

# QUIMPO: Qualitative Imputation of Missing Potential Outcomes

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## Abstract

We propose a framework for summarizing beliefs and uncertainty about average causal effects using qualitative information. Our approach synthesizes counterfactual qualitative inquiry with an insight from the quantitative causal inference literature, extreme value bounds. Under the Neyman-Rubin model, units are endowed with potential outcomes, or responses units would express depending on the level of some treatment. The goal of qualitative counterfactual analysis is to use the expressed outcome and auxiliary information to infer what would have happened had the treatment been set to a different level. Essentially, qualitative researchers engaged in counterfactual analysis impute missing potential outcomes. When we cannot impute some counterfactuals, we can fill in the missing potential outcomes with best- and worst-case scenarios. We show how the resulting extreme value bounds represent fundamental uncertainty and how the imputation of missing potential outcomes can shrink that uncertainty in a structured way. We provide an application of QUIMPO to 122 cases that could have experienced transitional truth commissions, 16 of which did. Before the application of QUIMPO, the extreme value bounds are 100 percentage points wide; incorporating qualitative beliefs about counterfactuals shrinks these bounds to approximately 40 points.

A special promise of qualitative inquiry is that deep case knowledge can generate informed counterfactual guesses of *what would have happened if things had been different*. Our goal is to show how a systematic aggregation of these guesses can shrink fundamental uncertainty about average causal effects in a principled manner. Our approach shares much in common with those of Glynn and Ichino (2015) and Humphreys and Jacobs (2015), both of which combine qualitative and quantitative information to produce better *estimates* of average causal effects. By contrast, our “estimator” is the qualitative researcher, who either does or does not have an informed guess as to true effect of treatment for a single unit.<sup>1</sup> Qualitative imputation of missing potential outcomes, or QUIMPO, is a procedure for summarizing beliefs about a set of case-specific causal effects, something akin to a meta-analysis for qualitative inquiry.

Under the Neyman-Rubin causal model (Neyman, 1923; Rubin, 1974), units are endowed with a set of potential outcomes, only one of which they reveal depending on the realization of exposure to causal agents. In the most basic case, units have only two potential outcomes,  $Y_i(1)$  and  $Y_i(0)$ , which correspond to the treated and untreated potential outcomes. This setup embeds an assumption of noninterference,<sup>2</sup> or the assumption that unit  $i$  does not have potential outcomes beyond  $Y_i(1)$  and  $Y_i(0)$  that might depend on the treatment assignments of other units. The realized treatment  $d_i$  then “reveals” the observed outcome  $Y_i$  via the “switching” equation:  $Y_i = Y_i(1)d_i + Y_i(0)(1 - d_i)$ . Treated units reveal their  $Y_i(1)$  and untreated units reveal their  $Y_i(0)$ .

Due to the fundamental problem of causal inference (Holland, 1986), we can never observe a unit in both its treated and untreated state. In Table 1, we see a treated unit ( $d_i = 1$ ) and its observed outcome ( $Y_i = 1$ ). We know  $Y_i(1)$  is because it is equal to the outcome that unit  $i$  revealed; we are missing the untreated potential outcome  $Y_i(0)$ . The goal of counterfactual analysis is to fill in the missing value with either a zero or a one.

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<sup>1</sup>We leave entirely to the side the question of when and how qualitative researchers should draw causal inferences in a single case. Doing so is generally quite difficult, but obviously not impossible. See Goertz and Mahoney (2012), Bennett and Checkel (2014), and Ragin (2014) for some approaches. The QUIMPO framework leaves open the possibility that the qualitative researcher does not have a good guess in a particular case. As we will show, such cases simply do not help to reduce fundamental uncertainty.

<sup>2</sup>Sometimes noninterference is referred to as SUTVA, or the “stable unit treatment value assumption,” but SUTVA encompasses an additional assumption beyond noninterference, rendering its invocation slightly ambiguous.

Table 1: Causal Inference for a Single Unit

$d_i$	$Y_i$	$Y_i(0)$	$Y_i(1)$
1	1	?	1

One of the analytic tasks of qualitative research is to understand the separate impacts of the many causal factors that explain outcomes in a single case. Moreover, qualitative researchers often consider the mechanisms by which treatments affect outcomes. Both questions (*which* factors matter and *why*) are important, complicated, and difficult to answer.<sup>3</sup> We will focus on a tiny slice of that analytic task: understanding the impact of a single causal factor on a single outcome, inclusive of any and all mechanisms that may be at play in a particular case. Using the dichotomy described in Goertz and Mahoney (2012), our question is about the “effects of a cause” rather than the “causes of effects.”

