bodies. However, a link may exist. TCs are often framed as the public face of a dirty secret: that in order to transition from authoritarian or civil war states, a compromise must be struck with actors who have engaged in mass violence.<sup>51</sup> If those violent actors are to engage politically in a new democratic arrangement, they must be provided assurances that they will avoid prison for their crimes. Such elite pacts are often needed to neutralize the risk that former violent actors will resume their old ways instead of playing by the rules of new institutions.

Elite bargains of this type are ugly and often unpopular because they forfeit victim's justice for the sake of social order.<sup>52</sup> TCs provide political cover. They serve as a useful policy alternative where demands for justice are high but elites deem trials too destabilizing because they may violate the pact and mobilize so-called spoilers.<sup>53</sup> At the same time, politicians can advance a plausible argument that the TC's operation will uphold the right to truth.<sup>54</sup> In the end, then, TCs may allow regime leaders to hold free and fair elections without worry because they know that former opponents will participate, no longer seeing the benefit in resorting to extralegal violence.<sup>55</sup> If this scenario captures reality, the following expectation should hold:

Hypothesis 1: *TCs will be associated with regular, free, and fair electoral institutions.*

While possible, the election theory still strains credulity. It is unclear why perpetrators of mass violence, considering a coup or rebellion, would ultimately be convinced to play by the rules of the democratic game simply due to a TC's work. Why, for example, would they care whether the ruling government has political cover for an elite pact?

The more common, and perhaps more believable, argument, is that TCs alter democratic institutions by generating judicial reform. As Jennifer Llewellyn contends, "TCs were able to look beyond individual instances of abuse—the focus of trials—to identify broader patterns...and to recommend legal and institutional reforms..."<sup>56</sup> The mandate of TCs to make written recommendations for judicial reform is repeatedly emphasized as a source of positive change.<sup>57</sup> In addition, if they demonstrate procedural fairness and even-handedness in their own operations, TCs may be able to set an example for rule of law institutions.<sup>58</sup>

If TC recommendations—some of which make suggestions for constitutional amendments or administrative changes to the judicial recruitment and funding—are implemented, this could exert a substantial effect on liberal democratic institutions. Therefore:

Hypothesis 2a: *TCs will be associated with stronger liberal democratic institutions.*

Hypothesis 2b: *TCs will be associated with judicial reform.*

---

<sup>51</sup>O'Donnell and Schmitter 1986.

<sup>52</sup>Wilson 2001.

<sup>53</sup>Snyder and Vinjamuri 2003.

<sup>54</sup>Popkin and Roht-Arriaza 1995; Kritz 1996; Minow 1998; Rotberg and Thompson 2000.

<sup>55</sup>Popkin and Roht-Arriaza 1995; Rotberg and Thompson 2000.

<sup>56</sup>Llewellyn 2011.

<sup>57</sup>Minow 1998; Hayner 2001; de Greiff 2006; Bakiner 2014.

<sup>58</sup>Gibson 2009.



This expectation has so far gone untested in cross-national studies. The key link in the causal chain between TC operation and judicial reform is *whether* their recommendations are implemented. Some case-study research has begun to process-trace the extent to which governments have acted upon recommendations, finding only limited impact of TCs on reform.<sup>59</sup> Bakiner (2014) finds no relationship between TCs through vetting of abusive actors from public service; out of fifteen TCs, few actually recommend the removal of presumed perpetrators from office, and only one government partially met the demand. The same study finds some judicial impact, as a small number of legal cases incorporated TC findings as contextual information. The author concludes: “the design of TCs, which curtails their judicial powers, accounts for their relative insignificance in judicial processes. Evidence also suggests that even when commissions make full use of their capacity to facilitate prosecutions, courts still neglect their findings.”<sup>60</sup> Even though theorists still find promise in TCs’ recommendation function, case research suggests that TC impacts on judicial institutions are circumscribed.

There is another more sinister possibility. TCs may also be used by culpable state agents as public relations “smoke screens” to mask continuing abuses.<sup>61</sup> Because they direct attention at institutional and structural causes of abuses, they tend to neglect individual responsibility for crimes and the importance of judicial processes.<sup>62</sup> This might perpetuate impunity. Furthermore, because they are public spectacles, TCs may absorb attention away from corrupt institutions, leaving those problematic institutions untouched and intact.

By their nature, institutions are sticky. They do not reform quickly, and they do not reform on their own. Long-term change requires political will, and it requires that people alter their behaviors. Is it possible that TCs influence behavioral change?

### 3.2 Democratic Practices

Many analyses of democracy focus exclusively on the arrangement of formal institutions—including standing legal and constitutional provisions. Often neglected are democratic *practices*. Elections are worthless if citizens do not meaningfully participate. De jure judicial protections are worthless if judges do not demonstrate positive judicial independence.<sup>63</sup> And little of this matters if military, police and security forces regularly engage in extralegal violence.

Of course, modifying institutions is a way to change behavior. That is the reason lawmakers spend so much time crafting legislation. However, practices can change independent institutions as well. Short of encouraging institutional reform, how might TCs alter democratic practices?

One obvious answer is that TCs can alter behavior by sending signals. After all, these bodies are highly publicized affairs, attracting attention in national, and sometimes global, news media. More recent TCs, particularly those since the late 1990s, have also made common practice of conducting hearings that can be attended by lay members of the public, journalists, and foreign visitors.<sup>64</sup> The messages that TCs transmit through their public relations could alter collective behavior, much as high-profile trials or congressional hearings can serve an expressivist social function.

---

<sup>59</sup>Wiebelhaus-Brahm 2010.

<sup>60</sup>Bakiner 2014, 22.

<sup>61</sup>Snyder and Vinjamuri 2003; Mendeloff 2004.

<sup>62</sup>Tepperman 2002; Weiffen 2012.

<sup>63</sup>Hilbink 2012.

<sup>64</sup>Freeman 2009.

The message being sent, however honest or dishonest, is that the government means to recognize yesterday's political violence, establish accountability, and offer reforms to remove remaining enclaves of lawless power.<sup>65</sup> In short, the new regime intends to distance itself from the country's violent or authoritarian past.<sup>66</sup>

Who receives this message? In one formulation, it is citizens, or civil society actors, who for years have toiled for change.<sup>67</sup> With the establishment and operation of a TC, these groups are provided a forum for engaging politically, with little fear of reprisal. The effect is that TCs and their reports may bring "the views of different groups into a single realm of comprehensibility."<sup>68</sup> In so doing, these bodies encourage societal actors to engage in broader democratic politics and respect the rule of law.<sup>69</sup> If this is the case, then the following expectation should hold:

*Hypothesis 3: TCs will be associated with higher levels of democratic participation.*

Some may perceive a theory based on signalling or reform-based expressivism to be too abstract. The symbolism of TCs can seem far removed from the cynical political realities of transitional periods. However, it is worth noting that *any* theories connecting TCs to macro-social outcomes must rely on signalling in some form. Otherwise, why would we assume TC recommendations to be important? The reason is that, if implemented by parliamentarians or presidents in a transitional country, they could strengthen liberal rule of law. However, this causal story relies on government officials getting a message from TCs hearings and reports that certain reforms are critical. In this version of events, as in theories linking TCs to political participation, messaging is still important. The only difference is the audience. In the institutional reform theory, the TCs is communicating recommendations directly to the ruling government itself. In the participatory theory, TCs's audience is citizens.

One more audience exists. In some accounts, TCs might deter repression. For this to happen, the state's agents of violence must receive the message that change is afoot. In authoritarian and civil war countries, these agents are provided a great deal of latitude to enforce order however they see fit. And in states with weak governments, central officials may have a difficult time even controlling their agents in the field. In either case, the result is violent repression carried out by police, security forces, and pro-government militias.

Mounting research finds that these agents of violence are deterred by criminal prosecutions.<sup>70</sup> However, this is not just because of the potential material costs of punishments, which include prison time. It is also because of "normative costs."<sup>71</sup> TCs, like trials, have a powerful communicative function. They can spread stigma. The desire to avoid being called a rights violator, or being associated with the violence of the past, might be enough to alter an agent's willingness to commit repressive acts. If this is the case, the following expectation should hold:

*Hypothesis 4: TCs will be associated with greater human rights protections in practice.*

---

<sup>65</sup>Gairdner 1999.

<sup>66</sup>Minow 1998; Teitel 2000.

<sup>67</sup>Crocker 2000.

<sup>68</sup>Taylor and Dukalskis 2012, 674.

<sup>69</sup>Brahm 2007; Arenhövel 2008; Weiffen 2012.

<sup>70</sup>Kim and Sikkink 2010; Jo and Simmons 2016; Dancy and Montal 2017; Dancy et al. 2019.

<sup>71</sup>Kim and Sikkink 2010, 957.

Importantly, the expectations we outline concerning the relationships between TCs and democratic practice do not rely on psycho-social notions like healing, trust, or reconciliation. Citizens may have strongly negative attitudes toward others in society, and even toward TCs themselves. Even so, the public discussion surrounding TCs could still serve as a jumping-off point for engaging in political contestation. Regardless of its positivity or negativity, such engagement would be participating in the new democracy. For their part, agents of violence would probably begrudge commissioners conducting oversight into their business. Nevertheless, they may still be influenced to alter behavior to avoid spotlight. In short, it might be more accurate to think of truth commissions in relation to agonistic, or highly contested, modes of democratic practice, rather than deliberative or harmonious forms.<sup>72</sup> The truth is that TCs might excite feelings of injustice, but where feelings are expressed in public, and resolved short of violence, they contribute to democracy

## 4 Data and Methods

### 4.1 Sample

Our sample needs to be limited to relevant cases for analyzing TC impact. We include transitional countries in three senses of the concept. First, we include any country that has had a democratic transition since 1970, from the year of transition. To determine cases of democratic transition, we use only the baseline electoral contestation criterion, taken from a dataset created by Boix, Miller, and Rosato (2012); when they code a change from non-democracy to democracy from one year to the next, we include this case as a democratic transition. Second, new states established or reestablished since 1970 are included if they are democratic according to Boix, Miller, and Rosato (2012) in the first year of their existence. Third, we include any democracies that experience internal conflict, because TCs are often established to deal with transitions from war to peace. To determine these cases, we rely on the Uppsala Conflict Data Programme (UCDP) conflict data discussed in Section 4.5. Once a country enters the sample, it remains, even if it experiences autocratic reversion. We end the period of analysis in 2015, because there is insufficient information for coding our TC variables (see Section 4.3) for commissions established after that year. The included country-years are listed in Appendix B.

