

Dear CPW Participants:

Thank you for taking the time to read and comment on my manuscript. As you review the piece, I'd like to draw your attention to a couple of issue areas that have come up in discussion about the manuscript and are topics that I am presently trying to address.

- Does the study presently infer more support for the theory than can reasonably be assumed given the analysis? Specifically:
  - o What types of rival explanations are there that could compete with the theory presented here? The paper refers to these alternative explanations generically as "Time-varying unobserved variables," but what specific explanations might there be that challenge the perspective given in the paper?
  - o What are the theoretical reasons for believing those arguments over the ones put forward in the paper?
  - o Are there empirical tests that would allow the paper to distinguish between the causal mechanisms proposed in the present study linking and rival explanations accounting for the observed increases in violence?
- Does the paper adequately address the civil war context? Theoretically, does it go far enough in theorizing torture during civil war instead of a general argument of torture? Empirically, are there reasons to expect that the dynamics of civil war that motivate torture might also correlate with a subsequent increase in violence?
- Is the matching process persuasive? What other types of methodological approaches need to be considered?
- Could (should?) the paper's framing be expanded to discuss the role of violence in acquiring violence more generally? Or is a framing more concretely focused on torture preferable?

- **The (In)Effectiveness of Torture for Combating Insurgent Violence**

**Abstract:**

It is commonly believed that torture is an effective tool for combating an insurgent threat and preventing insurgent violence. While torture is practiced widely, little evidence supports these claims. This study provides the first micro-level statistical analysis of the effects of torture. Monthly municipal-level data on political violence are used to analyze the effects of torture committed during Guatemala's civil war. Using data compiled from 22 different press and NGO sources as well as thousands of interviews, the study estimates torture's short-term effects on both state and insurgent violence. While state violence is shown to increase following torture, torture appears to have no discernible effect on the insurgents' ability to perpetrate acts of violence.

Keywords: Torture, Insurgency, Civil War, Repression, Human Rights

Running Head: Torture and Insurgent Violence

Words: 9,989

Torture remains one of the most widely practiced instruments of political repression. In any given year between 1981 and 2010, an average of 80% of states engaged in torture (Cingranelli and Richards, 2011). This includes nearly every state engaged in domestic conflict (Conrad and Moore, 2010). The pervasive application of torture during domestic conflict indicates an enduring belief in the effectiveness of torture for reducing insurgent violence that persists despite a spirited debate on the merits of torture (e.g. Greenberg, 2006). In the course of these debates little evidence of torture's effects on political violence has been provided to justify its continued application. Emerging scientific research on torture has succeeded in identifying a number of structural and behavioral characteristics associated with both the application of torture and its restriction (e.g. Conrad and Moore, 2010; Hill, 2010; Powell and Staton, 2009). However, the inherent secrecy involved in utilizing torture, combined with the challenges associated with evaluating government and dissident behavior at the micro-level, has so far prevented clear evidence of torture's effects from coming to light. As a result, contemporary decisions to engage in torture have not been grounded by systematic analysis, but have instead been made based on the presentation of anecdotal case evidence and the plausibility of various 'what if' scenarios (Hafner-Burton and Shapiro, 2010).

This study provides the first systematic examination of torture's effects on political violence conducted using micro-level data. Monthly municipal-level data on political violence from Guatemala's civil war (1979-1995) are employed to evaluate torture's effects on political violence. Compiled from 21 different press and NGO

sources as well as thousands of interviews, the data allow the study to generate precise estimates of torture's short-term effects on both state and insurgent violence.

The analysis demonstrates that torture is not an effective tool for combating insurgent violence. After torture occurs, dozens of additional victims are killed by the state. However, engaging in acts of torture does not reduce the insurgents' abilities to perpetrate acts of violence against the state or the civilian population.

These results attend to broader concerns over the effects of state repression on challenger behavior. More than 40 years of research into the effects of state repression on dissident behavior produced a range of inconsistent findings (e.g., Davenport, 2005; Hibbs, 1973; Moore, 1998; Rasler, 1996; Siegel, 2011). One explanation for why researchers have been unable to account for how government coercion shapes dissent is that existing studies too frequently rely on measures that combine multiple forms of state repression into a single indicator (see Davenport, 2007; McCormick and Mitchell, 1997). By lumping together a multitude of coercive policies, these studies overlook the possibility that diverse forms of state repression can influence dissent in dissimilar ways. The results of this study demonstrate the value of disaggregating state repression to evaluate the effects of specific repressive tactics.