Causal inference for a single unit requires the researcher to *impute* the missing potential outcome. Using case knowledge, information about individual actors’ incentives, institutional arrangements, temporal variation, and logic, qualitative researchers can make a guess about what would have happened the treatment had been set to a different level. The uncertainty attending to that guess can also be qualitatively expressed. For some units, this task is easy. For others, it is much harder. Qualitative methodologists have developed a battery of approaches for determining whether the available qualitative data are sufficient to license a causal interference for a single case (Collier, Brady and Seawright, 2004; Collier, 2011; Goertz and Mahoney, 2012; Mahoney, 2012; Fairfield, 2013; Bennett and Checkel, 2014; Ragin, 2014). Whether a particular method is applicable to a particular case is of course a matter of debate, and we make no effort here to differentiate among them. Indeed, in our empirical application, we rely in large part on (our reading of) the qualitative inferences drawn by other researchers, each of whom has made choices among the variety of methods available to them. As our application below will show, sometimes no approach (qualitative, quantitative, or otherwise) is sufficient for causal inference and we are forced to admit ignorance of causal effects. We view the ability to incorporate the *lack* of knowledge about counterfactuals as

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<sup>3</sup>See Bullock, Green and Ha (2010) for a discussion of the extreme difficulty inherent in studying mechanisms (mediators), even when treatments are randomly allocated to subjects.

a major strength of the QUIMPO approach.

Our goal will be to summarize qualitative inferences about individual level causal effects for a set of  $N$  units. In particular, we aim to place bounds around the average treatment effect (ATE) for these  $N$  units:  $ATE \equiv \sum_i^N Y_i(1) - Y_i(0)$ . The ATE is a common target of inference in quantitative research but less so in qualitative work (Goertz and Mahoney, 2012). One might reasonably argue that a chief advantage of qualitative methods is that they are addressed to targets that are far more subtle than a simple average over possibly very heterogeneous cases. While we will focus on the ATE in this paper, QUIMPO could easily be extended to many other estimands, even to the full distribution of treatment effects in a population. Nothing about our approach limits researchers to studying average effects.

Since QUIMPO is a way to summarize qualitative beliefs about treatment effects, it is not a testing framework, nor can it be considered an estimation approach. We view QUIMPO as a way to structure the uncertainty researchers have about causal effects. This uncertainty is not anything like a standard error or a confidence interval. Instead, the bounds represent fundamental uncertainty. They summarize what we know for sure (the data revealed by the world), what we think we know (the inferences we draw in some or all cases), and what we know we do not know.

## The Procedure

Extreme value bounds (Manski, 1999) are the logical bounds around the ATE.<sup>4</sup> Consider a binary outcome and a binary treatment. In the “best” case, the outcome for everyone in the treatment group is “1” and the outcome for everyone in the control group is “0.” In this scenario, the ATE is +100 percentage points. By the same logic, in the worst case scenario, the ATE is -100 percentage points. Before any data are collected, the extreme value bounds are 200 percentage points wide, which correctly characterizes our utter ignorance of the ATE.

Once the data are collected, we observe each unit in either its treated or untreated state and

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<sup>4</sup>Bounds are often used when outcome data are missing. See Gerber and Green (2012, chp. 7) for an accessible introduction to bounding approaches for attrition. Aronow, Baron and Pinson (2017) applies bounds to the case of researcher-induced attrition (dropping subjects who fail a manipulation check) and Coppock (2018) applies bounds to the analysis of experiments in which researchers condition on one post-treatment variable to study the effects of treatment on a second post-treatment variable.

we observe the associated potential outcome. In the control group we observe  $Y_i(0)$  but not  $Y_i(1)$ , and in the treatment group we observe  $Y_i(1)$  but not  $Y_i(0)$ . If we now impute the best case and worst case scenarios, we only have to impute *half* of the potential outcomes because the world has revealed the other half. Once the data are collected, the extreme value bounds are 100 percentage points wide.

We can extend this idea beyond the binary case. Suppose the logical extrema of the outcome variable are  $Y^{MAX}$  and  $Y^{MIN}$ . We substitute these value for the unknown potential outcomes in order to calculate the maximum and minimum possible values for the average treated ( $\bar{Y}_1$ ) and untreated ( $\bar{Y}_0$ ) potential outcomes. These are

$$\begin{aligned}\bar{Y}_1^{MAX} &= \frac{\sum_1^m Y_i + (N - m) * Y^{MAX}}{N} \\ \bar{Y}_1^{MIN} &= \frac{\sum_1^m Y_i + (N - m) * Y^{MIN}}{N} \\ \bar{Y}_0^{MAX} &= \frac{\sum_{m+1}^N Y_i + (m) * Y^{MAX}}{N} \\ \bar{Y}_0^{MIN} &= \frac{\sum_{m+1}^N Y_i + (M) * Y^{MIN}}{N}\end{aligned}$$

, where the first  $m$  of  $N$  units are treated and the remainder are untreated. The ATE cannot be larger than  $\bar{Y}_1^{MAX} - \bar{Y}_0^{MIN}$  nor can it be smaller than  $\bar{Y}_1^{MIN} - \bar{Y}_0^{MAX}$ . These bounds represent – before the inclusion of any priors, qualitative information, or other expertise – the uncertainty attending to the ATE. Again, this uncertainty is not due to the sampling or assignment procedures, but instead to the fact that we only observe half the data and we are uncertain about the other half.