### 4.2 Dependent variables

We use the Varieties of Democracy (V-Dem) dataset to measure different aspects of democracy. This data collection project is a large-scale effort involving thousands of country and subject matter experts to code key conceptions of democracy, including electoral, liberal-institutional, participatory, deliberative, and egalitarian dimensions, and many sub-component measures of democratic institutions and practices. All V-Dem democracy indices, sub-component and institutional measures are derived from Bayesian item response theory measurement models that aggregate ordinal ratings on a given question by multiple subject-specific country experts and provide estimates of uncertainty due to disagreement and systematic measurement errors.

Rather than testing our hypotheses on the broader democracy indices, which all use electoral democracy as the minimum criterion, we employ three democracy component indices addressing

---

<sup>72</sup>Mouffe 2013.

the electoral, liberal, and participatory aspects separately. The electoral democracy index aggregates indices measuring freedom of association, clean elections, freedom of expression, elected officials, and suffrage.<sup>73</sup> The liberal component index combines indices measuring equality before the law and individual liberties, judicial constraints on the executive, and legislative constraints on the executive. This combined measure is intended to capture the extent to which individual and minority rights are protected “against the tyranny by the state and the tyranny of the majority.”<sup>74</sup> We also use the V-Dem judicial reform measure as an outcome because, as discussed in Section 3.1, the TJ literature sometimes claims that TCs help prevent future abuses by encouraging institutional reforms that improve accountability. This measure answers the question “Were the judiciary’s formal powers altered this year in ways that affect its ability to control the arbitrary use of state authority?” The original ratings of this measure range from *reducing* to *enhancing* the judiciary’s ability to control arbitrary power.<sup>75</sup>

To address democratic practice rather than institutions, we use the V-Dem participatory component index, which emphasizes active participation by citizens in electoral and non-electoral political processes.<sup>76</sup> This measure combines indices measuring civil society participation, direct popular vote, elected local government power, and elected regional government power.

Further, to examine the effect of TCs on human rights practices, we use the latent physical integrity (LPI) measure created by Schnakenberg and Fariss (2014). A key inferential issue in human rights research is measurement bias due to information effects and changing standards of human rights accountability.<sup>77</sup> On the one hand, the quality and quantity of information on physical integrity violations has increased as monitoring agencies have multiplied and gathered more accurate information on more violations in more places. On the other hand, in continually pressing governments to reform, activists tend to classify more and more acts as human rights violations. As a result, human rights reports have become increasingly stringent assessments of state behaviour over time. Standards-based measures commonly used in scholarship are coded from such reports reflect a dynamic standard of accountability<sup>78</sup> and may not respond well to actual changes in the levels or types of abuses.<sup>79</sup> This makes it difficult to infer human rights practices based on such measures because these indicators are biased toward finding worsening abuses.<sup>80</sup> The LPI measure is one of the most sophisticated efforts to date to address concerns about measurement bias of quantitative human rights measures. It is derived from a measurement model designed to take into account changing standards of accountability, by combining a range of standards-based and events measures of repression and state mass violence and incorporating over-time effects<sup>81</sup>. While the latent measure primarily addresses the dynamic standard of accountability, it is also useful in addressing concerns about information effects, since it combines information from a variety of different human rights measures, several of which are based on different source materials.

---

<sup>73</sup>This index combines additive and multiplicative aggregation approaches, but our results below are the same when using just the additive approach (not shown).

<sup>74</sup>Coppedge et al. 2018, 45.

<sup>75</sup>We also ran the analyses on the V-Dem judicial constraints measure, a sub-component of the liberal democracy component measure; the TC variables have no statistical effects on this measure (not shown).

<sup>76</sup>Coppedge et al. 2018, 46.

<sup>77</sup>Keck and Sikkink 1998, 195; Clark and Sikkink 2013, 175–77; Fariss 2014.

<sup>78</sup>Fariss 2014.

<sup>79</sup>Clark and Sikkink 2013.

<sup>80</sup>Clark and Sikkink 2013, 568.

<sup>81</sup>Schnakenberg and Fariss 2014.

The original V-Dem democracy component indices are on a zero-to-one scale, while the V-Dem judicial reform and the LPI are on a standardized interval scale which is similar to a normal Z-score. For comparison between results for the different outcome variables, we also put the latter two variables on a zero-to-one scale.

### 4.3 Truth commission variables

To capture the richest and most up-to-date information on truth commissions, we use variables constructed from the relational Transitional Justice Research Collaborative Database (TJRC).<sup>82</sup> The TJRC codes qualitative and quantitative differences in all transitional justice mechanisms used globally. It possesses over 10,000 event records, and it contains dozens of disaggregated variables related to prosecutions, truth-telling procedures, and amnesties.<sup>83</sup> The TJRC allows us to generate measures that go beyond previous studies of transitional justice in two ways. The first is that it contains information on 87 TCs, which is the full universe of cases, collected irrespective of the context in which they operate. Second, it allows us to go beyond the binary measures of truth commissions that have been used in the past. Using the TJRC, we are able to analyze information far more nuanced than whether a TC existed or did not.

We code four dichotomous indicator variables from the TJRC to capture increasingly robust TCs. All of these variables consider only TCs that have completed their work or disbanded after a period of operation; we do not consider ongoing commissions. The first variable covers all TCs that produced a final report. The second variable is restricted to those whose final report was made public and is reasonably accessible to the public. The other two variables further require that the public final report includes official recommendations for institutional reforms or prosecutions, respectively. The four binary variables are coded zero when the conditions are not met, and one for the first year when the conditions are fulfilled and all subsequent years. Thus, while we employ binary TC variables like previous studies, between them, they capture variation in the quality of TC implementation. For countries with multiple TCs, we code any country-year according to the most robust TC experience of a country to date.

### 4.4 Endogeneity

As in other social science research fields, selection effects are a thorny inferential problem in TJ impact studies.<sup>84</sup> A common approach in econometrics is instrumental variable regression, whereby an instrument is used to isolate exogenous variation in an otherwise endogenous variable. While this technique is well established, valid instruments are notoriously difficult to find because they must fulfill the exclusion restriction. This assumption, that the instrument only influences the outcome variable through the mediating endogenous variable, cannot be empirically tested, but must be theoretically supported.<sup>85</sup> While many instruments in human rights studies are questionable, we contend that the regional incidence of TCs is a reasonable instrument, because TCs are regionally clustered, suggesting diffusion processes.<sup>86</sup> However, there is no obvious reason why a given country

---

<sup>82</sup>Dancy et al. 2019; Dancy and Sikkink, *forthcoming*.

<sup>83</sup>See [www.transitionaljusticedata.com](http://www.transitionaljusticedata.com).

<sup>84</sup>Thoms, Ron, and Paris 2010.

<sup>85</sup>Sovey and Green 2011.

<sup>86</sup>Kim 2019.

should alter its democratic institutions or practices because neighbouring countries have had TCs. Our regional diffusion variable is based on whether countries within the geographic region have had any experience with TCs regardless of the quality of their implementation.<sup>87</sup>

## 4.5 Covariates

The analyses include several covariates to account for alternative explanations. Where relevant, these are lagged one year to ensure against reverse causation.

The models of democracy components and judicial reforms include Schnakenberg and Fariss's LPI as a covariate, to account for recent human rights conditions because violations tend to be “sticky”, such that states that violate human rights are likely to continue to do so in the future due to institutional inertia. The models of the LPI as an outcome variable do not include the lagged LPI. Moreover, all models include the cumulative mean of the LPI for a given country from 1949. This accounts for historical legacies of repression; countries that experienced longer and/or more severe periods of human rights violations are both more likely to establish TC and face greater challenges in altering institutions and practices.

The natural log of real gross domestic product (GDP) per capita (in constant US dollars) controls for a country's level of national wealth, because both democracy and human rights protection have often been found to be strongly correlated with economic wealth. Population size, also logged, accounts for possible challenges posed by larger countries to democratic governance and the greater opportunity to violate human rights in larger populations. These data come from the World Development Indicators database.<sup>88</sup>

An indicator for internal (including internationalized) armed conflict, coded from the UCDP/PRIO Armed Conflict Dataset, controls for threats to national security, which may undermine both democratic institutions and human rights protection. This variable distinguishes between low-level conflict (at least 25 battle-related deaths) and war (at least 1000 battle-related deaths).<sup>89</sup>

The V-Dem core civil society index is included because a robust civil society is likely a key driver of both the establishment of TCs and of our outcome variables.<sup>90</sup> The V-Dem index conceptualizes robust civil society as “one that enjoys autonomy from the state and in which citizens freely and actively pursue their political and civic goals, however conceived,” and combines indicators of government control over access to public life for civil society organizations, their repression by the state, and their participatory environment, which measures the extent of popular involvement in them.<sup>91</sup>

A variable indicating years since the last democratic transition, again based on the democracy data by Boix, Miller, and Rosato (2012), accounts for a couple of possible theoretical claims. First, TCs may be more likely to be established soon after a transition. Second, if democracy is slowly consolidated over time, higher levels of democratic institutions and practices should be observed later, regardless of the presence of a TC.

