

July 2017

The Quantitative Turn in Transitional Justice Research: What Have We Learned About Impact?

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Recommended Citation

Stewart, Brandon and Wiebelhaus-Brahm, Eric (2017) "The Quantitative Turn in Transitional Justice Research: What Have We Learned About Impact?," *Transitional Justice Review*: Vol. 1 : Iss. 5 , Article 4.

Available at: <https://ir.lib.uwo.ca/tjreview/vol1/iss5/4>

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Transitional Justice Review, Vol.1, Iss.5, 2017, 97-133

The Quantitative Turn in Transitional Justice Research: What Have We Learned About Impact?

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Introduction

In recent years, scholars have increasingly turned to quantitative research methods to understand the impact of transitional justice on societies emerging from periods of violence and repression. The term transitional justice (TJ) has been used to describe a range of tools that countries use to address past (and increasingly contemporary) human rights abuses. With some variation, observers often distinguish at least seven different types of transitional justice mechanisms: prosecutions, truth commissions, vetting procedures, reparations, memorialization, traditional practices, and institutional reform.¹ Although some would dispute labeling them a TJ mechanism, amnesties are another policy tool with clear societal implications. To date, quantitative studies of TJ impact have focused primarily on trials, truth commissions, and amnesties. These are the mechanisms that are most frequently discussed in policy debates as to how to build lasting peace in post-conflict societies. They often have a higher profile and possess higher stakes for political elites. Arguably, these TJ mechanisms also are the easiest to identify due to media attention

¹ M. Cherif Bassiouni and Daniel Rothenberg. *The Chicago Principles on Post-Conflict Justice*. (Chicago: The International Human Rights Law Institute, DePaul University, 2008).

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and the legal trail they leave behind. Hence, they are the most amenable to constructing large datasets.

The growth in quantitative scholarship is a response to dissatisfaction with much of the early qualitative transitional justice literature. Several observers have noted the need for basing claims about the impact of transitional justice on more solid empirical foundations.² In the early qualitative literature, many judgments about the value of TJ were based upon impressionistic conclusions and anecdotal evidence. In other cases, authors made claims based upon legal or moral arguments rather than empirical evidence. Moreover, much of this early literature was written by TJ practitioners who had an inherent interest in seeing a particular outcome to the research.³ The quantitative research seeks to address these shortcomings by assembling large datasets of TJ events that span space and time and employing ostensibly more objective, methodologically rigorous research designs.

Existing empirical quantitative research often treats TJ mechanisms as independent variables and assesses their impact on dependent variables such as human rights, democracy, and peace duration. Much of this quantitative literature makes bold policy recommendations. Yet, the first wave of research offers relatively little clear guidance to governments and societies emerging from periods of conflict and repression, intergovernmental organizations and global civil society that frequently help devise TJ plans, or

² Eric Brahm, "Uncovering the Truth: Examining Truth Commission Success and Impact," *International Studies Perspectives* 8.1 (2007): 16-35; David Mendeloff, "Deterrence, Norm Socialization, and the Empirical Reach of Kathryn Sikkink's The Justice Cascade: How Human Rights Prosecutions Are Changing World Politics," *Journal of Human Rights* 11.2 (2012): 289-295; Hugo van der Merwe, Victoria Baxter, and Audrey R. Chapman, eds., *Assessing the Impact of Transitional Justice* (Washington, DC: United States Institute of Peace, 2009); Oskar N.T. Thoms, James Ron, and Roland Paris, "State-Level Effects of Transitional Justice: What Do We Know?" *International Journal of Transitional Justice* 4.3 (2010): 1-26.

³ Michael Ben-Josef Hirsch, Megan Mackenzie, and Mohamed Sesay, "Measuring the Impacts of Truth and Reconciliation Commissions: Placing the Global 'Success' of TRCs in Local Perspective," *Cooperation and Conflict* 47.3 (2012): 386-403.

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governments and private donors in the Global North that often fund these initiatives. Part of the problem involves explaining statistical findings to a constituency that may have little to no statistical training. For those more statistically inclined, however, methodological challenges are apparent. Quantifying key concepts and devising statistical models that provide a fair assessment of potential cause-effect relationships is difficult. This has led some observers to conclude that even the most advanced quantitative research leaves much to be desired.⁴ Perhaps the greatest obstacle to quantitative research having a greater impact on TJ policy discussions is that, collectively, the policy advice is inconsistent if not downright contradictory. What are policymakers and activists to make of this messiness?

In this article, we critically review the first wave of this emerging quantitative literature that examines the impact of TJ mechanisms. We find divergence on several methodological issues that help to explain the different conclusions reached by these studies, including sampling strategies, model construction, and the measurement of key variables. Furthermore, these studies have often failed to provide compelling theoretical or empirical bases for a causal relationship between TJ mechanisms and dependent variables such as democracy and human rights. We suggest several ways in which quantitative scholars could produce findings with broader credibility. Although we support the use of quantitative methods to understand the impact of TJ mechanisms, greater methodological care is needed in supporting policy recommendations.

Reviewing the Findings of the “First Wave” Quantitative Literature

In the past decade, there has been a rapid expansion in the amount of transitional justice research that employs quantitative research methods. Some research treats transitional justice as the dependent

⁴ Anna MacDonald, “From the Ground Up: What Does the Evidence Tell Us about Local Experiences of Transitional Justice,” *Transitional Justice Review* 1.3 (2015): 72-121; Thoms et al, “State-Level Effects of Transitional Justice,” 1-26.

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variable, others as the independent variable. Within the latter literature that explores the effects of TJ, one can roughly divide research based on the unit of analysis. Some quantitative research focuses on the individual level, examining data on the reaction to TJ by victims or mass publics and how TJ processes have shaped individual attitudes toward other groups, human rights, democracy, and the like.⁵ In this article, we concern ourselves with quantitative research that examines the impact of TJ at the national, or societal, level.

In our review, we focus on the first wave of the quantitative TJ literature, which ended in roughly 2011. As we shall illustrate, this research has looked at a variety of TJ mechanisms and examined their impact on a range of societal dimensions. We focus on studies that examine the effect of TJ on human rights, democracy, and peace duration, the three most common dependent variables in the quantitative literature. Furthermore, we consider only those studies that have been published in peer-reviewed outlets or that have been released for attribution by their authors. As such, our review underrepresents the use of quantitative methods as we do not include the many conference papers and doctoral dissertations that were produced during this first wave. In short, we review the most prominent early studies, most of which are the product of major database initiatives. Collectively, this emerging body of research does not provide decisive policy prescriptions. In fact, there is a lack of consensus as to whether or not particular TJ policies generally produce benefits for society. It is likely that the methodological challenges partially responsible for this divergence also are part of the

⁵ David Backer, "Victims' Responses to Truth Commissions: Evidence from South Africa" in M. Ndulo, ed., *Security, Reconstruction, and Reconciliation: When the Wars End* (London: University College London, 2007); James Gibson, "The Contributions of Truth to Reconciliation," *Journal of Conflict Resolution* 50.3 (2006): 409-432; James L. Gibson, "The Legacy of Apartheid: Racial Differences in the Legitimacy of Democratic Institutions and Processes in the New South Africa," *Comparative Political Studies* 36.7 (2003): 772-800; James L. Gibson, *Overcoming Apartheid: Can Truth Reconcile a Divided Nation?* (New York: Russell Sage Foundation, 2004).