The paper proceeds as follows: The next section provides a definition for torture and reviews the existing evidence surrounding torture's effectiveness. The third section details theoretical arguments contending that torture is unlikely to produce the types of outcomes its proponents desire. The fourth reviews the data, case and research design. The fifth section presents the empirical analysis. The results are

summarized in the conclusion and implications are drawn connecting the findings to contemporary debates about torture as well as academic research on political conflict.

### **What is Torture and What Do We Know About its Effects?**

According to the United Nation's (UN) Convention Against Torture, torture may be defined as:

[a]ny act by which severe pain or suffering, whether physical or mental, is intentionally inflicted on a person for such purposes as obtaining from him or a third person information or a confession, punishing him for an act he or a third person has committed or is suspected of having committed or intimidating or coercing him or a third person, or for any reason based on discrimination of any kind, when such pain or suffering is inflicted by...a public official or person acting in an official capacity.

The UN definition points to three possible motives for state torture. First, governments employ torture to extract information. States torture captured enemies when they believe those tortured will release secrets that might aid the state in its attempts to identify and eliminate domestic threats (Wantchekon and Healy, 2005). Second, states engage in torture in order to punish. The desired result is to create a link between disobedient behavior and pain, thereby reinforcing legal norms and associating transgression with negative sanctions (Foucault, 1979). Finally, states engage in torture in order to instill a sense of terror in the surrounding population. By promoting the image of state agents as threatening and potentially sadistic, torture is

designed to inspire fear, reduce social trust and control the population (Walter, 1969; Wantchekon and Healy, 2005).

By all three accounts, torture is a tool of political repression committed for the purposes of containing or eliminating dissent. From the perspective of a state seeking to combat insurgency, torture can be viewed as effective when it leads to subsequent depreciations in insurgent violence against the state or the civilian population. But while torture remains widely practiced, little evidence has been provided to suggest that it effectively reduces insurgent violence. In many cases, debates between those who support and oppose torture have been settled based on hypothetical scenarios, rather than empirical facts.<sup>1</sup> Where evidence has been brought to bear on the subject, it has too often been presented anecdotally.<sup>2</sup>

The internal review processes of interrogation organization provide few additional clues. The CIA's (1963: 18) review of its interrogation procedures, concludes that, "in general, direct physical brutality creates only resentment, hostility and further defiance," but the evidence on which this conclusion is based is not explicit. In a more recent review, the Department of Defense charged there has been, "almost no scientific research examining education practices," and that, "scientific investigation of education practices is needed to supplement lessons learned from field experience" (Lehner, 2006: 303).

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1 The most famous of these has been dubbed the "ticking time bomb scenario" (e.g. Dershowitz, 2003; Scheppelle, 2005).

2 For example, torture opponents often identify the case of Abu Zubaydah. According to the FBI, using "conventional interrogation methods," they were able to convince Zubaydah to identify alleged 9/11 mastermind Khalid Sheik Mohammed (KSM). Afterwards, however, the CIA tortured Zubaydah and he stopped talking (Benjamin, 2009). Torture proponents, by contrast, tend to focus on KSM's behavior. Once captured, KSM refused to provide the CIA with any actionable intelligence. However, after being "subjected to an escalating series of coercive methods, culminating in 7 ½ days of sleep deprivation, while diapered and shackled, and 183 instances of waterboarding," he began to cooperate (Finn, Warrick and Tate, 2009).

Examining existing research from within the academy confirms these suspicions. Rejali (2007) devotes two chapters to evaluating torture's effectiveness, relying on social psychological studies, experiments on animals and case material from Algeria to support his positions. But as the author (*ibid.*, 7) admits, "the empirical material [presented]...is...too fragmentary to allow for precise, validated causal claims." Regan (2009) and Walsh (2009) utilize cross-national time-series data to conduct the first statistical analyses of torture's effects. Regan's analyses show that, *ceteris paribus*, torture by a state in a given year is negatively correlated with the number of terror attacks perpetrated the following year, while Walsh' work shows exactly the opposite. The two studies are path breaking in their attempts to bring statistical rigor to address the problem, but the results remain inconclusive. One explanation for the divergent findings is that the level of data aggregation employed limits the ability of the statistical models to clearly identify the causal links between the independent and dependent variables.