The QUIMPO procedure repeats the following two steps, adding possibly stronger assumptions each time, until the reservoir of qualitative case knowledge on the topic is drained.

1. Impute missing potential outcomes using qualitative case and process knowledge
2. Recompute extreme value bounds

What is novel here is step 1. Imputing a missing potential outcome amounts to stating that, on

the basis of special knowledge of what would have happened had a treated unit been in control, or a control unit in treatment. By definition, whether an imputation is correct cannot be demonstrated, because the Fundamental Problem of Causal Inference is so fundamental. Even so, the vast majority of causal inferences we draw on a daily basis can be seen as single-case imputations of missing potential outcomes.

## A Toy Example

Consider a population of ten units, seven of whom have been treated and three of whom have not. Before adding any qualitative information, the bounds on the ATE extend from -40 percentage points to 60 percentage points. Table 2 presents the table of potential outcomes as it proceeds through three rounds of imputation: easy cases, monotonicity, and hard cases.

The 4<sup>th</sup> and 5<sup>th</sup> columns describe the imputation of the five “easy” cases. These are scenarios in which the untreated outcomes for units 1, 2, and 5 are obviously (to the researcher) 0, 0, and 1, respectively. Similarly, the treated outcomes of units 8 and 9 are 1 and 0. These cases are “easy” in the sense that it’s clear to the researcher that the treatment could not have had an effect on the outcome, perhaps because the outcome was clearly the consequence of a complex set of factors that exclude the treatment under consideration. These five imputations have shrunk the extreme value bounds considerably, and they now reach from -20 points to 30 points.

Suppose the researcher is next willing to grant that at a minimum, the treatment cannot hurt outcomes, and so applies monotonicity:  $Y_i(1) \geq Y_i(0)$ . This assumption should not be made lightly, and would need to be defended (as with each step in this process) on qualitative grounds. Monotonicity in this case implies that the untreated outcomes of units 5 and 6 must be 0, shrinking the bounds to  $[0, 30]$ . Monotonicity implies that the extreme value bounds cannot include negative values: all treatment effects must be 0 or 1, but cannot be -1.

As a final step, the researcher invests heavily in the remaining “hard cases,” units 3, 4, and 10. Suppose that the efforts are only partially rewarded. In cases 3 and 4, the researcher concludes that the treatment had a positive effect, and so imputes a 0 for the untreated outcomes in those cases. But in case 10, the empirical record is too thin and the causal story too murky to make a

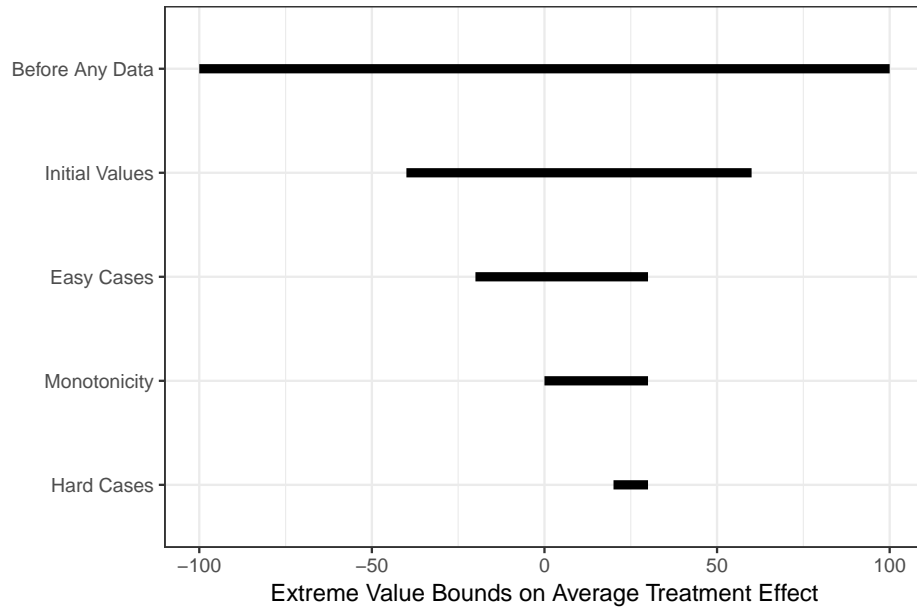
confident call. The resulting bounds [20, 30] correctly incorporate the remaining uncertainty in the researchers' summary of beliefs about causal effects.