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reason why more of this research has yet to appear in peer-reviewed outlets.

Surveying the Findings

Several studies have reached pessimistic conclusions regarding the effects of particular types of TJ mechanisms. Wiebelhaus-Brahm, for example, focuses specifically on truth commissions.⁶ Building his truth commission dataset from Hayner, the United States Institute of Peace, and news reports, he explores their role in promoting human rights and democracy.⁷ Using a global sample of countries over the years from 1980 to 2006, he finds that truth commissions have a negative effect on human rights and no effect on democracy. Wiebelhaus-Brahm utilizes a multi-method research design that seeks to gain the best of quantitative and qualitative approaches. Unfortunately, his case study findings are not entirely consistent with his statistical results, leaving the reader to wonder whether his cases are not representative of truth commissions in general or if there are weaknesses in the statistical model.

Melander focuses on amnesties, which many see as an obstacle to justice.⁸ Specifically, he assesses whether amnesty provisions in peace agreements signed between 1989 and 2005 make peace more or less durable. He finds that amnesties reduce the risk of renewed fighting in authoritarian regimes because, he argues, the greater stability and insulation provided to autocrats make them more immune to pressures to renege on the amnesty. By contrast, amnesties do not have a pacifying effect in democracies or regimes in transition. In either scenario, the results are not positive on normative grounds: either justice is foregone or armed conflict resumes.

⁶ Eric Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies: The Impact on Human Rights and Democracy* (New York: Routledge, 2010).

⁷ Priscilla B. Hayner, *Unspeakable Truths: Confronting State Terror and Atrocity* (New York: Routledge, 2001).

⁸ Erik Melander, "Justice or Peace? A Statistical Study of the Relationship between Amnesties and Durable Peace," *JAD-PbP Working Paper No. 4*: 2009.

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Meernik, Nichols, and King consider the effects of prosecuting human rights violators before domestic and international courts.⁹ They focus on examining the effect of prosecution on human rights practices and the durability of peace in countries that emerged from civil war between 1982 and 2007. Their findings do little to support the strong claims of either trial supporters or critics. In fact, they conclude that countries that experience trials are no more or less likely to experience a recurrence of intrastate conflict or improvements in human rights practices compared to countries in which prosecutions have not occurred. According to Meernik et al., trials do not risk renewed violence, but neither do they necessarily cement peace or promote human rights.

Other research looks at a combination of TJ mechanisms. Olsen, Payne, and Reiter, for instance, explore the consequences of trials, truth commissions, and amnesties for subsequent human rights practices and democratization processes in countries transitioning to democracy between 1970 and 2004.¹⁰ In examining the individual and interactive effects of these three TJ policies, they reach interesting conclusions. First, they find that trials or amnesties alone do not have a statistically significant effect on improving human rights or democracy. Second, in line with Wiebelhaus-Brahm, they find that truth commissions on their own have a negative effect on human rights.¹¹ Third, only when trials and amnesties are used together do they have a positive effect on human rights and democracy. They conclude that this approach is effective by balancing demands for accountability with perpetrators' needs for security. In those situations where both trials and amnesties exist, they find that truth commissions do not have a negative effect.

⁹ James D. Meernik, Angela Nichols, and Kimi L. King, "The Impact of International Tribunals and Domestic Trials on Peace and Human Rights after Civil War," *International Studies Perspectives*, 11.3 (2010): 309-334.

¹⁰ Tricia D. Olsen, Leigh A. Payne, and Andrew G. Reiter, *Transitional Justice in Balance: Comparing Processes, Weighing Efficacy* (Washington, D.C.: United States Institute of Peace Press, 2010).

¹¹ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

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Of the quantitative studies we examine, Lie, Binningsbø, and Gates offer the most expansive study in terms of TJ-related mechanisms included.¹² They examine trials, purges, reparations to victims, truth commissions, amnesties, and the use of exile. They consider the effects of these mechanisms on the likelihood of conflict recurrence in countries in which a civil war, as defined by Uppsala-PRIO's armed conflict dataset, ended between 1946 and 2003. Overall, they conclude that TJ processes have a weak and inconsistent effect on the durability of peace. Trials appear to be the most important type of justice, though the effect of trials is sensitive to the mode of conflict termination. The other interesting finding is that amnesty reduces peace duration, but, as with trials, the effect of this variable also is sensitive to conflict termination, indicating that conflict termination clearly affects the prospect for justice as well as post-conflict peace duration.

By contrast, other research finds that transitional justice has a clearer positive benefit for societies emerging from conflict or repression. Kim and Sikkink test the alleged deterrent effect of prosecuting human rights violators.¹³ In other words, can trials promote human rights? They include a range of trial processes, including those conducted by domestic courts; in foreign courts under universal jurisdiction principles; and international courts, which encompass the International Criminal Court, ad-hoc international tribunals, and hybrid courts. They examine the effect of trials on human rights with a sample of countries that have undergone a transition from authoritarian rule, civil war, or state creation between 1974 and 2004. According to Kim and Sikkink, "the mechanisms through which transitional justice measures influence human rights do not only involve a calculation of the possibility of punishment, but

¹² Tove Grete Lie, Helga Malmin Binningsbø and Scott Gates, "Post-Conflict Justice and Sustainable Peace," *World Bank Policy Research Working Paper 4191, Post-Conflict Transitions Working Paper No. 5*: 2007.

¹³ Hunjoon Kim and Kathryn Sikkink, "Explaining the Deterrence Effect of Human Rights Prosecutions for Transitional Countries," *International Studies Quarterly* 54.4 (2010): 939–963.

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also respond to processes that provide information and communicate norms.”¹⁴ By this line of reasoning, human rights trials and truth commissions help to spread a norm of accountability that makes human rights abuses socially atrocious; as a result, violations decline. Building on this logic, they code a dichotomous variable for trial activity in a given year. Their data source, US State Department human rights reports, does not permit measuring the number of trials that take place so they simply measure the presence or absence of trial activity in a given year. Moreover, they do not distinguish between whether the target of prosecution is a high-level official or someone from the lower ranks. Although they devote relatively little attention to them theoretically, the article also explores whether truth commissions have a deterrent effect. Contrary to Wiebelhaus-Brahm¹⁵ and Olsen et al.¹⁶, they find that both truth commissions and human rights prosecutions have a positive impact on lessening repression. Moreover, countries that have had more years of prosecutions have better human rights practices than countries with fewer.

Summing It Up

Collectively, the aforementioned research offers convoluted policy prescriptions. In terms of amnesties, Lie et al.¹⁷ and Melander¹⁸ argue they promote peace if implemented by authoritarian regimes; the reverse is true for democratic countries. At the same time, according to Olsen et al.¹⁹, amnesties promote human rights and democracy, though only if balanced with trials. With respect to trials, the answer truly is that “it depends”. Kim and Sikkink²⁰ are optimistic about the

¹⁴ Kim and Sikkink, “Explaining the Deterrence Effect,” 953-954; also see Kathryn Sikkink, *The Justice Cascade: How Human Rights Prosecutions are Changing World Politics* (New York: W. W. Norton & Co, 2011).

¹⁵ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

¹⁶ Olsen et al., *Transitional Justice in Balance*.

¹⁷ Lie et al, “Post-Conflict Justice.”

¹⁸ Melander, “Justice or Peace?”