---

<sup>87</sup>However, the results are substantially unaltered when when this variable is based on the more restrictive TC variables discussed in Section 4.3 (not shown).

<sup>88</sup>World Bank 2018.

<sup>89</sup>Gleditsch, Wallensteen, Eriksson, Sollenberg, and Strand 2002; Themnér and Wallensteen 2014.

<sup>90</sup>Crocker 2000.

<sup>91</sup>Coppedge et al. 2018, 275.

Finally, we include indicators of the type of the last democratic transition by distinguishing negotiated or pacted transitions from those resulting from rupture, in order to address the possibility that certain types of transitions are more likely to involve TCs and influence democratic outcomes; the reference category are transitions due to a new state or no democratic transition (in the case of established democracies experiencing conflict).<sup>92</sup>Dancyetal2019 Like the TC variables, unless a new transition occurs, this variable is carried forward to subsequent country-years.

Summary statistics for all variables are provided in Appendix A.

## 4.6 Missing data and uncertainty of latent measures

Only two variables – GDP per capita and population size – are subject to a small fraction of missing data.<sup>93</sup> To address these missing data, to use as much of the available information in other variables, and to incorporate the estimated uncertainty of the latent V-Dem and LPI measures, we employ multiple imputation.<sup>94</sup>

## 4.7 Country heterogeneity

Since all outcome variables are continuous on the unit-interval, we employ linear regression models. The country-year structure of the data entails dependence of observations within countries over time. Quantitative studies commonly rely on fixed effects to control for potential unobserved (time-constant) country heterogeneity. If covariates are correlated with such unit effects, a standard linear regression model (such as OLS) can lead to poor fit and misleading estimates due to omitted variable bias.<sup>95</sup>

---

<sup>92</sup>Olsen, Payne, and Reiter 2010b.

<sup>93</sup>Three percent of observations in the sample have missing values for GDP per capita and less than one percent of observations in the case of population size.

<sup>94</sup>The multiple imputation approach assumes a multivariate normal distribution of measurement error in predicting missing from existing data to produce multiple datasets. The prediction model incorporates both country intercepts and polynomial time trends to account for dependence of observations within countries over time (Honaker and King 2010). Consistent with the assumptions of the measurement models producing the latent variables, we use the estimated means and standard deviations provided by their authors to draw their values for each imputed dataset from the normal distribution (Fariss 2014; Coppedge et al. 2018). Statistical estimates and standard errors from analyses of each imputed dataset are then aggregated to incorporate the additional uncertainty due to the imputations. All analyses are coded in the *R* statistical programming language, employing the *Amelia* and *plm* (R Core Team 2016; Honaker, King, and Blackwell 2011; Croissant and Millo 2008).

<sup>95</sup>By estimating only within-unit effects, fixed effects models allow for dependence between the unobserved country effects and the observed covariates. However, fixed effects models have high sample dependence, meaning they are overly sensitive to random error in a given dataset, and estimates of within unit effects may diverge from true effects, for instance when covariates vary little within units. In short, fixed effects models have increased variance of estimates due to the inclusion of a parameter for each unit. Random effects models do not estimate parameters for units but assume that the unit effects are drawn from a modelled distribution. These models reduce the variance of estimates, and have other practical advantages, but they introduce bias under certain conditions. Thus, there is a bias-variance trade-off between fixed effects and random effects models. To avoid bias, random effects models rely on the strong assumption of no correlation between covariates and the unit effects. A recent study employing simulations has found, however, that “even in the presence of rather extreme violations of that assumption, the random-effects estimator can still be preferable to (or at least no worse than) the fixed-effects estimator,” for estimating covariate effects in panel data. Clark and Linzer 2015, 407.

We implement both fixed effects and random effects models for each specification, and in many cases, both lead to the same substantive conclusions. Note that the fixed effects models average only the within-country effects of covariates, while the random effects models estimate the combination of within and between-country effects.

## 5 Results

We present results of bivariate differences-in-means and multivariate instrumental variable regressions. Here, we show only the effects of the four different TC variables in figures for comparison between the four TC variables and the five outcome variables. Complete multivariate regression results are provided in tables in Appendix C.

### 5.1 Differences-in-means

Before presenting our main analyses, we check bivariate relationships. The estimates of the differences-in-means in Figure 1 are derived from simple linear regression models of the outcome variables that include only the individual TC variables and do not address endogeneity or country heterogeneity. The differences-in-means are shown with 95% confidence intervals. Estimates that do not touch the zero-line are statistically significant. Since higher values of all the outcome variables indicate higher or “better” levels of the democratic components, judicial reforms, or physical integrity practices, a positive estimate indicates improvements and a negative estimate indicates worsening.

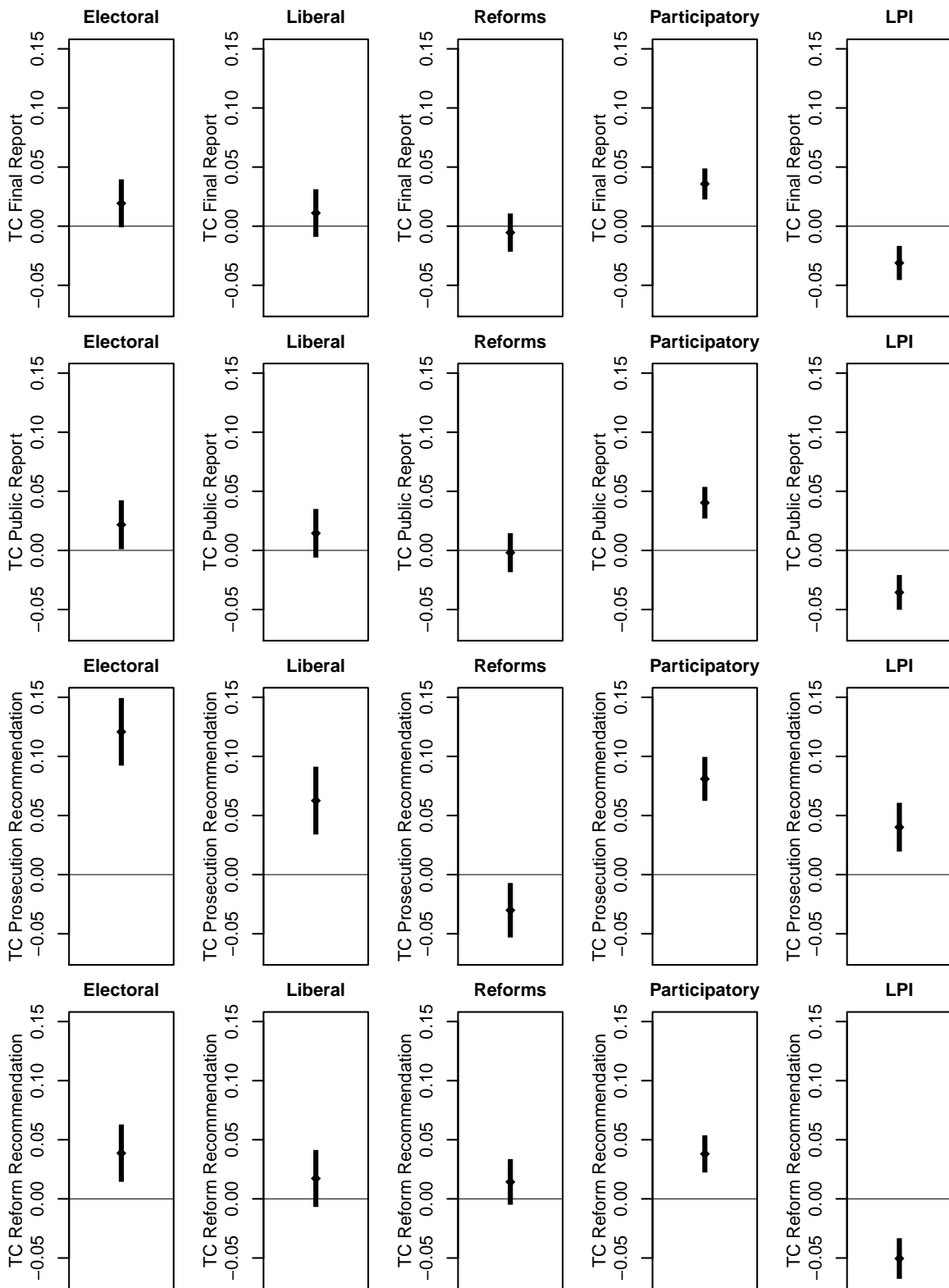
According to Figure 1, TCs which make their final reports public and those which make recommendations are associated with higher levels of electoral democracy. Those which do not make their final reports public do not have a statistically significant effect, providing support to the notion that the quality of TCs matters to outcomes. Only TCs which make their final reports publicly available and issue official recommendations for prosecutions of human rights violators are associated with the liberal conception of democracy, which is focused on the protection of rights against arbitrary actions by public officials. Considering the impact on reforms designed to improve accountability through the judiciary, again only TCs issuing public reports with recommendations for prosecutions matter, but the effect is opposite to important claims in the TJ literature: they are actually associated with decreased reform efforts.

Regardless of their robustness, all TC variables are associated with improved democratic participation. Finally, the findings regarding physical integrity rights practices are particularly interesting. Only TCs issuing public reports with recommendations for prosecutions are associated with improved human rights practices. All other TC variables are associated with worse physical integrity outcomes, lending support to the claim that TCs may provide “smoke screens” for renewed violations.