Figure 1 shows the width of the extreme value bounds at each step in the process. The incorporation of both quantitative and qualitative information reduces the width of the bounds from 200 points to 10 points, corresponding to a dramatic reduction in fundamental uncertainty.

Table 2: Toy Application of QUIMPO

	Observed		Initial values		Easy cases		Monotonicity		Hard cases	
	$d_i$	$Y_i$	$Y_i(0)$	$Y_i(1)$	$Y_i(0)$	$Y_i(1)$	$Y_i(0)$	$Y_i(1)$	$Y_i(0)$	$Y_i(1)$
1	1	1		1	1	1	1	1	1	1
2	1	1		1	1	1	1	1	1	1
3	1	1		1		1		1	0	1
4	1	1		1		1		1	0	1
5	1	0		0	0	0	0	0	0	0
6	1	0		0		0	0	0	0	0
7	1	0		0		0	0	0	0	0
8	0	1	1		1	1	1	1	1	1
9	0	0	0		0	0	0	0	0	0
10	0	0	0		0		0		0	
Bounds in points:			[-40, 60]		[-20, 30]		[0, 30]		[20, 30]	

Figure 1: Toy Application of QUIMPO





## Application of QUIMPO to the Effect of Truth Commissions

In this section, we apply QUIMPO to the study of transitional truth commissions. Our first task was to define the universe of cases that were *eligible* for Transitional Truth Commissions (TTCs). Clearly, all cases that received a TTC were eligible; the difficulty was finding those cases that could have but did not experience a TTC. We obtained information on two types of political transitions: transition from civil war, and transitions to democratic rule starting 1980 and until 2008. We chose this historical period because the first completed truth commission was established in 1983 and we need some time to elapse after a truth commission is possible in order for the world to reveal outcomes.

We identify civil conflict transition cases eligible for truth commissions from the Kreutz (2010) Uppsala Conflict Data Program's (UCDP) Conflict Termination Dataset. Each case is a separate conflict occurrence that (a) reaches an intensity of at least 1000 cumulative battle deaths, and (b) terminates after 1980 and before 2008 through a peace agreement, ceasefire, victory (by government or rebel forces), or petering out through low activity. In order to arrive at the instances of regime change eligible for truth commissions, we recorded all instances of democratic transition from the various types of autocratic regimes occurring between 1980 and 2008 from the Geddes, Wright and Frantz (2014) dataset. We will analyze our two datasets entirely separately. We have 55 end-of-conflict observations and 66 democratization observations.

### Definition of treatment

Our treatment is the establishment of a TTC. We use the broad definition of truth commissions given in Hayner (2002): "officially created investigative bodies that document patterns of past human rights abuse over a specified period of time." That said, which bodies meet this or similar definitions has been the subject of much debate. Some lists of TTCs are relatively expansive (USIP, 2011; Olsen, Payne and Reiter, 2010; Hayner, 2006) and others are relatively conservative (Dancy, Kim and Wiebelhaus-Brahm, 2010; Bakiner, 2015; Kim and Sikkink, 2010). In keeping with the more restrictive accounts, we consider a body a TTC if it (i) investigates for a limited amount of time; (ii) publishes a final report; (iii) examines a limited number of past events along with

their patterns, causes and consequences; (iv) enjoys autonomy from direct intervention by political actors; and (v) remains official in character (Bakiner, 2015; Dancy, Kim and Wiebelhaus-Brahm, 2010).

Second, truth commissions can be further separated into two types. Bakiner (2013) separates the two into “transitional” truth commissions (those that come up within the first three years of transition to peace or democracy) and “non-transitional” truth commissions (those that do not arise in the context of transition), arguing that the two types display specific dynamics and require different analytic tools. In addition, Wiebelhaus-Brahm (2009*b*) describes non-transitional commissions as “historic” truth commissions and argues that they are qualitatively different from commissions that take place during a transition. Consequently, we consider as “treated” the fifteen cases that Bakiner considers to be transitional.

The “untreated” category then includes not only cases that experience no commission whatsoever but also those that establish truth commissions many years after transition (Uruguay, South Korea, Panama, Brazil); those that announce truth commissions but do not implement them (Burundi, Nepal); those created by authoritarian governments (Morocco); and those initiated in the context of limited demobilization during ongoing conflict (Colombia). Substantively, we also consider as “untreated” truth commissions that are set up to investigate corruption, embezzlement, fraud, and similar crimes (as in Olsen et al. (2010)); unofficial commissions created by community processes (such as Brazil); and commissions of inquiry set up to investigate a singular event (such as a riot, pogrom or massacre leading to disappearances). Most importantly, some states institute “sham” commissions or bodies that come purely as window-dressing political maneuvers (Sri Lanka), while some establish truth commissions that are disbanded before they complete their work (Bolivia, Ecuador).