¹⁹ Olsen et al., *Transitional Justice in Balance*.

²⁰ Kim and Sikkink, “Explaining the Deterrence Effect.”

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ability of trials to promote human rights, while Lie et al. and Meernik et al.²¹ suggest we should not hope for too much. Olsen et al. tell us that democracy and human rights will advance furthest where trials are balanced by amnesties. Finally, the truth commission conclusions are the most obviously contradictory. While Kim and Sikkink argue they promote human rights, Wiebelhaus-Brahm²² and Olsen et al. find the opposite.

Making Sense of the Divergent Findings

Several factors help to explain these divergent findings. We highlight three major issues that are the result of using quantitative methods. The first issue relates to definition and measurement of key concepts. In particular, studies have defined transitional justice mechanisms in different ways, resulting in a lack of consistency on what cases are included. As we have seen, these studies also vary in what dependent variable they use. While they may be concerned with different areas of impact, more general conclusions about the utility of particular TJ mechanisms may implicitly follow. Second, these studies have drawn their samples in different ways. Some look at cases of democratic transition, others post-civil war, or both. The time period covered also varies considerably. As a result, the TJ and non-TJ cases in the studies' samples are not necessarily the same. In other words, the cases upon which these causal claims are tested are not identical across studies. Finally, scholars have constructed their statistical models in different ways. It is important to explore whether the models chosen are appropriate for testing causal claims and whether they have done a reasonable job of addressing concerns such as endogeneity and omitted variable bias.

Definitional and Measurement Issues

TRANSITIONAL JUSTICE MECHANISMS

In general, statistical research has treated transitional justice mechanisms as dummy variables. In other words, they have been

²¹ Meernik et al., "The Impact of International Tribunals."

²² Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

coded as ones and zeroes to reflect the presence or absence of a TJ mechanism. One can reasonably question whether it is appropriate to treat all trials (or truth commissions, amnesties, etc.) as being the same for theory testing. Trials certainly vary on many dimensions, including how many trials occur; who is on trial; whether the process is free and fair; and whether a conviction is reached. Truth commissions vary in terms of the scope of the investigative task they were given; the powers that were granted to compel cooperation; the makeup of the commissioners; and whether they produce a final report, to cite a few important differences. Among other things, amnesties differ in terms of who is eligible; and whether recipients are required to do anything as a precondition for receiving the amnesty. As we shall see, several studies under review try to address some of this diversity. Nonetheless, these steps are unlikely to satisfy some qualitative scholars, particularly those who view each TJ process as *sui generis*.

Within the quantitative literature, there are significant differences in how trial processes are measured. Lie et al.²³, for instance, do not provide explicit criteria for case selection. For Olsen, et al.²⁴, a trial is coded only when a verdict is reached. They hypothesize that outcome, rather than process, is critical for any impact of trials on democracy and human rights. At the same time, they include in their sample trials of a diversity of suspects, including armed rebel groups; state agents fighting civil wars, conducting counterinsurgency operations, or engaging in government repression; and domestic political opposition groups (including those participating in coup attempts). This is different from Kim and Sikkink, who view process, rather than outcome, as important. They code for indictments, arrests, extraditions, detentions and trials under the umbrella category of human rights prosecutions. Moreover, Kim and Sikkink's²⁵ sample, unlike Olsen et al., is limited to *government agents* accused of human rights violations. As long as the prosecution

²³ Lie et al, "Post-Conflict Justice."

²⁴ Olsen et al., *Transitional Justice in Balance*, 32.

²⁵ Kim and Sikkink, "Explaining the Deterrence Effect," 948.

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activity (as outlined in US State Department and Amnesty International reports) inflicts a cost on a *government agent* accused of human rights violations, it is coded. Their definition of cost includes the financial costs of legislation, lost income during preventative detention, as well as the loss of prestige and legitimacy that accompanies trials.²⁶ One commonality between these studies is that they lump domestic and international prosecutions into a single trial variable. Meernik et al.²⁷ adopt Kim and Sikkink's definition, but treat domestic prosecution and international courts as separate variables.

Overall, Kim and Sikkink's²⁸ sampling strategy leads to a much smaller sample compared to Olsen et al.²⁹ As illustrated in Appendix A, Olsen et al. have 81 countries with trials in their sample, while Kim and Sikkink have only 48. While Kim and Sikkink's definition is much broader in regards to what court activity constitutes prosecutorial activity because they determine their sample using the cost-inflicted standard as opposed to the verdict-only standard of Olsen et al., the latter include the trials of a broader range of actors. These contrasting sampling strategies lead to a very different sample base. In total, the Kim and Sikkink and the Olsen et al. samples have only 36 trial countries in common. Moreover, because of the different sampling strategies, the number of trial-years included in both datasets is very different.

Determining which sampling strategy is most beneficial for testing trials as an independent variable depends in part on theoretical judgments about whether the impact of trials serves as a deterrent or as a norm generator. Coding only verdicts presumes that trial activity in and of itself does not have an impact on attitudes and behaviors. As such, by coding for verdicts only, Olsen et al.³⁰ fail to include several high-profile cases in which a verdict was not reached due to the death of the defendant (Slobodan Milosevic and Augusto Pinochet, for example). Kim and Sikkink argue that their approach

²⁶ Kim and Sikkink, "Explaining the Deterrence Effect," 942.

²⁷ Meernik et al., "The Impact of International Tribunals."

²⁸ Kim and Sikkink, "Explaining the Deterrence Effect."

²⁹ Olsen et al., *Transitional Justice in Balance*.

³⁰ Olsen et al., *Transitional Justice in Balance*.

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indicates that trial activity deters future rights abuses through norm diffusion.³¹ Yet, it is difficult to determine if and to what extent trials genuinely help promote norm diffusion. As Mendeloff argues, the causal logic of normative-based arguments is problematic because it assumes that the presence of human rights prosecutions (indictments, arrests, trials, etc.) is a legitimate expression of the norm.³² Human rights trials may themselves be unjust depending on the manner in which they are conducted. In terms of actors subject to trials, Kim and Sikkink assume that human rights are promoted primarily through the prosecution of state agents. It is unclear how these types of trials would impact the behavior of non-state actors. By contrast, Olsen et al.'s inclusion of coup plotters introduces a range of trial processes not necessarily associated with TJ. More generally, neither study adequately accounts for differences in the quality of trial activity. However, these somewhat subjective judgments would be difficult to make for a large number of cases.

It is particularly difficult to test normative explanations for quantitative findings, because how can we know that repression is reduced because of burgeoning human rights norms? While Kim and Sikkink's findings offer an important counter-weight to some of the other TJ literature, the theoretical explanations for their empirical findings are extremely difficult, if not impossible, to empirically test.³³ By contrast, Olsen et al. may underappreciate the influence of trial processes.³⁴ Indeed, both the concepts of deterrence and norm diffusion (as well as norm creation) are complex processes that call for more research; because of this, definitional choices should be firmly rooted in theory.

Several of the studies under review address the impact of truth commissions. At least implicitly, they generally adopt Hayner's definition of truth commissions as "bodies set up to investigate a past history of violations of human rights in a particular country – which

³¹ Kim and Sikkink, "Explaining the Deterrence Effect."

³² David Mendeloff, "Deterrence, Norm Socialization and the Empirical Reach."