One should be very cautious inferring generalizations from these bivariate relationships, however, because the differences-in-means do not take into account alternative explanations nor do they model the endogeneity of TCs to democratic institutions and practices. In the remainder of the analyses, we show that addressing these inferential issues leads to quite different conclusions about the impacts of TCs, with some novel findings.



Figure 1: Differences-in-means



## 5.2 Instrumental variable models

The key results from the multivariate instrumental variable models are shown in Figure 2. In any given sub-figure the left-hand estimate is the fixed effects estimate, which indicates the average within-country effects, while the right-hand estimate is from the random effects model, which combines within and between country statistical effects. Estimates are again shown with 95% confidence intervals. We do not discuss the results with respect to covariates but note that they do not present any particularly surprising findings. For instance, the civil society index and negotiated transitions are most consistently associated with improvements in democracy components.

These models confirm some of the bivariate associations but contradict others in important ways. The first thing to note is that, surprisingly, the quality of TC implementation does not make a difference to the outcomes, as long as TCs complete their work and produce a final report.

According to the fixed effects estimates, TCs strengthen the electoral dimension of democracy, but this association is not supported by the random effects estimates. In a surprising finding, all estimates for effects on the liberal dimension of democracy are negative, and they are statistically significant in the case of the random effects models. The findings for the judicial reform variable are even stronger: regardless of the TC variable and of the estimator employed, TCs are associated with a reduction in the judiciary's ability to provide accountability for the arbitrary use of state power. This strongly contradicts the key claim in the TJ literature that TCs can encourage institutional reforms. Note, however, that the models of judicial reform account for relatively little of the total variation (3-8%), compared to the models of other outcome variables (see Appendix C). Still, the lack of effects on institutional rights protections is an important finding that contradicts a number of TJ claims.

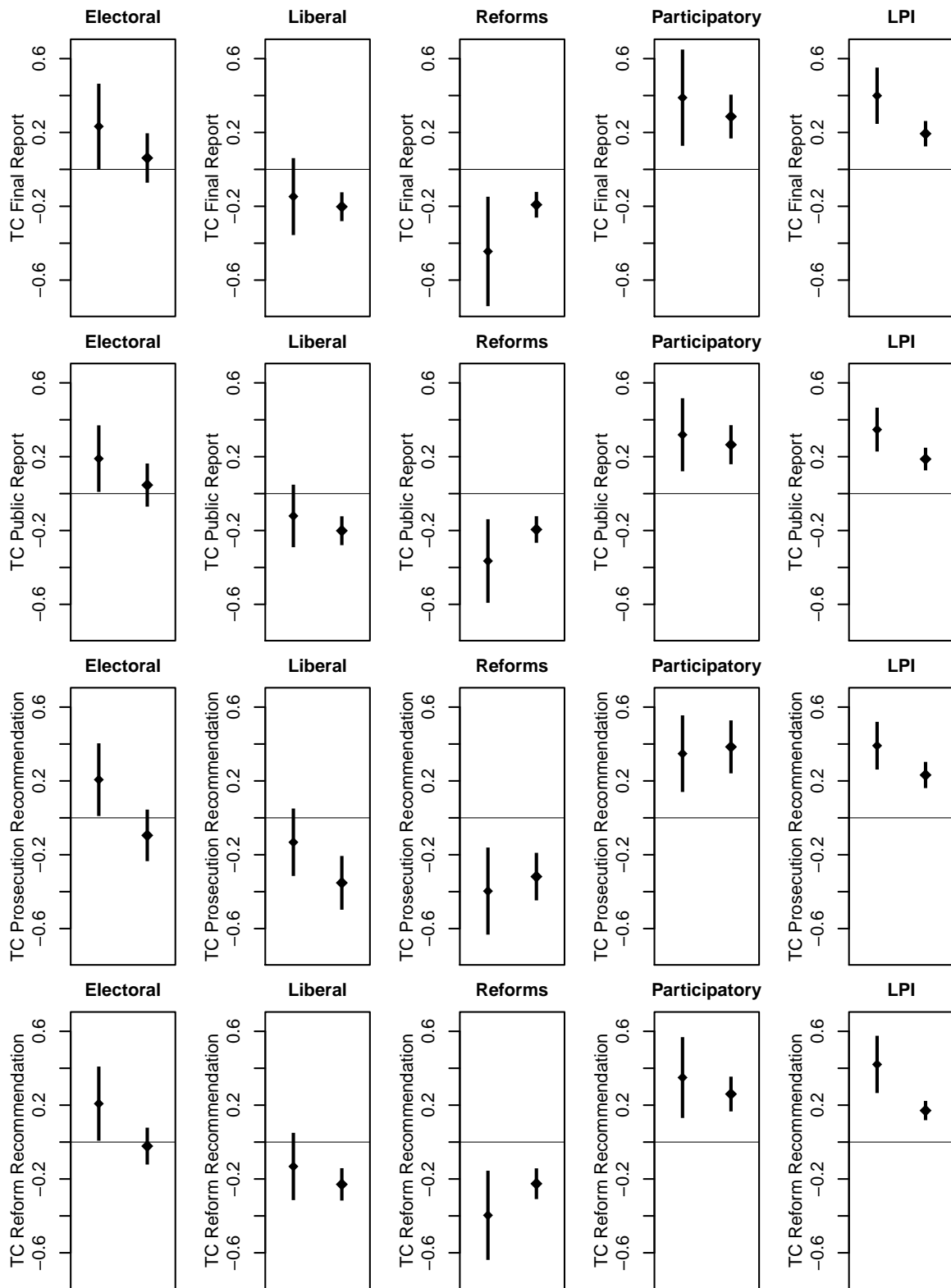
The final two columns in Figure 2 address democratic practices, finding consistent support for claims of beneficial impacts. The only variable for which the bivariate and instrumental multivariate analyses are in complete agreement, the participatory dimension of democracy is improved in the presence of completed TC processes. Finally, in contrast to the bivariate associations, physical integrity rights practices are also clearly improved after TCs have completed their work. We briefly note some implications of these findings in the final section.

## 6 Conclusion

In sum, we find that completed TC processes are associated with democratic practices but not with improvements in key accountability institutions. In fact, contrary to important TJ literature, they are associated with diminished institutions of liberal democracy and reduced judicial reform. We do not find any support for Hypotheses 2a and 2b, but we do find strong support for Hypotheses 3 and 4. We also suspect that TCs do not change attitudes, either, though we do not provide evidence for this; as discussed in Section 3, there is some suggestive case study research on this.

Thus, we do observe improved democratic and human rights practices, but not for the reasons often claimed. Moreover, as Thoms, Ron, and Paris (2010) argued, it may be that both TJ proponents and skeptics are partially right in their causal claims, because we find both positive and negative effects. That TCs may actually limit institutional change or function as “distractions” from reform is an important finding that deserves further attention by other researchers. The distinction between institutions and practices is novel in the TJ literature, which often assumes that the former mediate

Figure 2: Effects of truth commissions on democracy components



the latter. Like Bakiner (2014)'s qualitative evidence, our analyses raise doubts about this causal mechanism. We invite others to further disaggregate and test this and other pathways of TC influence.

There is still a lot of room for methodological innovation in empirical TJ impacts studies. We believe that the instrumental variable modelling in this paper is a sensible and productive approach to the key issue of TJ endogeneity, leading to some novel findings in light of the existing literature. However, good instruments are difficult to find and we would welcome other plausible instrumental variables or other methodological approaches to address this issue. Moreover, the focus on causal pathways invites other quantitative approaches, such as interaction effects and causal mediation analyses, in order to sort out which causal mechanisms in the TJ literature are actually at work.

## References

- Arenhövel, Mark. 2008. "Democratization and Transitional Justice." *Democratization* 15 (3): 570–587.
- Backer, David. 2009. "Cross-National Comparative Analysis." Chap. 2 in *Assessing the Impact of Transitional Justice: Challenges for Empirical Research*, ed. by Hugo van der Merwe, Victoria Baxter, and Audrey R. Chapman, 23–89. Washington, DC: United States Institute of Peace Press.
- . 2010. "Watching a Bargain Unravel? A Panel Study of Victims' Attitudes about Transitional Justice in Cape Town, South Africa." *International Journal of Transitional Justice* 4 (3): 443–456.
- Bakiner, Onur. 2014. "Truth Commission Impact: An Assessment of How Commissions Influence Politics and Society." *International Journal of Transitional Justice* 8 (1): 6–30.
- . 2015. "One truth among others? Truth commissions' struggle for truth and memory." *Memory Studies* 8 (3): 345–360.
- Bashir, Bashir. 2012. "Reconciling Historical Injustices: Deliberative Democracy and the Politics of Reconciliation." *Res Publica* 18 (2): 127–143.
- Boix, Carles, Michael Miller, and Sebastian Rosato. 2012. "A Complete Data Set of Political Regimes, 1800–2007." *Comparative Political Studies* 46 (12): 1523–1554.
- Borer, Tristan Anne, ed. 2006. *Telling the Truths: Truth Telling and Peace Building in Post-Conflict Societies*. Notre Dame, IN: University of Notre Dame Press.
- Brahm, Eric. 2007. "Uncovering the Truth: Examining Truth Commission Success and Impact." *International Studies Perspectives* 8 (1): 16–35.
- Chapman, Audrey R., and Patrick D. Ball. 2001. "The Truth of Truth Commissions: Comparative Lessons From Haiti, South Africa, and Guatemala." *Human Rights Quarterly* 23: 1–43.
- Clark, Ann Marie, and Kathryn Sikkink. 2013. "Information Effects and Human Rights Data: Is the Good News About Increased Human Rights Information Bad News for Human Rights Measures?" *Human Rights Quarterly* 35, no. 3 (Aug.): 539–568.
- Clark, Tom S., and Drew A. Linzer. 2015. "Should I Use Fixed or Random Effects?" *Political Science Research and Methods* 3, no. 2 (May): 399–408.