In experiments, sometimes subjects are assigned to be treated but they do not take treatment. Under an exclusion restriction that the assignment itself does not affect outcome, these “noncompliers” reveal their *untreated* potential outcome. In cases that experienced commission-like bodies that do not meet our criteria for a TTC, we also assert that they, like noncompliers, reveal their untreated outcomes. Our task then is to impute their treated potential outcome, i.e., what would

have happened had the case experienced a full TTC according to our definition.

When constructing counterfactuals, a major difficulty is specifying a “conceivable” counterfactual world that has with enough detail to be meaningful (Fearon, 1991). That is, for every case that experienced a TTC, there are an infinity of ways to not experience a TTC; a similar logic holds for the control group. We endeavored to imagine a world that is as similar as possible to the one that occurred, but for the difference of having a TTC.

## **Defintion of outcomes**

A main reason to separate the end-of-conflict cases from the democratization cases is the definition of outcomes. One major advantage of treating this problem qualitatively is that have no need to analyze the same outcome variable for all cases just in order to maximize N. Instead, we consider the outcome that seems most appropriate to each set of cases. For the end of conflict cases, our main is the resumption of violence whereas in the democratization cases, our outcome is the resumption of authoritarianism.

Since we are interested in the medium-term impact of truth commissions, these outcomes are recorded as ‘1’ for each case when conflict (greater than 25 battle deaths in a given year) or authoritarianism (following a ceasefire, peace agreement, victory or completed democratic transition that lasts at least 3 years) resumes within a timeframe of ten years from its termination, and ‘0’ otherwise. The moment of termination is gleaned from a combination of the UCDP Conflict Termination Dataset, GWF Autocratic Regimes Dataset, as well as on qualitative evidence on the timeline of activity of individual conflicts. According to this coding, “positive” treatment effects correspond to normatively bad events: violence and authoritarianism.

## **Imputing Missing Potential Outcomes**

We attempted to impute the missing potential outcome in 122 cases; we succeeded in making 68 imputations. We break up the imputations into four large categories. We give the main reasons for our choices here, but see the appendix for short descriptions of all cases.

## Step 1: Disbanded and Discredited Cases

As the first step, we identify truth commissions that were either established but disbanded before completion (Bolivia in 1982, the Philippines in 1986, the Federal Republic of Yugoslavia), or were established solely for the purposes of window dressing and political maneuvering (Sri Lanka in 2010). The observed outcome ( $Y_i(0)$ ) in each of these four cases has been ‘0’, as conflict did not resume within ten years of transition in any of these cases.

Upon researching each individual case, we found that they were disbanded (or reduced to window-dressing) due to underfunding or the lack of political will to investigate violations. We determined that they were not left incomplete or rendered ineffective because of fears of renewed violence. For instance, in the case of Bolivia, Hayner (1994) reports Loyola Guzman, executive secretary of the commission, as saying that the commission did receive very limited support from the government but lacked sufficient resources and political support to complete its work, leading to its disbanding within two years of formal operation. Similarly, looking at other disbanded commissions Brahm (2004) concludes: “In Bolivia and Ecuador, commissions were disbanded before completing their work because the investigations became too politically sensitive. Clearly, the commissions cannot be solely blamed for this – the political will to act on their findings did not exist.” Lastly, a truth commission was established in Sri Lanka in response to consistent international pressure to investigate human rights violations, but it was denounced as a farce domestically and internationally for whitewashing state culpability for casualties. Nevertheless, thousands of Tamils and Muslims displayed a strong desire to participate in the process, showing up to the capital to give testimony despite turbulent conditions (Thiranagama, 2013), and the new government in 2015 announced a proposal for a new truth commission to probe civil war-era violations.

These case-specific analyses lead us to believe that had a “real” truth commission been set up and completed in these cases, it would not have contributed to a resumption in conflict. Consequently, we impute the treated potential outcome in each of these cases to be 0.

## Step 2: Treated cases

Next, we turn to the treated cases, which are transitional cases that received transitional truth commissions. We identified 16 such cases (15 as per Bakiner’s (2013) definition of transitional truth commissions, and Kenya, which concluded its commission in 2013). Most of these cases have been the subject of extensive scholarship, which allows us to draw on multiple existing analyses. For instance, data on the effects of the South African Truth and Reconciliation Commission on a variety of outcomes have been made available by scholars from various fields and is especially useful in making inferences about unobservables. The landmark studies by Gibson (2006, 2004, 2002) deem the truth commission as key to South Africa’s transition. The investigation into and reparations for violations likely prevented their repetition. We summarize the consensus on the South African case that in the absence of the TTC, repression would have resumed. The untreated potential outcome is therefore imputed as 1.