³³ Kim and Sikkink, "Explaining the Deterrence Effect."

³⁴ Olsen et al., *Transitional Justice in Balance*.

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can include violations by the military or other government forces or armed opposition forces.”³⁵ Elsewhere, Hayner elaborates on this basic definition to emphasize that truth commissions are temporary bodies officially sanctioned by the state to examine a pattern of human rights abuses over a broad period of time.³⁶ However, definitional challenges plague the comparative study of truth commissions. Given ambiguity in the meaning of key attributes like official sanction, whether the commissions need to be newly established, and what constitutes a pattern of abuse and a broad period of time, studies contain very different lists of truth commission cases. Brahm’s survey of cross-national studies found vastly different lists of truth commission cases (see Appendix B for a comparison of cases in select studies).³⁷

The studies under review vary significantly in how they treat truth commissions. Kim and Sikkink include truth commissions, but do not define or explain their criteria for including cases.³⁸ Lie et al. do not even provide a list of cases.³⁹ Olsen et al. use Hayner’s definition of truth commissions; they define a truth commission as a “newly established, temporary body officially sanctioned by a state or an international governmental organization to investigate past human rights abuses.”⁴⁰ As is evidenced by Appendix B, however, Olsen et al. include many cases as truth commissions that other studies, including Hayner’s, do not. Olsen et al. have 68 truth commissions in their data, whereas Wiebelhaus-Brahm, for example, has 28 in his models.⁴¹ While not necessarily evidence they are wrong to do so, Olson et al. code for several truth commissions that others appear to have judged as different types of mechanisms. Some, such as Brazil’s

³⁵ Priscilla B. Hayner, “Fifteen Truth Commissions - 1974 to 1994: A Comparative Study,” *Human Rights Quarterly* 16.4 (1994): 597-655.

³⁶ Hayner, *Unspeakable Truths*, 14.

³⁷ Eric Brahm, “What is a Truth Commission and Why Does it Matter?” *Peace and Conflict Review* 3.2 (2009): 1-14.

³⁸ Kim and Sikkink, “Explaining the Deterrent Effect.”

³⁹ Lie et al., “Post-Conflict Justice.”

⁴⁰ Hayner, *Unspeakable Truths*, 34.

⁴¹ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

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1995-2007 commission, did not appear to have undertaken significant investigations of abuses; rather, it processed claims by victims.⁴² Furthermore, Hayner's definition includes the stipulation that the truth commission "investigate a pattern of abuses over a period of time, rather than a specific event."⁴³ Olsen et al. do not always follow their sampling criterion, as they code for some commissions that examine specific events rather than a pattern of abuses.⁴⁴ For instance, they include the Investigating Commission on the Kidnapping and Assassination of National Representatives Zelmar Michelini and Hector Gutierrez Ruiz in Uruguay. We are not suggesting that more narrowly-construed investigations cannot influence such things as human rights and democracy. However, there has yet to be a systematic study of different types of investigative commissions that explores whether the type of investigation matters for different outcomes.

Although in a fairly limited way, some of the studies attempt to account for qualitative differences in truth commissions. Olsen et al., for example, include any truth commission that begins its work.⁴⁵ Wiebelhaus-Brahm, by contrast, adds the stipulation that the commission must make recommendations to redress past abuses and to prevent such abuses from occurring again in the future.⁴⁶ In one model, he examines the effect of a commission's operation by only coding those years in which the commission existed. He also measures the years following the publication of a truth commission's final report to gauge longer term effects. Finally, in order to get at another qualitative difference, on the assumption that final reports are critical for their impact, he includes only cases where reports were

⁴² G. Mezarobba, "Between Reparations, Half Truths and Impunity: The Difficult Break with the Legacy of the Dictatorship in Brazil," *Sur: International Journal on Human Rights* 7.13 (2010): 7-26.

⁴³ Hayner, *Unspeakable Truths*, 14.

⁴⁴ Olsen et al., *Transitional Justice in Balance*.

⁴⁵ Olsen et al., *Transitional Justice in Balance*.

⁴⁶ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

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publicly released. He finds that the impact of truth commissions is magnified if the final report is made available.⁴⁷

Furthermore, the studies that examine amnesties also fail to make strong qualitative distinctions. Lie et al.⁴⁸ and Olsen et al.⁴⁹ treat amnesties as dummy variables. Melander only considers amnesty provisions that are part of formal peace agreements.⁵⁰ Otherwise, he also does not make qualitative distinctions among amnesties. Yet, we know that there is tremendous diversity among amnesties.⁵¹

Finally, Lie et al.⁵² and Olsen et al.⁵³ experiment with aggregating TJ experiences into one variable. For example, aside from treating the TJ mechanisms in their model separately, Lie et al. also create an additive index to distinguish where between 0 and 4 mechanisms were used. In other words, a country in which trials, purges, a truth commission, and reparations have been conducted would be scored a 4, whereas a country that experienced trials and purges and another country that experienced a truth commission and reparations would each be coded a 2. Theoretically, it is difficult to make a compelling argument that these latter two TJ configurations are identical. By contrast, Olsen et al. construct an ordered variable to measure the extent to which accountability has been achieved. To do this, countries are scored to reflect how far along an accountability continuum they have moved, where amnesty=1, truth commission=2, and trial=3. While arguably useful if accountability is the most desirable dimension of TJ, other criteria might lead us to rank mechanisms differently. Moreover, measuring in this way prevents us from examining how the timing and sequencing of these mechanisms may shape outcomes. In general, aggregating TJ measures does not seem to be a very promising avenue as it requires

⁴⁷ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*, 139, 159.

⁴⁸ Lie et al., "Post-Conflict Justice."

⁴⁹ Olsen et al., *Transitional Justice in Balance*.

⁵⁰ Melander, "Justice or Peace?"

⁵¹ Louise Mallinder, *Amnesty, Human Rights and Political Transitions: Bridging the Peace and Justice Divide* (Portland, OR: Hart Publishing, 2008).

⁵² Lie et al., "Post-Conflict Justice."

⁵³ Olsen et al., *Transitional Justice in Balance*.

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making questionable assumptions about ranking and aggregation and generally obscures valuable qualitative information.

DEPENDENT VARIABLES

Comparatively speaking, there is greater consensus on the measures used to judge the impact of transitional justice. Kim and Sikkink⁵⁴, Wiebelhaus-Brahm⁵⁵, and Olsen et al.⁵⁶, for example, all use Cingranelli and Richards' (CIRI) measure of human rights, which is focused on physical integrity rights. Wiebelhaus-Brahm and Olsen et al. also use the Political Terror Scale (PTS) as a robustness check. Both CIRI and PTS provide countries' human rights scores based upon annual human rights reports produced by the US State Department and Amnesty International. CIRI provides a yearly evaluation of countries' protection of a range of different human rights, namely the rights to freedom from extra-judicial killing, disappearance, torture, and political imprisonment. In addition, the dataset contains an index of these items that measures the general protection of physical integrity rights.⁵⁷ By contrast, the PTS provides a five-point measure of the degree to which physical integrity rights are protected by the government.⁵⁸ The PTS reports two human rights scores, corresponding to each of the annual human rights reports used as a reference. The PTS has been criticized for

⁵⁴ Kim and Sikkink, "Explaining the Deterrent Effect."

⁵⁵ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

⁵⁶ Olsen et al., *Transitional Justice in Balance*.