Reviewing the literature on state repression more generally provokes questions as to why we would expect torture to suppress insurgent violence. Research has shown that when directing their efforts at repressing dissent, states have had mixed results. Reviewing the existing findings, Davenport (2005: vii) writes that when

(c)onfronted with state repression, dissidents have been found to run away (e.g., White 1993), fight harder (e.g., Eckstein 1965; Feirabend and Feirabend 1972; Gurr and Duvall 1973; Kuran 1990; Khawaja 1993; Francisco 1996), and alternatively run away or fight (e.g., Bwy 1968;



Gurr 1969; Gupta and Venieris 1981; Lichbach and Gurr 1981; Weede 1987; Rasler 1996; Moore 1998).

One explanation for the mixed findings is that existing studies of state repression generally lump together all types of political repression (e.g., political arrests, torture, disappearances, massacres) into a single indicator, which distorts the ability of analysts to properly identify repression's effects. As a result, we do not know whether particular repressive tactics, such as torture, have consistent effects, or what those effects might be. At present all that has consistently been shown is that the combined forms of state repression can inspire a multitude of insurgent responses.

### **The Ineffectiveness of Torture for Combating Insurgent Violence**

Without systematic analysis of the ways in which torture affects both state and non-state violence, policy makers and scholars have no way of knowing whether torture will yield the intended effect of suppressing insurgent violence. This section presents arguments challenging the effectiveness of torture as tool for reducing insurgent violence. The contention is not only that torture is ineffective for reducing insurgent violence against the state or the civilian populace but also that engaging in torture leads to an escalation of state violence.

Generating expectations of torture's short-term effects on political violence committed by the insurgency and the state is different from generating a theory of whether torture leads to confessions and denunciations. But focusing on how torture affects state and insurgent violence provides both a more exacting theory and more demanding tests than an analysis of torture's immediate effects, such as a theory of

confession and denunciation.<sup>3</sup> Justifications for torture do not rest on the contention that engaging in torture will reveal information, but on arguments that engaging in torture will allow state agents to somehow stop insurgents from engaging in violence (e.g. Bagaric and Clark, 2007; Derschowitz, 2003). If torture cannot produce discernible effects on insurgent violence then any immediate effects of torture, including the revelation (or non-revelation) of information, are of little consequence.

Torture has the potential to shape subsequent violence through multiple pathways. Because states engage in torture to reduce insurgent activity, agents of the state can be expected to act on any information that is revealed by taking steps to contain or eliminate any individuals or insurgent actions identified. While some steps taken following torture may be relatively non-violent, such as rendition and detention, torture has a number of properties that prioritize violence in the ensuing actions of the state. Torture simultaneously breaks down psychological and organizational constraints on future state violence while also promoting new professional incentives for an escalation in repressive action. Psychological research has shown that engaging in torture dehumanizes victim groups and weakens moral constraints against violence (e.g., Harris and Fiske, 2011). Concurrently, state torture signals a system of oversight and reward on behalf the military command that can provide new incentives for engaging in violence. Professional members of the military can come to see increased repression as a means to promotion or reward, while more sadistic members will notice that mechanisms of control limiting the application of violence have been lifted (Wantchekon and Healy, 2005). Decisions to use torture may even be made

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<sup>3</sup> Simultaneously, analyzing short-term effects allows the study to generate tighter causal claims than an analysis of long-term effects, such as torture's effects on racial subordination (e.g., Rejale, 2007).

intentionally to encourage future acts of violence by members of the military (Conrad and Moore, 2010; Mitchel, 2004). As constraints are lifted and incentives for engaging in violence increase, the amount of political repression applied following torture will increase.

— Hypothesis 1: Engaging in torture will increase state violence.