On the other end of the spectrum, we consider a case like Nigeria, whose truth commission process was considered a failure owing to “a lack of sincerity on the part of the initiating regime. . . there was deliberate financial strangulation in order to ensure that the Panel became a political weapon in the hands of the President against the potential contenders for the presidency in the 2003 elections” (Yusuf, 2007). It is very unlikely that a truth commission which was used as a political weapon reduced the probability of a resumption of authoritarianism. Such ineffectual and politically captured truth commissions are imputed as  $Y_i(1) = Y_i(0)$ , implying no causal effect of the truth commission on outcomes.

More information on treated cases, however, does not always result in conclusive imputations. In cases of considerable scholarly disagreement, we leave the missing potential outcome unimputed. As a case in point, while scholars have analyzed on the politics of human rights and truth-telling in Chile during and following the commission’s work, it has invited opposing reactions from its domestic parties and scholars. The Chilean case is considered by some to be a creative and successful endeavor in its provision of acknowledgement, reparations, and apologies (Ensalaco, 1994; Brahm, 2005). At the same time, episodes of political violence immediately following the release of the report, its outright rejection by the military and claims that the truth commission served to reinforce political

divisions between the left and the right instead of capturing public imagination (Amstutz, 2005; Quinn, 2001) complicate this picture. Given its varied impact across stakeholders and time, the Chilean case is left unimputed.

### **Step 3: Untreated cases (Non-transitional Truth Commission Cases)**

While we choose to restrict the scope of treatment to include only transitional truth commission cases, not all truth commissions are established at the time of transition, and many come up in the context of consolidated regimes many years after. Following Bakiner (2013), we term cases that set up truth commissions more than three years after transition (and hence do not meet our definition of treatment) as “non-transitional” truth commissions. Case study scholarship focusing on these contexts often provide clues about dynamics around truth and reconciliation options at the time of transition, allowing us to make guesses about what would have happened. In most scenarios where non-transitional truth commissions are set up many years later, we find that the demand for truth commissions exists even at the time of transition, but reason for not establishing a truth commission then is rarely a threat of renewed violence. Instead, it is often a result of continuing political infighting despite a formal transition, limited capacity amid other rebuilding concerns, leadership preferences, military unwillingness or conflict fatigue (USIP, 2011; Vandeginste, 2012; Wiebelhaus-Brahm, 2009*a*). Such cases are imputed as  $Y_i(0) = Y_i(1)$ . Under these conditions, a truth commission (even if not effective in light of these obstacles) is unlikely to make things worse by contributing to a resumption in conflict or authoritarianism.

In some cases, however, a non-transitional truth commission comes up years after transition specifically because of the idea’s outright rejection at the time of transition. In Uruguay, for instance, Roniger and Sznajder (1998) shows how the idea of the truth commission was both, responded to by direct threats of backlash from the military and rejected by the public at large. Specifically, he reports that “for years, the military opposed any opening of the issue of past human rights violations and did not acknowledge any institutional responsibility for abuses committed before 1985, threatening to destabilize the democratic situation if policies of accountability reached the state agenda.” In addition, Uruguay had in place an impunity-imparting Law of Expiry that

provided immunity to the members of security forces involved in acts that constituted violations of basic human rights. After a sustained human rights mass mobilization initiative at the domestic level, a referendum was conducted to decide whether to overturn the law. The results upheld the Law of Expiry by a margin of over 13 points (56.6% in favor and 43.3% against), with a turnout of 84.7%. While these results did not end public discussion and civil society pressure around truth commission creation, this case is emblematic of the dangers that a truth commission could potentially pose if it were transitional in nature.

The imputation is strengthened by a comparison of this case to that of Brazil, which presents a natural match. In this context (also post-authoritarian like Uruguay), a similar Amnesty Law was in effect and was in fact credited with having opened the path to democratization in important ways (Schallenmueller, 2014). However, despite the law, Wiebelhaus-Brahm (2009a) shows that civilians did not reject the idea of a truth commission. Instead, they organized incremental demands for investigation of past human rights, though with very limited success because of the lack of military cooperation. In response, lawyers and other civilians sought the assistance of the Church along with victims' and human rights groups who documented human rights violations to collect information on cases brought before military courts. Based on this information, the São Paulo diocese published the investigation's final report, *Brasil: Nunca Mais*, a collection of allegations of over 1,800 cases of torture and murder committed since the military takeover in 1964. While this report was not officially endorsed nor did it cover all violations, it did not invite military backlash and allowed for human past rights abuses to remain in the public eye during the transition. These observations lead Wiebelhaus-Brahm (2009a) to conclude that in Brazil, "Groups frequently turn over information they have accumulated to truth commissions. A prospective Brazilian truth commission would likely be no different." This statement is not only a fitting representation of scholars of transitional justice making counterfactual guesses about what would have happened had there been a truth commission. It is also an indication that had an official truth commission been established, it would have been able to garner similar information without risking a return to military rule.