⁵⁷ In each of these four areas, a country's behavior is coded as follows: 0 = practice reported 50 or more times in the year; 1 = practice reported between 1 and 49 times in the year; and 2 = practice not reported in the year. To construct the physical integrity rights score, Mokken Scaling Analysis is used, resulting in a measure that varies between 0 and 8. For more on CIRI's coding, see David L. Cingranelli and David L. Richards, *The Cingranelli-Richards Human Rights Dataset Version 2008.03.12*, Available: <http://www.humanrightsdata.org> [Accessed April 25 2009].

⁵⁸ Mark Gibney, Linda Cornett, and Reed Wood, *Political Terror Scale 1976-2007*, Available at <http://www.politicalterror scale.org> (Accessed April 25 2009)

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attempting to force into one measure a multidimensional concept.⁵⁹ Moreover, the scores are based on qualitative judgments of the prevalence of state-sanctioned violence rather than on a strict count as CIRI uses. Theoretically, both measures are useful in measuring potential deterrent effects. They are less effective, however, in judging the impact of TJ on attitudes toward human rights that fall short of observable behavior. Moreover, both measures rely on organizations that may be biased in their reporting.⁶⁰

Wiebelhaus-Brahm and Olsen et al.⁶¹ also are interested in the effect on democratization. Wiebelhaus-Brahm focuses on Freedom House's Civil Liberties measure, which he argues best measures the aspects of democracy that truth commissions are most likely to influence.⁶² Olsen et al., by contrast, provide little explicit reflection on measurement choices and whether they reasonably measure effects of TJ. Rather, they take an inclusive approach and include Polity's measure⁶³ as well as Freedom House's Civil Liberties and

⁵⁹ James M. McCormick and Neil J. Mitchell, "Human Rights Violations, Umbrella Concepts, and Empirical Analysis," *World Politics* 49.4 (1997): 510-525; David L. Cingranelli and David L. Richards, "Measuring the Level, Pattern, and Sequence of Government Respect for Physical Integrity Rights," *International Studies Quarterly* 43.2 (1999): 407-417.

⁶⁰ For a review of some of the issues with quantitative human rights measures, see Todd Landman, "Measuring Human Rights: Principle, Practice and Policy," *Human Rights Quarterly* 26.4 (2009): 906-931.; Steven C. Poe, Sabine C. Carey and Tanya C. Vazquez, "How are These Pictures Different? A Quantitative Comparison of the US State Department and Amnesty International Human Rights Reports, 1976-1995," *Human Rights Quarterly* 23.3 (2001): 650-677.; Reed M. Wood and Mark Gibney, "The Political Terror Scale (PTS): A Re-introduction and a Comparison to CIRI," *Human Rights Quarterly* 32.2 (2010): 367-400."

⁶¹ Olsen et al., *Transitional Justice in Balance*.

⁶² Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*, 25.

⁶³ Polity IV bases its democracy measure on: Executive Recruitment (3.1 XRREG Regulation of Chief Executive Recruitment; 3.2 XRCOMP Competitiveness of Executive Recruitment; 3.3 XROPEN Openness of Executive Recruitment); The Independence of Executive Authority (3.4 XCONST Executive Constraints (Decision Rules)); and Political Competition and Opposition (3.5 PARREG Regulation of Participation; 3.6 PARCOMP The Competitiveness of Participation).

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Political Rights scores.⁶⁴ In reality, the causal links between any form of TJ and the aspects of democracy measured by these datasets are likely to involve one or more intervening variables. Theoretically, therefore, making causal arguments regarding TJ impact on these aspects of democracy is problematic. However, other aspects that might be more relevant, such as public attitudes about perpetrators, the extent to which voters cross group boundaries, or attitudes toward members of other groups in society are not widely available, at least not in a form amenable to cross-national research.

⁶⁴ Freedom House “ratings process is based on a checklist of 10 political rights questions and 15 civil liberties questions. The political rights questions are grouped into three subcategories: Electoral Process (3 questions), Political Pluralism and Participation (4), and Functioning of Government (3). The civil liberties questions are grouped into four subcategories: Freedom of Expression and Belief (4 questions), Associational and Organizational Rights (3), Rule of Law (4), and Personal Autonomy and Individual Rights (4). Scores are awarded to each of these questions on a scale of 0 to 4, where a score of 0 represents the smallest degree and 4 the greatest degree of rights or liberties present. The political rights section also contains two additional discretionary questions: question A (For traditional monarchies that have no parties or electoral process, does the system provide for genuine, meaningful consultation with the people, encourage public discussion of policy choices, and allow the right to petition the ruler?) and question B (Is the government or occupying power deliberately changing the ethnic composition of a country or territory so as to destroy a culture or tip the political balance in favor of another group?). For additional discretionary question A, a score of 1 to 4 may be added, as applicable, while for discretionary question B, a score of 1 to 4 may be subtracted (the worse the situation, the more that may be subtracted). The highest score that can be awarded to the political rights checklist is 40 (or a total score of 4 for each of the 10 questions). The highest score that can be awarded to the civil liberties checklist is 60 (or a total score of 4 for each of the 15 questions). The scores from the previous survey edition are used as a benchmark for the current year under review. In general, a score is changed only if there has been a real world development during the year that warrants a change (e.g., a crackdown on the media, the country’s first free and fair elections) and is reflected accordingly in the narrative. In answering both the political rights and civil liberties questions, Freedom House does not equate constitutional or other legal guarantees of rights with the on-the-ground fulfillment of these rights. While both laws and actual practices are factored into the ratings decisions, greater emphasis is placed on the latter.” (from <http://www.freedomhouse.org/report/freedom-world-2012/methodology>).

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Finally, both Lie et al.⁶⁵ and Melander⁶⁶ use Uppsala-PRIO data to measure the length of the post-conflict peace period. This dataset marks the presence of violent conflict when fighting crosses a 25 battle-related death threshold. Using a 25 battle-death threshold is an advance over traditional ways of measuring wars as 1,000 battle-related deaths. However, this criterion overlooks other types of violence, such as acts of government repression, acts of terrorism, domestic violence, or criminal violence that may be evidence of TJ impact (or lack thereof).

Sampling strategies

Consumers of this emerging quantitative research would be well-served by paying careful attention to how these scholars craft their sample because it has clear implications for the scope of their findings. Chronological coverage is limited by data availability for the dependent variable. As a result, studies that assess the impact on human rights or democracy are generally limited to focusing only on the period since the 1970s. Conflict data are not so limited. Furthermore, scholars vary as to whether they seek to contribute to discussions about states transitioning to democracy, post-conflict societies, or both. Wiebelhaus-Brahm tries to avoid selection effects by using a global sample that includes countries that did not experience civil conflict or an attempted transition from authoritarian rule to democracy.⁶⁷ By doing so, however, he includes many extraneous cases and potentially dilutes his findings.

Given their focus on whether TJ increases the risk of conflict resumption, Lie et al.⁶⁸ and Melander⁶⁹ rightly focus only on post-conflict countries. Both further limit their sample. Melander, for example, looks only at the durability of peace following formal peace agreements. As a result, his findings should be read with this more limited applicability in mind. In addition, both he and Lie et al.

⁶⁵ Lie et al., "Post-Conflict Justice."

⁶⁶ Melander, "Justice or Peace?"