- Coppedge, Michael, John Gerring, Carl Henrik Knutsen, Staffan I. Lindberg, Svend-Erik Skaaning, Jan Teorell, David Altman, Michael Bernhard, Agnes Cornell, M. Steven Fish, Haakon Gjerløw, Adam Glynn, Allen Hicken, Joshua Krusell, Anna Luhrmann, Kyle L. Marquardt, Kelly McMann, Valeriya Mechkova, Moa Olin, Pamela Paxton, Daniel Pemstein, Brigitte Seim, Rachel Sigman, Jeffrey Staton, Aksel Sundtr om, Eitan Tzelgov, Luca Uberti, Yi-ting Wang, Tore Wig, and Daniel Ziblatt. 2018. *Varieties of Democracy (V-Dem) Project Codebook*. Version 8. <http://www.v-dem.net/>.
- Crocker, David A. 2000. "Truth Commissions, Transitional Justice, and Civil Society." In Rotberg and Thompson 2000, 99–121.
- Croissant, Yves, and Giovanni Millo. 2008. "Panel Data Econometrics in R: The plm Package." *Journal of Statistical Software* 27 (2): 1–43.
- Dahl, Robert. 1971. *Polyarchy: Participation and Opposition*. Yale University Press.
- Daly, Erin. 2008. "Truth Skepticism: An Inquiry into the Value of Truth in Times of Transition." *International Journal of Transitional Justice* 2 (1): 23–41.
- Dancy, Geoff, Hun Joon Kim, and Eric Wiebelhaus-Brahm. 2010. "The Turn to Truth: Trends in Truth Commission Experimentation." *Journal of Human Rights* 9 (1): 45–64.
- Dancy, Geoff, Bridget Marchesi, Tricia D. Olsen, Leigh A. Payne, Andrew G. Reiter, and Kathryn Sikkink. 2019. "Behind Bars and Bargains: New Findings on Transitional Justice in Emerging Democracies." *International Studies Quarterly* 63 (1): 99–110.
- Dancy, Geoff, and Florencia Montal. 2017. "From Law versus Politics to Law in Politics: A Pragmatist Assessment of the ICC's Impact." *American University International Law Review* 32 (3): 645–705.
- Dancy, Geoff, and Kathryn Sikkink. Forthcoming. "The Data of Transitional Justice." In *The Oxford Handbook of Transitional Justice*. Oxford University Press.
- de Brito, Alexandra Barahona, Carmen González Enríquez, and Paloma Aguilar, eds. 2001. *The Politics of Memory: Transitional Justice in Democratizing Societies*. Oxford, UK: Oxford University Press.
- de Greiff, Pablo. 2006. "Truth Telling and the Rule of Law." In Borer 2006, chap. 6.
- Dimitrijević, Nenad. 2006. "Justice beyond Blame: Moral Justification of (the Idea of) a Truth Commission." *Journal of Conflict Resolution* 50 (3): 368–382.
- Du Toit, Andre. 2000. "The Moral Foundations of the South African TRC: Truth as Acknowledgement and Justice as Recognition." In Rotberg and Thompson 2000, 122–140.
- Fariss, Christopher J. 2014. "Respect for Human Rights Has Improved Over Time: Modeling the Changing Standard of Accountability." *American Political Science Review* 108 (2): 297–318.
- Foweraker, Joe, and Roman Krznaric. 2000. "Measuring Liberal Democratic Performance: An Empirical and Conceptual Critique." *Political Studies* 48 (4): 759–787.
- Freeman, Mark. 2009. *Necessary Evils: Amnesties and the Search for Justice*. New York, NY: Cambridge University Press.

- Gairdner, David. 1999. "Truth in Transition: The Role of Truth Commissions in Political Transition in Chile and El Salvador." Working paper, Chr. Michelsen Institute of Development Studies and Human Rights, Bergen, Norway.
- Gibson, James L. 2004. "Does Truth Lead to Reconciliation? Testing the Causal Assumptions of the South African Truth and Reconciliation Process." *American Journal of Political Science* 48 (2): 201–217.
- . 2009. "On Legitimacy Theory and the Effectiveness of Truth Commissions." *Law and Contemporary Problems* 72: 123–141.
- Gleditsch, Nils Petter, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg, and Håvard Strand. 2002. "Armed Conflict 1946-2001: A New Dataset." *Journal of Peace Research* 39 (5): 615–637.
- Goldstone, Richard J. 1996. "Justice as a Tool for Peacemaking: Truth Commissions and International Criminal Tribunals." *New York University Journal of International Law and Politics* 28: 485–503.
- Graybill, Lyn, and Kimberly Lanegran. 2004. "Truth, Justice, and Reconciliation in Africa: Issues and Cases." *African Studies Quarterly* 8 (1): 1–18.
- Gutmann, Amy, and Dennis Thompson. 2000. "The Moral Foundations of Truth Commissions." In Rotberg and Thompson 2000, chap. 2.
- Hafner-Burton, Emilie M., and James Ron. 2009. "Seeing Double: Human Rights Impact Through Qualitative and Quantitative Eyes." *World Politics* 61 (2): 360–401.
- Hayner, Priscilla B. 2001. *Unspeakable Truths: Facing the Challenges of Truth Commissions*. New York, NY: Routledge.
- Hilbink, Lisa. 2012. "The Origins of Positive Judicial Independence." *World Politics* 64 (4): 587–621.
- Honaker, James, and Gary King. 2010. "What to Do About Missing Values in Time-Series Cross-Section Data." *American Journal of Political Science* 54 (2): 561–581.
- Honaker, James, Gary King, and Matthew Blackwell. 2011. "Amelia II: A Program for Missing Data." *Journal of Statistical Software* 45 (7): 1–47. <http://www.jstatsoft.org/v45/i07/>.
- Ishiyama, John, and Oluwagbemiso Laoye. 2016. "Do Truth Commissions Promote Trust in the Judiciary in African States?" *Journal of Asian and African Studies* 51 (5): 528–544.
- Jo, Hyeran, and Beth A. Simmons. 2016. "Can the International Criminal Court Deter Atrocity?" *International Organization* 70 (3): 443–475.
- Keck, Margaret E., and Kathryn Sikkink. 1998. *Activists Beyond Borders: Advocacy Networks in International Politics*. Ithaca, NY: Cornell University Press.
- Kenney, Charles D., and Dean E. Spears. 2005. "Truth and Consequences: Do Truth Commissions Promote Democratization?" Annual Meeting of the American Political Science Association. Washington, DC, Sept.
- Kim, Hun Joon. 2019. "Why Do States Adopt Truth Commissions After Transition?" *Social Science Quarterly* 100 (5): 1485–1502.
- Kim, Hun Joon, and Kathryn Sikkink. 2010. "Explaining the Deterrence Effect of Human Rights Prosecutions for Transitional Countries." *International Studies Quarterly* 54 (4): 939–963.

- Kritz, Neil J. 1996. "Coming to Terms With Atrocities: A Review of Accountability Mechanisms for Mass Violations of Human Rights." *Law and Contemporary Problems* 59: 127–152.
- Lie, Tove Grete, Helga Malmin Binningsbø, and Scott Gates. 2007. "Post-Conflict Justice and Sustainable Peace." World Bank Policy Research Working Paper 4191, Post-Conflict Transition Working Paper No. 5.
- Llewellyn, Jennifer. 2011. "Truth Commissions and Restorative Justice." In *Handbook of Restorative Justice*, ed. by Gerry Johnstone and Daniel Van Ness, 351–371. Routledge.
- Loyle, Cyanne E., and Christian Davenport. 2016. "Transitional Justice: Subverting Justice in Transition and Postconflict Societies." *Journal of Human Rights* 15 (1): 126–49.
- Mamdani, Mahmood. 2002. "Amnesty or Impunity? A Preliminary Critique of the Report of the Truth and Reconciliation Commission of South Africa (TRC)." *Diacritics* 32 (3–4): 33–59.
- Meierhenrich, Jens. 2008. "Varieties of Reconciliation." *Law & Social Inquiry* 33 (1): 195–231.
- Mendeloff, David. 2004. "Truth-Seeking, Truth-Telling, and Postconflict Peacebuilding: Curb the Enthusiasm?" *International Studies Review* 6 (3): 355–380.
- . 2009. "Trauma and Vengeance: Assessing the Psychological and Emotional Effects of Post-Conflict Justice." *Human Rights Quarterly* 31 (3): 592–623.
- Méndez, Juan E. 1997. "In Defense of Transitional Justice." Chap. 1 in *Transitional Justice and the Rule of Law in New Democracies*, ed. by James McAdams, 1–26. Notre Dame, IN: University of Notre Dame Press.
- Millar, Gearoid. 2011. "Local Evaluations of Justice through Truth Telling in Sierra Leone: Postwar Needs and Transitional Justice." *Human Rights Review* 12 (4): 515–535.
- Minow, Martha. 1998. *Between Vengeance and Forgiveness: Facing History After Genocide and Mass Violence*. Boston, MA: Beacon Press.
- Mouffe, Chantal. 2013. *Agonistics: Thinking The World Politically*. Verso.
- Munck, Gerardo L., and Jay Verkuilen. 2002. "Conceptualizing and Measuring Democracy: Evaluating Alternative Indices." *Comparative Political Studies* 35 (1): 5–34.
- O'Donnell, Guillermo, and Philippe C. Schmitter. 1986. "Transitions From Authoritarian Rule: Tentative Conclusions about Uncertain Democracies." In *Transitions from Authoritarian Rule*, ed. by Guillermo O'Donnell, Philippe C. Schmitter, and Laurence Whitehead, 28–36. Johns Hopkins University Press.
- Olsen, Tricia D., Leigh A. Payne, and Andrew G. Reiter. 2010a. "The Justice Balance: When Transitional Justice Improves Human Rights and Democracy." *Human Rights Quarterly* 32: 980–1007.
- . 2010b. *Transitional Justice in Balance: Comparing Processes, Weighing Efficacy*. Washington, DC: United States Institute of Peace Press.
- Olsen, Tricia D., Leigh A. Payne, Andrew G. Reiter, and Eric Wiebelhaus-Brahm. 2010. "When Truth Commissions Improve Human Rights." *International Journal of Transitional Justice* 4 (3): 457–476.