#### Step 4: Untreated cases

Lastly, we looked at cases that were truly untreated in that they never received a truth commission. This step represents the toughest case for imputation, given the lack of scholarship about truth commissions in scenarios where they never came up. As a result, this is the step from which the most cases are left unimputed. Within some of these untreated cases, the idea of truth commissions is sometimes brought up by civil society actors, opposition parties and international organizations but not acted upon. Some communities set up their own, unofficial truth-telling processes, while in others, there is no discussion around transitional justice at all. Few studies actively address the reasons behind the failure of a truth commission to be set up, making our task more difficult.

A few examples clarify the type of evidence used to make imputations in this step. First, in a recent account of post-conflict Bosnia, Dragovic-Soso (2016) analyze the reasons behind Bosnia's failure to establish a truth commission. She finds that multiple attempts to establish a truth commission failed on account of resistance by conflict-era leaders to embrace social and political reconstruction. Other reasons included institutional rivalry between the judicial International Criminal Tribunal for the former Yugoslavia (ICTY) and the truth commission project and its lack of legitimacy among Bosnia's domestic victim associations. Given that none of the reasons for the failure of truth commission setup (even by scholars that focused on the specific question) was a threat of return to conflict, and given that the ICTY was already pursuing questions of accountability in the context, it is very improbable that a truth commission would have caused a return to conflict. As a result, the Bosnian case is coded as  $Y_i(0) = Y_i(1) = 0$ .

At the same time, in many military-ruled contexts, the military continues to wield disproportionate influence even after a formal transition is completed. In such a scenario, some scholars and policy-makers have expressed concerns that a truth commission would make fragile transitions more intractable. For example, in Myanmar, the military junta was at the forefront of perpetrating abuses against its minority populations during its civil conflicts in Arakan and Kachin, and it was still in power at the time of their transition to peace, though violence resumed in both cases (Dukalskis, 2015; Smith, 2012). Consequently, a group of country experts meeting in Chiang Mai to discuss Burmese transitions argued that "Burma is not yet ready to follow in South Africa's



footsteps by embarking on a path toward transitional justice...Such a move could even hinder the ongoing process of political reform in Burma” (Naing, June 15, 2012). Based on the experts’ analysis,  $Y_i(1) = 1$  in both post-conflict cases in Myanmar, i.e., TTCs would have caused violence.

Stakeholders from other contexts that did not establish truth commissions often assess potential TTCs by comparing their context to that of other successful commissions. In Angola, for instance, for Transitional Justice (2008) presents the following vignette and analysis: “One respondent asked, ‘Should we (Angolans) all sit at an Angolan TRC? How can we expect Angolans, exhausted from years and years of conflict, to even entertain such an idea?’ The struggle of day-to-day existence leaves very little time for any other issues, including reconciliation. Moreover, concerns were raised that a TRC would taint the government’s record as liberators, compromising the ‘liberation discourse’ so cherished by government since the end of the war.” As discussed above, such analyses show that truth commissions are often not established for reasons unrelated to threats of return to conflict – in this case then, we followed the same logic and imputed  $Y_i(1) = Y_i(0)$ .

## Summary

Figure 2 and Table 3 summarize our results. Before any data collection, the extreme value bounds are 200 points wide – we literally have no information about the average effect of TTCs. After the observed data are collected, the width of the bounds shrink to 100 points because the world reveals half the potential outcomes. The four steps above shrink the uncertainty further as missing potential outcomes are filled in. For the end of conflict cases, the bounds come to  $[-18, 24]$  (only 42 points wide). In the democratization cases, the final bounds are 49 points wide.

For both sets of cases, the bounds include zero. The data and our state of knowledge are currently consistent with both positive, negative, and zero average effects. This is *very importantly* different from a “null” finding. The bounds are as wide as they are because we do not know as much about counterfactuals as we would like. The width of the bounds indicates either what work is left to be done or what counterfactuals are simply to unknowable to be imputed.