⁶⁷ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

⁶⁸ Lie et al., "Post-Conflict Justice."

⁶⁹ Melander, "Justice or Peace?"

require at least two years of non-violence between conflict years before the country would enter the sample. While not unreasonable, the choice reduces the study's ability to say something about the dynamics in the immediate aftermath of conflict termination. While Lie et al. use Uppsala/PRIO data to determine their sample of post-conflict societies, it is possible that their number of post-conflict societies is inflated because the Uppsala/PRIO data has conflict dyads of governments and an armed opposition group as its unit of analysis. This means that if a government is at war with several different factions, each of these conflicts may be coded as a separate conflict dyad. Given the fact that they do not report their data, it is difficult to ascertain how this issue is addressed. Meernik et al., by contrast, do not suffer from the problem of multiple dyads, as they measure the presence or absence of internal war in a given state in a given year.⁷⁰

Another illustration of the significance of sampling can be seen by comparing the findings of Meernik et al.⁷¹ and Kim and Sikkink. Although they define human rights trials according to the same parameters, Meernik et al.'s results are not as optimistic. Rather, they find that domestic human rights trials and international tribunals after civil war have had no effect on subsequent human rights practices. How do we explain the contrasting findings of Meernik et al. and Kim and Sikkink, when both use the same definition for human rights trials? For one, the sampling strategies for transitional societies are different. Kim and Sikkink⁷² consider countries going through three types of transitions: from autocracy to democracy, from war to peace, and transition by state creation. Meernik et al., by contrast, focus exclusively on post-civil war trials. Even beyond that, there is a major difference in the number of post-civil war states in the two studies' samples. Kim and Sikkink determine their sample by using the Polity IV data set; they code 16 states as transitioning from civil war. Meernik et al., using Uppsala/PRIO, find a total of 82 post-

⁷⁰ Meernik et al., "The Impact of International Tribunals."

⁷¹ Meernik et al., "The Impact of International Tribunals."

⁷² Kim and Sikkink, "Explaining the Deterrent Effect," 178.

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civil war states. The much higher number of post-civil war states in Meernik et al. is due to the fact that Uppsala/PRIO's definition is one in which there are at least 25 battle deaths in a dyadic conflict in which at least one actor is a state actor. As a result of using what this dataset defines as a civil war, several states that had very short periods of civil war (e.g., Kenya, 20 days; Moldova, 7 months; Lesotho, 2 ½ months) are included in their sample of post-civil war states (see Appendix C for comparative post-civil war data).

The much smaller number of post-civil war transitions in the Kim and Sikkink⁷³ data can be addressed in part because some of the states that they code as having a transition by state creation (Azerbaijan, Eritrea, Macedonia, Croatia, Moldova, etc.) also had civil wars after the new state emerged. While Kim and Sikkink code Bosnia in both the transition from civil war (1995-) and transition of state creation (1992-) categories, many other states that had a civil war after state creation are not included in the transition from civil war category. In addition, as Meernik et al.'s data from Uppsala/PRIO illustrates, many of these civil conflicts were small in scope. Finally, some states in the Kim and Sikkink data (such as Azerbaijan in 1992, Guatemala in 1984) are coded as having democratic transitions while they were in the middle of civil wars, according to the data from Uppsala/PRIO. In sum, scholars need to be cautious in how they construct their samples to bolster the quality of their inferences. Moreover, academics have to be wary of the non-linear relationship between processes of democratization and civil conflict.

Statistical modeling

Finally, the studies under review employ a variety of statistical techniques. These methodological choices are made to strengthen the robustness and validity of statistical findings. Yet, these choices have implications for how effectively the model can account for real-world causal processes. Methodological choices also have implications as to

⁷³ Kim and Sikkink, "Explaining the Deterrent Effect."

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the studies' unit of analysis. As we shall see, these questions have been dealt with in a variety of ways.

Arguably, Olsen et al. provide the simplest model.⁷⁴ They take the political transition as their unit of analysis (91 transitions in total) and compare human rights and democracy scores at the transition with five and ten years after. However, they fail to account for when TJ is implemented in that five or ten year period. Essentially, therefore, regardless of when it is implemented, TJ gets 'credit' for the five or ten year change in human rights or democracy as if TJ had been implemented right at the transition. Taking five or ten year snapshots can mask significant fluctuations, which are lost in this framework. In their model, Olsen et al. include controls such as GDP per capita, time since transition, levels of democracy, and regional dummies, but leave out others such as conflict involvement that previous quantitative research on human rights and democracy have found important.⁷⁵

Kim and Sikkink⁷⁶ and Wiebelhaus-Brahm⁷⁷, by contrast, employ cross-national time-series techniques, using the country-year as their unit of analysis. They are able to control for country-specific factors that might be influencing results. Both studies also employ two-stage estimation techniques that help researchers control for endogeneity; in other words, the possibility that an alleged effect of TJ was actually caused by an antecedent condition that produced TJ and the outcome of interest. While arguably an improvement, these models too are only as good as the data behind them. Several variables that theoretically should be included as a control or in the first stage equation, such as the nature of the political

⁷⁴ Olsen et al., *Transitional Justice in Balance*.

⁷⁵ Steven Poe and C. Neal Tate, "Repression of Human Rights to Personal Integrity in the 1980s: A Global Analysis," *American Political Science Review* 88.4 (1994):853-872; Steven Poe, C. Neal Tate, and Linda Camp Keith, "Repression of the Human Right to Personal Integrity Revisited: A Global Cross-National Study Covering the Years 1976-1993," *International Studies Quarterly* 43.2 (1999): 291-313.

⁷⁶ Kim and Sikkink, "Explaining the Deterrent Effect."

⁷⁷ Wiebelhaus-Brahm, *Truth Commissions and Transitional Societies*.

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transition/balance of forces, are omitted due to a lack of cross-national data. Moreover, they do not control for the presence of other TJ mechanisms.

Because they are interested in the propensity of states that employ TJ to see a resumption of conflict, Lie et al.⁷⁸ employ an events history model, namely a Cox Proportional Hazards Model. Theoretically, this makes great sense as the technique allows the researcher to examine whether the ‘treatment’, in this case TJ, increases or decreases the probability of conflict recurrence. The unit of analysis is the duration of the peace spell following a cessation of conflict. It is superior to limited dependent variable techniques, like logit and probit, which is used by Meernik et al.⁷⁹ and Melander⁸⁰, because these latter approaches assume that each observation is independent.

However, Lie et al.’s approach may artificially inflate the number of cases in their sample, as they disaggregate their data into ‘peace years’, which starts the first day of peace after conflict and ends December 31 the same year; the problem with their methodology is that subjects enter the dataset again on the first day of peace after the resumed conflict ends. They employ a duration variable measuring the length of the post-conflict peace period, measured in days represented from the first day of peace to the first day when the conflict again passes the 25 battle-related deaths threshold. This is a problem because there are many countries that have several peace agreements signed before lasting peace occurs, so the number of post-conflict societies as defined by this sample (200 in total) may be exaggerated. This potential problem could be solved by controlling for conflict proneness.

Conclusions: Bridging the Qualitative-Quantitative Divide?

This growing quantitative literature has been met with significant criticism. Two challenges we have discussed, the measurement

⁷⁸ Lie et al, “Post-Conflict Justice.”

⁷⁹ Meernik et al., “The Impact of International Tribunals.”