While torture is predicted to increase state repression, it is expected to be ineffective for reducing subsequent acts of insurgent violence against the state or the civilian population. First, torture is unlikely to reduce insurgent violence by itself. While it has been argued that torture can create a sense of fear that paralyzes potential insurgents (e.g. Walter, 1969), such arguments do not address the range of strategic options available to a repressed population seeking safety in the midst of shifting security conditions. Even among the truly fearful, torture often acts as a transformative event, inspiring a sense of backlash among the repressed (Francisco, 2004; Hess and Martin, 2006). Rather than be deterred from action against the regime, victim groups can turn to the insurgency both because joining increases their chances for survival (Kalyvas and Kocher, 2007) and because insurgent groups offer opportunities for organized opposition to future repressive action (Wood ,2003).

Second, the ensuing repression that torture generates is expected to be ineffective for reducing insurgent violence. Information starved counter-insurgents often torture individuals with little or no knowledge of the insurgency, which can lead to poorly targeted repression and an insurgent backlash.<sup>4</sup> In order to cease their

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<sup>4</sup> For similar reasons, the application of torture applied as a punishment is unlikely to act as a deterrent (e.g., Foucault, 1979). For torture to deter it would have to be applied selectively (Kalyvas, 2006). The available evidence suggests that selective torture

suffering, tortured individuals who do not know the answers to the torturer's questions may decide to give up the names of innocent victims. They may even seek to turn subsequent repression to their advantage by identifying individuals with whom they share private grudges (Kalyvas, 2006). Verifying the claims made by victims of torture takes time and in situations where time is at a premium the torturers may feel pressure to act without corroboration.<sup>5</sup> Insurgencies, meanwhile, are constantly adapting. Built into the insurgents' modus operandi are plans for minimizing the damage inflicted should one of their members be tortured. These include both proactive steps, such as compartmentalizing information and organizing in a cell structure, as well as reactive steps, such as shifting to new hideouts and altering their tactical repertoire in the event that one of their members is captured (see Rejali, 2007: ch 7).<sup>6</sup> The result is to counter the potential for repressive actions taken following torture to effectively target the insurgents or limit their capacity to perpetrate attacks on the state or civilians.

— Hypothesis 2: Engaging in torture will not decrease insurgent violence.

## **Research Design**

One explanation for why there has been so little success empirically evaluating the effects of torture is that estimating torture's causal effects is an effort fraught with methodological challenges. Two threats to causal inference are particularly noteworthy and need to be discussed: the observation problem and the

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is extremely difficult during an insurgency. One study of U.S. torture in South Vietnam suggests that 80% of those tortured were innocent victims (Otterman, 2007).

<sup>5</sup> Simultaneously, torture may undercut alternative means for gathering information, such as the use of double agents or informants.

<sup>6</sup> In this way, torture produces different liabilities than other forms of information collection, such as direct surveillance or *agents provocateur*, because insurgents often know when it is taking place.

endogeneity problem. The observation problem presents challenges because (1) torture generally takes place under conditions of secrecy and (2) systematically collecting data on insurgent and counter-insurgent behavior is extraordinarily difficult. The effect is to constrain observations of torture and its potential outcomes, as researchers must counter both the strategies employed by governments to hide the use of torture and the challenges associated with collecting data in a conflict ridden or post-conflict society.

At the same time, we know that the processes generating political violence are highly endogenous (Kalyvas, 2006). Generating counter-factual claims of what an observed situation of torture would look like had torture not taken place becomes difficult because the endogeneity of political violence processes separate out violent and non-violent areas. Processes that lead to initial instances of violence set in motion feedback mechanisms that, in turn, shift the strategies of the warring parties to inspire or restrain future acts of violence. It would be incorrect, therefore, to assume that the probability of experiencing torture is independent of prior experience with the dependent variables of interest.

Countering the observation and endogeneity problems requires combining detailed records of torture and other forms of political violence with methodological techniques capable of addressing biases resulting from the non-random application of torture.

#### *Case Selection and Data*

To empirically evaluate the effects of torture, micro-level data on political violence from Guatemala's civil war (1979-1994) are utilized. The insurgency in

Guatemala, largely organized under the banner of the Army of the Poor (EGP), sought to overthrow the military government and impose a communist regime through a combination of popular mobilization and a hit and run strategy targeting government forces (Stoll, 1993). Throughout the war the Guatemalan state engaged in high levels of political repression, which included the widespread application of torture (Ball et al., 1999; CEH, 1999).