Figure 2: Application of QUIMPO to the ATE of Transitional Truth Commissions

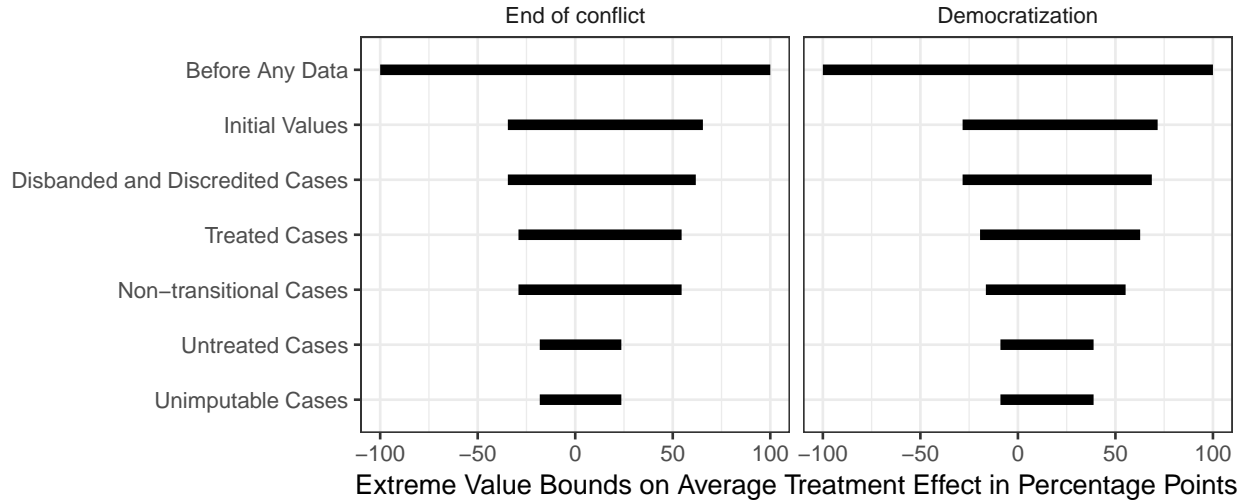


Table 3: Application of QUIMPO to the ATE of Transitional Truth Commissions

	End of Conflict (N = 54)			Democratization (N = 67)		
	Extreme Value Bounds	# Known $Y_i(0)$ s	# Known $Y_i(1)$ s	Extreme Value Bounds	# Known $Y_i(0)$ s	# Known $Y_i(1)$ s
Before Any Data	[-100, 100]	0	0	[-100, 100]	0	0
Initial Values	[-35, 65]	49	6	[-28, 72]	57	10
Disbanded and Discredited Cases	[-35, 62]	49	8	[-28, 69]	57	12
Treated Cases	[-29, 55]	55	9	[-19, 63]	66	13
Non-transitional Cases	[-29, 55]	55	9	[-16, 55]	66	20
Untreated Cases	[-18, 24]	55	32	[-9, 39]	66	36
Unimputable Cases	[-18, 24]	55	32	[-9, 39]	66	36

## Discussion

QUIMPO combines single-case qualitative inference with the extreme value bounds of Manski (1999). The purpose of this procedure is to summarize beliefs in a structured fashion.

Applying this procedure to transitional truth commissions was difficult but rewarding. Possibly the most difficult part of applying QUIMPO was defining potential outcomes. For those places that experienced a TTC, it is clear what is meant by the “treated potential outcome.” But what does it mean if those places did not experience a TTC? Would they have experienced lustrations or purges instead? There are many ways that each of these transitions could have evolved without TTCs and choosing just one for all treated cases was difficult. We followed Hernán and Robins (2016)’s notion of a “target trial,” or what would have happened if at the moment of choosing to do a TTC, we had intervened to stop it.

Another difficulty was defining the universe of cases. We settled on including those cases that had, in our view, a probability of treatment that was not 0 or 1. This choice of course backs up the problem to deciding, among those cases that were not treated, whether they had a positive probability of being treated (and vice versa for the treated group). We acknowledge that others might make different choices about which cases were “eligible” for TTCs.

In the spirit of full transparency, we note that we realized halfway through data collection that the end of conflict and democratization cases could not be analyzed together because they did not share the same outcome variable. While we include this as a “difficulty” we faced, in fact, we felt liberated by the ability to split the analysis on qualitative grounds, rather than trying to force the two sets of units together, possibly by finding a dependent variable they shared in common that might have been less relevant for theory.

Of course, the main difficulty was making guesses about counterfactual states of the world. We were only able to engage with such a large set of cases because of the efforts of previous qualitative scholars of transitional justice.

Stepping back from the application of QUIMPO to transitional justice, we think there are many advantages to summarizing qualitative inferences in this way.

First, we avoid conditioning our analyses on treated cases only. Because units are not randomly

assigned to treatments, the average effect of treatment among the treated need not be the same as the average effect of treatment among the untreated. Our approach avoids the distortions associated with studying treated units only.

Second, we can explicitly account for the nonrandom selection into treatment. Clearly, places that do and do not receive treatment are different from each other, so comparing them (as in an observational, quantitative study) is inappropriate. A series of single- case qualitative studies considers each unit individually so worries about “confounders” are handled directly. Indeed, information about what make each unit special and unique forms the basis for the qualitative inference.

Third, the process is transparent. If critics disagree about an imputation, they can offer a different outcome. If the disagreement is insoluble, we can simply remove the imputation altogether. Disputes over imputations underscore that we do not know the counterfactual, so it would be inappropriate to claim knowledge about a particular causal effect.

Finally, we think that QUIMPO can serve as a way of making disagreements among scholars explicit. Oftentimes, alternative readings of a case have so little in common that even locating the source of disagreement is difficult. QUIMPO asks that scholars state their best guess as to what would have happened to an outcome in particular. In this way, it encourages scholars to be bold in their causal claims.

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