⁸⁰ Melander, “Justice or Peace?”

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problem and modeling causal relationships, are frequently pointed out. More generally, critics charge, quantitative studies fail to account for the unique nature of each country's TJ experience. As one set of critics argue, large-N cross-national time-series studies fail to account for "changes in the [global] normative context" over time and for "local contextual factors."⁸¹ Some quantitative studies do seek to control for world-historical context and we have discussed some attempts to account for local context. Nonetheless, cross-national studies necessarily sacrifice detail for generalizability. Quantitative studies may be attributing causality to formal TJ processes in part because they cannot hope to control for all local factors.

Ben-Josef Hirsch et al.⁸² charge that large-N studies are inherently biased by "presuming or predetermining the goals" of TJ. It is the case that quantitative research has selected dependent variables that are measurable through existing datasets. Not only might it be difficult to construct a convincing causal argument for a relationship, but these measures reflect an implicitly Western conception of rights and justice that neglects deeper structural, socioeconomic issues. Yet, in their critique, critics seem to sometimes confuse goals, or measuring success, with measuring impact, which could be considered distinct. Success relates to TJ mechanisms achieving the goals set out for them by their creators, whereas impact refers to the political, social, economic, and psychological effects of TJ processes.⁸³ Quantitative studies have generally tested cause-effect claims about various TJ mechanisms and a variety of areas of impact that have been asserted for years. More generally, while acknowledging these challenges, we reject the claim that quantitative methodologies are worthless for understanding the impact of TJ.

⁸¹ Ben-Josef Hirsch et al., "Measuring the Impacts of Truth and Reconciliation Commissions," 395-396.

⁸² Ben-Josef Hirsch et al., "Measuring the Impact of Truth and Reconciliation Commissions," 398.

⁸³ Brahm, "Uncovering the Truth," 17-19.

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Frankly, the field as a whole benefits from a mixed methods research program.⁸⁴ Quantitative approaches are well-suited to aggregating large amounts of data and parsing out patterns of behavior over time and space. Qualitative research, by contrast, provides us with intimate case details that allow for the identification and tracing of causal processes. As such, qualitative and quantitative methods are actually quite complementary. Together, both approaches are important in advancing knowledge and putting TJ policy on more sound footing. Based on our survey of the quantitative literature, we offer a few recommendations for future TJ research.

A first issue relates to the scope of these findings. As we have seen, studies have drawn their samples in different ways to examine different types of transitional periods. This is appropriate, but researchers and consumers need to be cautious of overgeneralizing about what the findings mean in policy terms. This speaks, in part, to broader debates in the field about whether the term 'TJ', which emerged to characterize transitions from authoritarian rule, is applicable to transitions from war to peace. One benefit of quantitative methods is that variation in transition is something for which we can control to explore how much (or how little) these cases have in common.

Second, the question of the causal mechanisms through which TJ effects outcomes needs further exploration. To focus specifically on human rights, while Kim and Sikkink⁸⁵ argue that human rights prosecutions lower levels of repression through deterrence and norm diffusion, future research needs to empirically determine whether or not this is the case. Doing so will help to answer important sampling dilemmas, such as whether trials should only be coded if a verdict is reached⁸⁶ or if the entire process including indictments, arrests, trials, etc. should be coded. Likewise,

⁸⁴ P. Pham and P. Vinck, "Empirical Research and the Development and Assessment of Transitional Justice Mechanisms," *International Journal of Transitional Justice* 1..2 (2007): 234-235.

⁸⁵ Kim and Sikkink, "Explaining the Deterrent Effect."

⁸⁶ Olsen et al., *Transitional Justice in Balance*.

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future empirical research into the role that truth commissions play in norm diffusion can help to determine which sampling strategies are best in regards to whether only those truth commissions that hold public hearings or that see their reports publicly released should be coded, or if all truth commissions that begin their work should be coded. However, as Mendeloff notes, the concept of norm diffusion is difficult to empirically examine and may require great methodological sophistication.⁸⁷ We lack adequate cross-national measures to distinguish the two causal mechanisms so progress will likely need to come through qualitative research.

One prominent way through which norm diffusion potentially occurs is through the efforts of external actors. As such, their role in post-conflict and transitional societies may serve as a valuable predictor for which types of TJ mechanisms are employed. This speaks to the issue of selection effects. Recent literature has made some headway in regards to figuring out why states choose the TJ measures that they do. This new research paradigm is important for understanding the normative implications of TJ, because if certain post-conflict scenarios (such as the involvement of the United Nations) create conditions that are riper for (particular types of) TJ, these measures may play a critical role in the emergence of post-conflict human rights norms. For example, the involvement of non-governmental and inter-governmental organizations is a significant predictor of whether TJ measures are implemented in post-conflict societies.⁸⁸ Interestingly, the involvement of the United Nations in post-conflict societies is significant for predicting the implementation of human rights prosecutions, truth commissions, amnesty and other TJ measures. This is significant, because the presence of international organizations in post-conflict societies may help establish norms that emphasize accountability for human rights abuses. While this is speculation, it is important to note that better understanding of the normative implications of TJ may require treating the emergence of

⁸⁷ Mendeloff, "Deterrence, Norm Socialization."

⁸⁸ Dawn L. Rothe and Scott Maggard, "Factors that Impede or Facilitate Post-Conflict Justice Mechanisms?" *International Criminal Law Review* 12.2 (2012): 193-217; Olsen et al., *Transitional Justice in Balance*, 79-96.

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TJ as the dependent variable because there are certain variables that increase the likelihood of TJ processes being created. As such, the quantitative studies that seek to control for antecedent conditions in an attempt to isolate causality on the part of TJ are on the right track. However, as Olsen et al. note, future research needs to uncover the precise role that external involvement plays in influencing which TJ mechanisms are employed. A mixed-methods approach may be most useful for understanding this complex process.

Third, research should address the role of timing and sequencing of TJ mechanisms. Many studies treat individual TJ mechanisms in isolation. Yet, states frequently employ multiple mechanisms, whether simultaneously or in sequence. Quantitative studies haven't effectively dealt with this. Interaction terms have rarely been used, but would allow the researcher to examine the cumulative effects when more than one mechanism has been utilized. Timing and sequencing is a more difficult modeling challenge. TJ mechanisms may or may not be implemented when political transitions occur, but quantitative studies typically implicitly assume that they are. Moreover, TJ mechanisms may be implemented in different orders in different countries. This order likely matters for their individual and collective impact, but quantitative studies have not adequately dealt with this.

Finally, further research should focus on specific elements of TJ processes. Future research on human rights prosecutions should measure the impact of specific elements of the prosecution process on the dependent variable, for example. Meernik⁸⁹ is a good starting point in this line of research, as he measures the impact of certain elements of ICTY trial processes (captures, surrenders, transfers to The Hague, and judgments) involving key figures in the Bosnian conflict (Slobodan Milosevic, Momcilo Krajisnik, Biljana Plavsic, Rahim Ademi, Sefer Halilovic, Naser Oric, Tihomir Blaskic, Dario Kordic and Mario Cerkez) on societal peace in Bosnia. While his measures on the dependent variable are limited to Bosnia, his

⁸⁹ James Meernik, "Justice and Peace? How the International Criminal Tribunal Affects Societal Peace in Bosnia," *Journal of Peace Research* 42.3 (2005): 271-289.