According to Guatemala's Commission for Historical Clarification (CEH, 1999: 489), torture was committed, "with the primary objective of obtaining information...[a]s an additional purpose, it was used to punish the victim, and to generate terror in the population."<sup>7</sup> When it came to torture, the Guatemalan military was both highly skilled in the techniques of torture and unconstrained in its application (Schirmer, 2000). During the 1960s, hundreds of Green Berets and CIA operatives were sent to Guatemala to restructure the military's intelligence apparatus, and train the agents of the military intelligence unit, the G-2, in the application of different torture techniques using lessons learned in Vietnam.<sup>8</sup> At the same time, when civilian efforts attempted to curtail the military's operational independence, military power was able to subvert these attempts through a series of coups and states of emergency (Schirmer, 2000). The result was to produce a military force with both the technical knowhow to engage in torture and the ability to apply that knowledge wherever they saw fit. Among other tactics, agents of the Guatemalan military forced the victims to stand hooded for hours or days, forced them to eat excrement, forced

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<sup>7</sup> These motives appear consistent with those derived from the UN definition above

<sup>8</sup> See Jonas, 1996; McClintock, 1985; Rejali, 2009; Schirmer, 2000. In addition to US training and support, the Guatemalan intelligence unit was supported by aid and training from the Argentine, Colombian, Taiwanese, Chilean, and Israeli intelligence forces (Schirmer, 2000: 152).

them to stay awake for days at a time, refused to give them food or water, subjected them to electric shocks, stripped them naked, burned them with cigarettes, suspended them from chains, sexually abused them, submerged them in water, cut them and broke their fingers (CEH, 1999: 472-489).

In the end, the overwhelming levels of repression imposed by the Guatemalan state worked to effectively eliminate insurgent violence within the country (Stoll, 1993). In this way, the country potentially serves as a “most likely case” for observing the effectiveness of torture as an instrument of counter-insurgency (Przeworski and Teune, 1976). Guatemala is a case where (1) torturers were expertly trained and unrestrained and (2) the repressive actions of the government ultimately undercut dissident mobilization and put an end to insurgent violence. Alternatively, it may be the case that examining aggregate indicators of repression and dissent tell us little about the effects of individual tactics such as torture. While we know that increases in overall levels of repression led to decreases in overall levels of insurgent violence, without disaggregating the different repressive tactics engaged in by the state it is impossible to know whether torture operated as an effective instrument for reducing insurgent violence.

The empirical analyses conducted below employ municipal-level events data on political violence in Guatemala collected from 17 different domestic press sources, the publications of five human rights organizations (Human Rights Watch, Amnesty International and three Guatemalan human rights groups), and the transcriptions of more than 5,000 interviews with victims of the conflict (Ball, 1999). The dataset was constructed by an NGO, The Center for Human Rights Research or CIIDH, based in

Guatemala City during the truth and reconciliation processes that took place in Guatemala in the 1990s (Ball et al., 1999; Ball, 2001). The CIIDH's mission was to collect local, event-level information on human rights abuses committed throughout the country for during nearly 40 years (1979-1996) of civil war (Ball et al., 1999). The project, which was directed by Patrick Ball and the Benetec Human Rights Data Analysis Group, began with a thorough review of daily press reports and published human rights documents for the purpose of identifying incidents of human rights abuses committed by state forces, insurgents and other militant actors. Abuses reported in these sources were recorded using the "who did what to whom" structure (Ball, 2001). The press sources were then supplemented with thousands of interviews, which began in 1994 among the survivors living near the Mexican border (Ball et al., 1999). Using snowball techniques based on these interviews, the team identified areas of high reported violence throughout the country and spent two years interviewing victims and witnesses in these areas and other reported sites of violence (ibid.). Events from the different sources were then combined into a single database and events reported in multiple sources were identified and deduplicated.<sup>9</sup>

The combination of multiple, partially overlapping sources cataloging political violence presents a more accurate portrayal of the distribution of violence across Guatemala than any one source would have on its own, and helps makes the dataset one of the most complete sets of event data from a civil war available to date. In their analysis of the data, Davenport and Ball (2002) find that the strengths of each of the different data sources help to compensate for the weaknesses of the others. For

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<sup>9</sup> Ball (2001) provides evidence that the snowball sample and a regionally stratified sample yielded similar results in comparison analyses.



example, the authors show that newspaper data tend to over report disappearances and violence located near urban centers. By contrast, the human rights reports tend to highlight large events, while the interview data captures smaller, day-to-day forms of violence and violence conducted in the rural regions of the country (Davenport and Ball, 2002). Taken together, the sources combine to produce more complete coverage of the variations in type, scope, intensity and location of political violence than alternative events-databases constructed around a single data source.