- Popkin, Margaret L., and Naomi Roht-Arriaza. 1995. "Truth as Justice: Investigatory Commissions in Latin America." *Law & Social Inquiry* 20 (1): 79–116.
- R Core Team. 2016. *R: A Language and Environment for Statistical Computing*. Comp. software. R Foundation for Statistical Computing, Vienna, Austria. <http://www.R-project.org/>.
- Rotberg, Robert I., and Dennis Thompson, eds. 2000. *Truth v. Justice: The Morality of Truth Commissions*. Princeton, NJ: Princeton University Press.
- Schnakenberg, Keith E., and Christopher J. Fariss. 2014. "Dynamic Patterns of Human Rights Practices." *Political Science Research and Methods* 2 (1): 1–31.
- Snyder, Jack L., and Leslie Vinjamuri. 2003. "Trials and Errors: Principle and Pragmatism in Strategies of International Justice." *International Security* 28 (3): 5–44.
- Sovey, Allison J., and Donald P. Green. 2011. "Instrumental Variables Estimation in Political Science: A Readers' Guide." *American Journal of Political Science* 55 (1): 188–200.
- Subotić, Jelena. 2009. *Hijacked Justice: Dealing With the Past in the Balkans*. Ithaca, NY: Cornell University Press.
- Taylor, Laura K., and Alexander Dukalskis. 2012. "Old Truths and New Politics: Does Truth Commission 'publicness' Impact Democratization." *Journal of Peace Research* 49 (5): 671–684.
- Teitel, Ruti G. 2000. *Transitional Justice*. New York, NY: Oxford University Press.
- Tepperman, Jonathan D. 2002. "Truth and Consequences." *Foreign Affairs* 81: 128–145.
- Themnér, Lotta, and Peter Wallensteen. 2014. "Armed Conflict, 1946-2013." *Journal of Peace Research* 51 (4): 541–554.
- Thoms, Oskar N. T., James Ron, and Roland Paris. 2010. "State-Level Effects of Transitional Justice: What Do We Know?" *International Journal of Transitional Justice* 4 (3): 329–354.
- Weiffen, Brigitte. 2012. "The Forgotten Factor: The Impact of Transitional Justice on the Development of the Rule of Law in Processes of Democratization." *Zeitschrift für Vergleichende Politikwissenschaft* 6 (S2): 125–147.
- Wiebelhaus-Brahm, Eric. 2010. *Truth Commissions and Transitional Societies: The Impact on Human Rights and Democracy*. New York, NY: Routledge.
- Wilson, Richard A. 2001. *The Politics of Truth and Reconciliation in South Africa: Legitimizing the Post-Apartheid State*. Cambridge, UK: Cambridge University Press.
- World Bank. 2018. *World Development Indicators*. World Bank. Washington, DC. <http://data.worldbank.org/data-catalog/world-development-indicators>.
- Zalaquett, Jose. 1992. "Balancing Ethical Imperatives and Political Constraints: The Dilemma of New Democracies Confronting Past Human Rights Violations." *Hastings Law Journal* 43 (6): 142–238.



## A Summary statistics

Table 2: Descriptive statistics, 2456 country-year observations

	min	max	mean	median
<b>Outcome variables</b>				
Electoral democracy index (Polyarchy)	0.07	0.95	0.59	0.61
Additive polyarchy index	0.14	0.98	0.77	0.81
Participatory democracy component index	0.09	0.88	0.52	0.55
Liberal democracy component index	0.02	0.96	0.66	0.71
Judicial constraints on executive	0.02	0.97	0.65	0.71
Judicial reform	0.01	1.00	0.56	0.56
Latent physical integrity	0.06	0.85	0.41	0.43
<b>Truth commission variables</b>				
ongoing TC	0	1	0.05	0
has had any TC	0	1	0.29	0
TC final report	0	1	0.22	0
TC public report	0	1	0.21	0
TC recommendations	0	1	0.17	0
TC recommendation to prosecute	0	1	0.09	0
TC recommendation to reform	0	1	0.14	0
regional TC diffusion (instrument)	0	45.71	17.77	13.95
<b>Independent variables</b>				
Latent physical integrity	0.06	0.85	0.41	0.43
Physical integrity cumulative mean	-2.90	2.73	-0.34	-0.44
GDP per capita (logged)	5.14	10.64	8.08	8.11
Population size (logged)	12.28	20.99	16.11	16.12
Internal armed conflict				
conflict: 405				
none: 1921				
war: 130				
Core Civil Society Index	0.06	0.98	0.75	0.82
Years since democratic transition	0	66	12.63	8.50
Transition type: negotiated	0	1	0.46	0
Transition type: ruptured	0	1	0.20	0
Transition type: new state	0	1	0.16	0
no transition type	0	1	0.19	0
<b>Other (for sample)</b>				
Regime type (BMR)				
democracy: 814				
new democracy: 1196				
nondemocracy: 446				

## **B Country-years included in sample**

Albania (1992-2015); Argentina (1973-2015); Bangladesh (1991-2015); Belarus (1991-2015); Benin (1991-2015); Bolivia (1979-2015); Bosnia & Herzegovina (1992-2015); Brazil (1985-2015); Bulgaria (1990-2015); Burundi (2005-2015); Cape Verde (1991-2015); Central African Republic (1993-2015); Chile (1990-2015); Colombia (1970-2015); Comoros (2006-2015); Croatia (2000-2015); Cyprus (1977-2015); Czechoslovakia (1990-1992) & Czech Republic (1993-2015); Ecuador (1979-2015); El Salvador (1984-2015); Estonia (1991-2015); Fiji (1970-2015); Gambia (1972-2015); Georgia (2004-2015); Ghana (1970-2015); Greece (1974-2015); Guatemala (1970-2015); Guinea-Bissau (1994-2015); Guyana (1992-2015); Honduras (1971-2015); Hungary (1990-2015); India (1970-2015); Indonesia (1999-2015); Israel (1970-2015); Kenya (2002-2015); Kosovo (2012-2015); Latvia (1993-2015); Lebanon (1971-2015); Lesotho (2002-2015); Liberia (2006-2015); Lithuania (1992-2015); Macedonia (1991-2015); Madagascar (1993-2015); Malawi (1994-2015); Maldives (2009-2015); Mali (1992-2015); Mexico (2000-2015); Moldova (1991-2015); Mongolia (1990-2015); Montenegro (2006-2015); Mozambique (1994-2015); Nepal (1991-2015); Nicaragua (1984-2015); Niger (1993-2015); Nigeria (1979-2015); Pakistan (1972-2015); Panama (1991-2015); Papua New Guinea (1975-2015); Paraguay (2003-2015); Peru (1980-2015); Philippines (1986-2015); Poland (1989-2015); Portugal (1976-2015); Romania (1991-2015); Russian Federation (1992-2015); Senegal (2000-2015); Sierra Leone (2002-2015); Slovakia (1993-2015); Slovenia (1992-2015); Solomon Islands (1978-2015); South Africa (1994-2015); South Korea (1988-2015); Spain (1977-2015); Sri Lanka (1971-2015); Sudan (1986-2010) & North Sudan (2011-2015); Suriname (1975-2015); Taiwan (1996-2015); Thailand (1975-2015); Timor-Leste (2002-2015); Trinidad & Tobago (1990-2015); Turkey (1983-2015); Uganda (1980-2015); Ukraine (1991-2015); United Kingdom (1971-2015); Uruguay (1972-2015); Venezuela (1982-2015); Yugoslavia FR (2000-2002) & Serbia & Montenegro (2003-2005) & Serbia (2006-2015); Zambia (2008-2015).