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methodological approach may be utilized elsewhere and, with the loss of some qualitative information, even be applied cross-nationally. Such an approach would enable the quantitative literature to move away from treating TJ as a dichotomous variable. A similar approach could be taken to explore key elements of other TJ processes, such as truth commission public hearings or the expiration of amnesty offers. Through such efforts, future research could better understand the impact of specific elements of TJ processes on variables such as human rights.

The second wave of quantitative research has the potential to move the field in this direction. The newly released Transitional Justice Research Collaborative and Justice Data Project are marked advances over the early datasets.⁹⁰ Both include more qualitative data on TJ processes to enable researchers to make their own theoretical judgments about how to account for TJ variation in their measurement and sampling choices. Ultimately, this could provide important policy recommendations by speaking to specific design elements of TJ processes.

Appendix 1: Variation in Human Rights Trial Data

Country	Kim and Sikkink 2010	Olsen et al. 2010
Afghanistan		x
Albania		x
Argentina	x	x
Bangladesh		x
Benin	x	x
Bolivia	x	x
Bosnia	x	x
Bulgaria	x	x
Burundi	x	
Cambodia	x	

⁹⁰ See <https://transitionaljusticedata.com/> and <http://www.justice-data.com/>.

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Chad	x	
Chile	x	x
DRC	x	
Republic of the Congo		x
Cote d'Ivoire		x
Croatia	x	x
Czechoslovakia		x
Czech Rep	x	x
East Tim	x	x
Ecuador	x	
Egypt		x
El Salvador	x	x
Eritrea	x	
Estonia		x
Ethiopia	x	
France		x
Gabon		x
Gambia		x
Ghana		x
Greece		x
Guatemala	x	x
Guinea-Bissau		x
Guinea		x
Haiti	x	x
Honduras	x	
Hungary	x	x
Indonesia	x	x
Iran		x
Iraq	x	x
Israel		x
Italy		x

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Jamaica		x
Jordan		x
Kenya		x
Kuwait		x
Latvia		x
Lebanon		x
Lesotho		x
Liberia		x
Lithuania	x	x
Macedonia	x	x
Madagascar		x
Malawi	x	x
Mali	x	x
Mauritania		x
Mexico	x	
Morocco		x
Myanmar		x
Namibia	x	x
Nicaragua	x	x
Niger	x	x
Nigeria		x
Oman		x
Pakistan		x
Panama	x	x
Papua New Guinea		x
Paraguay	x	x
Peru	x	x
Philippines		x
Poland	x	x
Portugal	x	
Romania	x	x

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Russia		x
Rwanda	x	x
Senegal	x	x
Serbia and Montenegro	x	x
Sierra Leone	x	x
Somalia		x
Slovenia	x	
South Africa	x	x
South Korea	x	x
Spain	x	x
Sudan		x
Thailand	x	x
Tunisia		x
Turkey	x	
Uganda		x
Uruguay	x	x
United Kingdom		x
United States		x
Uzbekistan		x
Zambia		x

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Appendix 2: Variation in Truth Commission Cases

Cases*	Hayner 2001	Freeman 2006	Wiebelhaus-Brahm 2010	Kim and Sikkink 2010	Olsen et al. 2010
Pakistan					x
Uganda 1974	x				x
Zambia 1975					x
India					x
Bolivia	x	x	x	x	x
Argentina	x	x	x	x	x
Kenya					x
Honduras					x
Uruguay 1985	x		x	x	x
Zimbabwe	x		x		x
Philippines			x	x	x
Uganda 1986	x	x	x	x	x
Ghana 1989					x
Chile 1990	x	x	x	x	x
Czech Republic					x
Nepal	x	x	x	x	x
South Africa 1990					x
Chad	x	x	x	x	x
Lithuania 1991					x
South Africa 1991					x
El Salvador	x	x	x	x	x
Germany	x	x	x	x	x

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Zambia 1993					x
Sri Lanka 1994	x	x	x		x
Sudan					x
Brazil					x
Burundi 1995	x			x	x
Haiti	x	x	x	x	x
Mexico 1995					x
South Africa 1995	x	x	x	x	x
Ecuador 1996	x	x	x	x	x
Guatemala	x	x	x	x	x
Sweden					x
Burkina Faso					x
Lithuania 1998					x
Indonesia				x	x
Nigeria	x	x	x	x	x
Rwanda 1999					x
Rwanda 1999-2000					x
Lebanon 2000					x
Malaysia					x
Poland					x
South Korea 2000-2					x
South Korea 2000-4			x	x	x
Uruguay 2000		x	x		x
Grenada		x			
Jamaica					x
Lebanon 2001					x
Mexico 2001					x
Panama		x	x	x	x
Peru		x	x	x	x
Sri Lanka					x
Thailand					x
Zambia 2001					x
East Timor		x	x	x	x
Ghana 2002		x	x	x	x
Serbia and		x	x	x	x

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Montenegro					
Sierra Leone		x	x	x	x
CAR				x	x
Chile 2003					x
Paraguay		x	x	x	x
DRC		x	x	x	x
Lebanon 2004					x
Morocco		x	x		x
Colombia					x
Indonesia 2005		x			
Indonesia-East Timor					x
Lebanon 2005					x
Liberia 2005		x	x	x	x
Bosnia					x
Sri Lanka 2006					x

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Appendix 3: Variation in Post-Civil War States

Country	Meernik et al.	Kim & Sikkink
Afghanistan	x	x
Angola	x	x
Azerbaijan	x	
Bangladesh	x	
Bosnia	x	x
Burkina Faso	x	
Burundi	x	x
Cambodia	x	
Cameroon	x	
Central African Republic	x	
Chad		x
Comoros	x	x
Republic of Congo	x	
Cote d'Ivoire	x	
Croatia	x	
Democratic Republic of Congo		x
Djibouti	x	
Egypt	x	
El Salvador	x	
Equatorial Guinea	x	
Eritrea	x	
Ethiopia	x	x
Gambia	x	
Georgia	x	
Ghana	x	
Guinea-Bissau	x	x
Haiti	x	

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India	x	
Indonesia	x	
Iran	x	
Iraq	x	x
Israel	x	
Kenya	x	
Laos	x	
Lebanon	x	x
Lesotho	x	x
Liberia	x	x
Macedonia	x	
Malaysia	x	
Mali	x	
Mauritania	x	
Mexico	x	
Moldova	x	
Morocco	x	
Mozambique	x	
Myanmar	x	
Nepal	x	
Nicaragua	x	
Niger	x	
Nigeria	x	
Pakistan	x	
Panama	x	
Papua New Guinea	x	
Paraguay	x	
Peru	x	
Philippines	x	
Romania	x	
Russia	x	

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Rwanda	x	x
Saudi Arabia	x	
Senegal	x	
Serbia and Montenegro	x	
Sierra Leone	x	x
Solomon Islands		x
Somalia	x	
South Africa	x	
Spain	x	
Sri Lanka	x	
Sudan	x	
Suriname	x	
Syria	x	
Tajikistan	x	
Thailand	x	
Togo	x	
Trinidad and Tobago	x	
Tunisia	x	
Turkey	x	
Uganda	x	
United Kingdom	x	
Uzbekistan	x	
Venezuela	x	
Yemen Arab Republic	x	
Yemen People's Republic	x	
Zaire	x	
Zimbabwe	x	

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