## **C Regression Tables**

Table 3: electoral

	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 4a	Model 4b
Latent physical integrity	-0.016 [0.118]	0.139 [0.075]	0.026 [0.094]	<b>0.154 [0.068]</b>	0.048 [0.085]	<b>0.263 [0.066]</b>	0.088 [0.070]	<b>0.207 [0.048]</b>
LPI cumulative mean	<b>0.294 [0.125]</b>	<b>0.219 [0.100]</b>	<b>0.295 [0.119]</b>	<b>0.216 [0.100]</b>	<b>0.405 [0.150]</b>	0.065 [0.109]	<b>0.424 [0.161]</b>	0.149 [0.105]
Population size	<b>-0.078 [0.034]</b>	-0.026 [0.017]	<b>-0.070 [0.029]</b>	-0.022 [0.016]	-0.026 [0.016]	-0.002 [0.008]	<b>-0.076 [0.032]</b>	-0.004 [0.011]
GDP per capita	<b>-0.068 [0.027]</b>	-0.023 [0.016]	<b>-0.048 [0.018]</b>	-0.016 [0.012]	<b>-0.048 [0.017]</b>	<b>0.021 [0.010]</b>	<b>-0.040 [0.015]</b>	0.004 [0.009]
Internal conflict	0.011 [0.010]	0.012 [0.009]	0.012 [0.009]	0.013 [0.008]	0.005 [0.010]	0.016 [0.009]	0.016 [0.009]	0.012 [0.008]
Internal war	0.001 [0.018]	0.018 [0.014]	0.007 [0.016]	0.020 [0.013]	0.008 [0.015]	<b>0.031 [0.014]</b>	0.012 [0.015]	<b>0.026 [0.013]</b>
Core civil society Index	<b>0.535 [0.045]</b>	<b>0.591 [0.031]</b>	<b>0.548 [0.038]</b>	<b>0.596 [0.029]</b>	<b>0.573 [0.028]</b>	<b>0.632 [0.024]</b>	<b>0.546 [0.039]</b>	<b>0.620 [0.026]</b>
negotiated transition	<b>0.076 [0.014]</b>	<b>0.070 [0.013]</b>	<b>0.088 [0.015]</b>	<b>0.072 [0.013]</b>	<b>0.073 [0.014]</b>	<b>0.060 [0.012]</b>	<b>0.108 [0.021]</b>	<b>0.061 [0.014]</b>
ruptured transition	-0.006 [0.035]	0.032 [0.024]	0.036 [0.020]	<b>0.043 [0.017]</b>	0.028 [0.021]	<b>0.046 [0.018]</b>	<b>0.062 [0.019]</b>	<b>0.041 [0.016]</b>
Years since transition	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>
TC final report	<b>0.233 [0.118]</b>	0.062 [0.068]						
TC public report			<b>0.190 [0.092]</b>	0.046 [0.060]				
TC recommend prosecution					<b>0.207 [0.100]</b>	-0.095 [0.071]		
TC recommend reform							<b>0.208 [0.103]</b>	-0.022 [0.051]
Intercept		0.539 [0.379]		0.421 [0.315]		-0.188 [0.145]		-0.027 [0.197]
n observations	2435	2435	2435	2435	2435	2435	2435	2435
n countries	88	88	88	88	88	88	88	88
years	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015
estimator	within	random	within	random	within	random	within	random
R-squared	0.457	0.564	0.503	0.571	0.508	0.56	0.493	0.572

estimates [standard errors]; significant at 0.05 alpha level in **bold**

Table 4: electoral (additive index)

	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 4a	Model 4b
Latent physical integrity	-0.002 [0.099]	0.091 [0.065]	0.031 [0.079]	0.104 [0.058]	0.047 [0.071]	<b>0.164 [0.056]</b>	0.082 [0.057]	<b>0.150 [0.043]</b>
LPI cumulative mean	<b>0.275 [0.100]</b>	<b>0.220 [0.087]</b>	<b>0.270 [0.094]</b>	<b>0.217 [0.086]</b>	<b>0.361 [0.118]</b>	0.137 [0.090]	<b>0.372 [0.122]</b>	<b>0.189 [0.090]</b>
Population size	-0.034 [0.028]	-0.008 [0.015]	-0.028 [0.024]	-0.006 [0.014]	0.006 [0.014]	0.005 [0.007]	-0.032 [0.026]	0.002 [0.010]
GDP per capita	<b>-0.059 [0.022]</b>	<b>-0.028 [0.014]</b>	<b>-0.044 [0.015]</b>	-0.021 [0.011]	<b>-0.044 [0.015]</b>	0.005 [0.008]	<b>-0.038 [0.013]</b>	-0.005 [0.008]
Internal conflict	0.009 [0.008]	0.009 [0.007]	0.010 [0.007]	0.010 [0.007]	0.004 [0.008]	0.010 [0.007]	0.013 [0.008]	0.011 [0.007]
Internal war	-0.002 [0.015]	0.008 [0.012]	0.002 [0.013]	0.010 [0.011]	0.003 [0.013]	0.015 [0.011]	0.006 [0.012]	0.014 [0.011]
Core civil society Index	<b>0.529 [0.035]</b>	<b>0.564 [0.024]</b>	<b>0.539 [0.029]</b>	<b>0.568 [0.023]</b>	<b>0.558 [0.022]</b>	<b>0.593 [0.018]</b>	<b>0.537 [0.030]</b>	<b>0.582 [0.021]</b>
negotiated transition	<b>0.093 [0.012]</b>	<b>0.088 [0.012]</b>	<b>0.103 [0.013]</b>	<b>0.091 [0.011]</b>	<b>0.091 [0.012]</b>	<b>0.075 [0.011]</b>	<b>0.118 [0.017]</b>	<b>0.085 [0.012]</b>
ruptured transition	0.013 [0.030]	0.034 [0.021]	<b>0.045 [0.017]</b>	<b>0.047 [0.015]</b>	<b>0.039 [0.019]</b>	<b>0.042 [0.015]</b>	<b>0.065 [0.015]</b>	<b>0.049 [0.014]</b>
Years since transition	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]
TC final report	0.177 [0.094]	0.078 [0.056]						
TC public report			<b>0.145 [0.074]</b>	0.065 [0.048]				
TC recommend prosecution					<b>0.158 [0.080]</b>	0.002 [0.057]		
TC recommend reform							<b>0.159 [0.081]</b>	0.028 [0.042]
Intercept		0.522 [0.327]		0.427 [0.272]	0.044 [0.122]			0.166 [0.173]
n observations	2435	2435	2435	2435	2435	2435	2435	2435
n countries	88	88	88	88	88	88	88	88
years	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015
estimator	within	random	within	random	within	random	within	random
R-squared	0.524	0.598	0.562	0.608	0.56	0.621	0.561	0.615

estimates [standard errors]; significant at 0.05 alpha level in **bold**

Table 5: liberal

	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 4a	Model 4b
Latent physical integrity	<b>0.245 [0.113]</b>	<b>0.316 [0.064]</b>	<b>0.221 [0.096]</b>	<b>0.313 [0.065]</b>	<b>0.207 [0.085]</b>	<b>0.368 [0.075]</b>	<b>0.178 [0.068]</b>	<b>0.238 [0.053]</b>
LPI cumulative mean	0.212 [0.119]	0.055 [0.096]	0.210 [0.117]	0.040 [0.097]	0.137 [0.137]	-0.115 [0.117]	0.125 [0.141]	-0.034 [0.100]
Population size	-0.009 [0.033]	0.015 [0.008]	-0.014 [0.030]	0.015 [0.008]	<b>-0.042 [0.018]</b>	-0.004 [0.008]	-0.010 [0.031]	0.011 [0.008]
GDP per capita	0.023 [0.025]	<b>0.035 [0.008]</b>	0.011 [0.018]	<b>0.029 [0.008]</b>	0.011 [0.018]	<b>0.044 [0.009]</b>	0.006 [0.015]	<b>0.031 [0.008]</b>
Internal conflict	0.003 [0.010]	0.005 [0.010]	0.002 [0.009]	0.004 [0.010]	0.006 [0.010]	0.014 [0.010]	-0.001 [0.009]	-0.002 [0.010]
Internal war	0.010 [0.018]	0.016 [0.016]	0.007 [0.017]	0.015 [0.016]	0.006 [0.016]	0.022 [0.017]	0.003 [0.015]	0.010 [0.015]
Core civil society Index	<b>0.620 [0.041]</b>	<b>0.642 [0.025]</b>	<b>0.612 [0.035]</b>	<b>0.642 [0.025]</b>	<b>0.596 [0.027]</b>	<b>0.640 [0.026]</b>	<b>0.614 [0.036]</b>	<b>0.652 [0.025]</b>
negotiated transition	<b>0.055 [0.015]</b>	<b>0.041 [0.014]</b>	<b>0.047 [0.016]</b>	<b>0.030 [0.014]</b>	<b>0.057 [0.015]</b>	<b>0.041 [0.015]</b>	0.035 [0.021]	0.011 [0.014]
ruptured transition	<b>0.074 [0.035]</b>	<b>0.060 [0.020]</b>	<b>0.047 [0.022]</b>	0.033 [0.018]	<b>0.052 [0.024]</b>	<b>0.052 [0.021]</b>	0.030 [0.020]	0.013 [0.017]
Years since transition	0.001 [0.000]	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>	0.001 [0.000]	<b>0.001 [0.000]</b>	<b>0.001 [0.000]</b>
TC final report	-0.147 [0.106]	<b>-0.202 [0.040]</b>						
TC public report			-0.121 [0.086]	<b>-0.201 [0.040]</b>				
TC recommend prosecution					-0.132 [0.093]			
TC recommend reform						<b>-0.352 [0.074]</b>		
Intercept		<b>-0.473 [0.160]</b>		<b>-0.416 [0.155]</b>		-0.212 [0.148]	-0.132 [0.093]	<b>-0.229 [0.045]</b>
n observations	2435	2435	2435	2435	2435	2435	2435	2435
n countries	88	88	88	88	88	88	88	88
years	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015
estimator	within	random	within	random	within	random	within	random
R-squared	0.409	0.428	0.42	0.422	0.438	0.369	0.439	0.444

estimates [standard errors]; significant at 0.05 alpha level in **bold**

Table 6: reform

	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 4a	Model 4b
Latent physical integrity	<b>0.421 [0.153]</b>	<b>0.202 [0.061]</b>	<b>0.340 [0.117]</b>	<b>0.200 [0.060]</b>	<b>0.299 [0.103]</b>	<b>0.230 [0.068]</b>	<b>0.212 [0.085]</b>	<b>0.127 [0.053]</b>
LPI cumulative mean	-0.303 [0.171]	<b>-0.231 [0.100]</b>	-0.286 [0.155]	<b>-0.239 [0.102]</b>	<b>-0.505 [0.187]</b>	<b>-0.337 [0.118]</b>	<b>-0.530 [0.190]</b>	<b>-0.284 [0.103]</b>
Population size	0.070 [0.045]	0.006 [0.007]	0.056 [0.039]	0.007 [0.007]	-0.030 [0.020]	-0.009 [0.007]	0.065 [0.040]	0.004 [0.007]
GDP per capita	<b>0.078 [0.036]</b>	-0.003 [0.007]	0.041 [0.023]	-0.007 [0.007]	0.040 [0.022]	0.006 [0.008]	0.025 [0.019]	-0.005 [0.006]
Internal conflict	0.002 [0.013]	0.000 [0.010]	-0.001 [0.012]	-0.001 [0.010]	0.012 [0.012]	0.006 [0.010]	-0.010 [0.011]	-0.007 [0.010]
Internal war	<b>0.056 [0.025]</b>	0.028 [0.016]	<b>0.045 [0.021]</b>	0.028 [0.016]	<b>0.043 [0.020]</b>	0.031 [0.016]	0.035 [0.019]	0.022 [0.016]
Core civil society Index	<b>0.359 [0.064]</b>	<b>0.279 [0.031]</b>	<b>0.335 [0.054]</b>	<b>0.280 [0.032]</b>	<b>0.287 [0.041]</b>	<b>0.274 [0.032]</b>	<b>0.340 [0.055]</b>	<b>0.291 [0.033]</b>
negotiated transition	0.006 [0.022]	0.010 [0.015]	-0.017 [0.022]	0.001 [0.015]	0.012 [0.019]	0.011 [0.015]	-0.055 [0.029]	-0.015 [0.015]
ruptured transition	<b>0.095 [0.044]</b>	0.024 [0.020]	0.015 [0.026]	0.002 [0.019]	0.030 [0.026]	0.017 [0.020]	-0.035 [0.026]	-0.014 [0.018]
Years since transition	<b>-0.001 [0.001]</b>	0.000 [0.000]	<b>-0.001 [0.000]</b>	0.000 [0.000]	-0.001 [0.000]	-0.001 [0.000]	-0.001 [0.000]	-0.001 [0.000]
TC final report	<b>-0.445 [0.151]</b>	<b>-0.192 [0.036]</b>						
TC public report			<b>-0.365 [0.116]</b>	<b>-0.194 [0.037]</b>				
TC recommend prosecution					<b>-0.397 [0.120]</b>	<b>-0.318 [0.066]</b>		
TC recommend reform							<b>-0.397 [0.123]</b>	<b>-0.226 [0.042]</b>
Intercept		<b>0.318 [0.134]</b>		<b>0.343 [0.135]</b>		<b>0.504 [0.128]</b>		<b>0.413 [0.122]</b>
n observations	2435	2435	2435	2435	2435	2435	2435	2435
n countries	88	88	88	88	88	88	88	88
years	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015
estimator	within	random	within	random	within	random	within	random
R-squared	0.039	0.08	0.037	0.072	0.032	0.058	0.032	0.082

estimates [standard errors]; significant at 0.05 alpha level in **bold**

Table 7: participatory

	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 4a	Model 4b
Latent physical integrity	<b>-0.298 [0.127]</b>	<b>-0.211 [0.074]</b>	<b>-0.229 [0.097]</b>	<b>-0.187 [0.068]</b>	<b>-0.194 [0.083]</b>	<b>-0.208 [0.072]</b>	<b>-0.119 [0.069]</b>	<b>-0.072 [0.049]</b>
LPI cumulative mean	<b>0.431 [0.153]</b>	<b>0.400 [0.121]</b>	<b>0.424 [0.136]</b>	<b>0.408 [0.116]</b>	<b>0.615 [0.165]</b>	<b>0.575 [0.132]</b>	<b>0.636 [0.173]</b>	<b>0.501 [0.116]</b>
Population size	-0.005 [0.038]	0.000 [0.014]	0.008 [0.032]	0.002 [0.013]	<b>0.083 [0.017]</b>	<b>0.025 [0.008]</b>	-0.002 [0.035]	0.006 [0.009]
GDP per capita	-0.057 [0.033]	-0.006 [0.015]	-0.024 [0.022]	0.009 [0.012]	-0.024 [0.021]	0.013 [0.009]	-0.011 [0.018]	<b>0.023 [0.009]</b>
Internal conflict	0.013 [0.012]	0.013 [0.010]	0.015 [0.011]	0.014 [0.010]	0.003 [0.011]	0.004 [0.010]	<b>0.022 [0.010]</b>	<b>0.021 [0.010]</b>
Internal war	-0.015 [0.020]	-0.005 [0.015]	-0.006 [0.017]	-0.001 [0.015]	-0.005 [0.016]	-0.004 [0.015]	0.003 [0.016]	0.008 [0.014]
Core civil society Index	<b>0.119 [0.053]</b>	<b>0.164 [0.031]</b>	<b>0.140 [0.044]</b>	<b>0.169 [0.030]</b>	<b>0.182 [0.032]</b>	<b>0.196 [0.026]</b>	<b>0.135 [0.045]</b>	<b>0.177 [0.027]</b>
negotiated transition	<b>0.089 [0.018]</b>	<b>0.081 [0.015]</b>	<b>0.110 [0.017]</b>	<b>0.098 [0.014]</b>	<b>0.085 [0.015]</b>	<b>0.072 [0.015]</b>	<b>0.143 [0.023]</b>	<b>0.111 [0.014]</b>
ruptured transition	0.004 [0.041]	0.030 [0.025]	<b>0.074 [0.022]</b>	<b>0.076 [0.019]</b>	<b>0.060 [0.023]</b>	<b>0.053 [0.020]</b>	<b>0.118 [0.020]</b>	<b>0.101 [0.016]</b>
Years since transition	0.001 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]
TC final report	<b>0.388 [0.133]</b>	<b>0.286 [0.061]</b>						
TC public report			<b>0.319 [0.101]</b>	<b>0.265 [0.054]</b>				
TC recommend prosecution					<b>0.348 [0.106]</b>	<b>0.384 [0.073]</b>		
TC recommend reform							<b>0.350 [0.112]</b>	<b>0.260 [0.048]</b>
Intercept		0.276 [0.298]		0.100 [0.256]		<b>-0.341 [0.144]</b>		<b>-0.166 [0.161]</b>
n observations	2435	2435	2435	2435	2435	2435	2435	2435
n countries	88	88	88	88	88	88	88	88
years	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015
estimator	within	random	within	random	within	random	within	random
R-squared	0.081	0.173	0.122	0.193	0.176	0.199	0.114	0.217

estimates [standard errors]; significant at 0.05 alpha level in **bold**

Table 8: LPI

	Model 1a	Model 1b	Model 2a	Model 2b	Model 3a	Model 3b	Model 4a	Model 4b
LPI cumulative mean	<b>0.582 [0.106]</b>	<b>0.722 [0.062]</b>	<b>0.649 [0.083]</b>	<b>0.743 [0.058]</b>	<b>0.899 [0.072]</b>	<b>0.809 [0.047]</b>	<b>1.035 [0.090]</b>	<b>0.859 [0.052]</b>
Population size	<b>-0.102 [0.027]</b>	<b>-0.033 [0.010]</b>	<b>-0.093 [0.022]</b>	<b>-0.033 [0.009]</b>	-0.012 [0.013]	-0.008 [0.004]	<b>-0.113 [0.028]</b>	<b>-0.019 [0.005]</b>
GDP per capita	<b>-0.069 [0.020]</b>	-0.010 [0.008]	<b>-0.038 [0.013]</b>	-0.002 [0.007]	<b>-0.038 [0.013]</b>	0.002 [0.005]	<b>-0.026 [0.013]</b>	0.009 [0.005]
Internal conflict	<b>-0.047 [0.009]</b>	<b>-0.056 [0.007]</b>	<b>-0.048 [0.008]</b>	<b>-0.056 [0.007]</b>	<b>-0.062 [0.008]</b>	<b>-0.062 [0.007]</b>	<b>-0.045 [0.009]</b>	<b>-0.057 [0.007]</b>
Internal war	<b>-0.104 [0.014]</b>	<b>-0.102 [0.011]</b>	<b>-0.101 [0.012]</b>	<b>-0.100 [0.010]</b>	<b>-0.104 [0.012]</b>	<b>-0.101 [0.010]</b>	<b>-0.104 [0.013]</b>	<b>-0.102 [0.010]</b>
Core civil society Index	-0.052 [0.037]	<b>0.040 [0.020]</b>	-0.030 [0.030]	<b>0.042 [0.018]</b>	0.017 [0.023]	<b>0.066 [0.016]</b>	-0.036 [0.033]	<b>0.060 [0.017]</b>
negotiated transition	0.020 [0.014]	0.016 [0.010]	<b>0.043 [0.013]</b>	<b>0.028 [0.010]</b>	0.016 [0.012]	0.017 [0.009]	<b>0.087 [0.019]</b>	<b>0.040 [0.009]</b>
ruptured transition	<b>-0.082 [0.027]</b>	-0.027 [0.015]	-0.010 [0.017]	0.003 [0.013]	-0.026 [0.017]	0.001 [0.012]	<b>0.041 [0.017]</b>	<b>0.026 [0.011]</b>
Years since transition	<b>0.001 [0.000]</b>	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]
TC final report	<b>0.399 [0.078]</b>	<b>0.193 [0.035]</b>						
TC public report			<b>0.347 [0.061]</b>	<b>0.187 [0.031]</b>				
TC recommend prosecution					<b>0.391 [0.066]</b>	<b>0.232 [0.036]</b>		
TC recommend reform							<b>0.421 [0.079]</b>	<b>0.171 [0.027]</b>
Intercept		<b>0.686 [0.215]</b>		<b>0.604 [0.182]</b>		<b>0.157 [0.080]</b>		<b>0.249 [0.094]</b>
n observations	2435	2435	2435	2435	2435	2435	2435	2435
n countries	88	88	88	88	88	88	88	88
years	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015	1971-2015
estimator	within	random	within	random	within	random	within	random
R-squared	0.192	0.394	0.233	0.408	0.247	0.459	0.167	0.444

estimates [standard errors]; significant at 0.05 alpha level in **bold**