In total, the database records nearly 18,000 incidents of political violence, including massacres, selective killings, disappearances and torture.<sup>10</sup> More than 45,000 individual victims of political violence are identified, of which nearly 500 were victims of torture.<sup>11</sup> Like all events databases collected during civil wars, the number of events are reported in the dataset undercounts the total number of events that took place in the civil war.<sup>12</sup> While there is no evidence to suggest that underreporting would be correlated with the underlying distributions of either the independent or dependent variables, danger lies in the possibility that the data sources share some underlying bias that is not addressed through their combination and that the data may systematically undercount violence during particular periods or in

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10 Entries in which a victim was tortured and then killed are still recorded as torture, with the death being recorded in another column.

11 Killings thus appear to be far more common than incidents of torture, which confirms the need to analyze torture in relation to other tactics of repressive violence.

12 When these events are collapsed down to the municipality and month in which they took place, it yields a sample with 142 unique municipality-months experiencing torture in Guatemala between 1979 and 1996, each of which had between 1 and 15 individual torture events recorded. Given that the government had an identifiable interest in publicizing its most successful instances of torture (to frighten the populace) and the insurgents had an identifiable interest in publicizing the least successful instances (to enrage the populace), the most likely missing cases are those lying in the middle, which were carried out without publicity. Whether these events are sufficiently different from those covered in the dataset as to potentially bias the results is an empirical question, and, regrettably, one that cannot be answered with the available data.

particular areas. To address potential biases imposed by non-random data collection, the models below employ a matching technique to condition the estimates on the reporting patterns found in the dataset. More details on these models are presented in the identification section.<sup>13</sup>

### *Operationalization*

*Dependent Variables* – The units of analysis for the models below are municipality-months.<sup>14</sup> The dependent variables are event counts of the number of state and insurgent killings measured over two different time intervals: one month in the future and six months in the future (i.e. month  $t+1$  and the sum of months  $t+1$  to  $t+6$ ). To examine torture's effects on state violence, the study analyzes the number of individuals subsequently killed by the state within the municipality. All state killings are occurring in the municipality are included in the analysis. To examine torture's effects on insurgent violence, two sets of models are run. The first estimates the number of individuals killed by the insurgency in a given municipality. The second estimates the number of individuals killed by the insurgency in municipalities neighboring a given municipality, weighted by the number of neighbors that

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<sup>13</sup> More extensive information on the strengths and weaknesses of the CIIDH data are laid out in Ball, 2001; Davenport and Ball, 2002. Additional discussion of the methods and data is presented in XXXXXX, 2010.

<sup>14</sup> Municipalities are Guatemala's second smallest administrative unit. In 1981, municipal populations varied from 464 (San Jose Jacaya) to 754,243 (Guatemala City). Of the 326 municipalities, 63 experienced at least one incident torture. Of those, most experienced only one incident. In general, the victims of torture appear to have been residents of the municipalities where they were tortured (CEH, 1999; Ball, 1999). Three municipalities (Santa Cruz Mulua, Rabinal and Guatemala City) experienced extremely high levels of torture, accounting for approximately 40% of all recorded torture incidents. Guatemala City is potentially problematic because it is the site of the police headquarters and is the one location where individuals were brought from other municipalities to be tortured (CEH, 1999). To ensure that that the results were not being driven by Guatemala City or the other two high torture municipalities, they were dropped sequentially and the models were re-estimated. Results proved substantively identical.

municipality has (Ward and Gleditch, 2008).<sup>15</sup> *Independent Variable* – The independent variable, torture, is identified by the CIIDH/AAAS dataset. The dataset used the following definition, which roughly matches the UN definition put forward above: “inflicting pain on a person in a premeditated and systematic manner...[where] the apparent motive is to obtain information or confessions, whether they are true or not.” Months in which the state engaged in at least one act of torture in a municipality are scored 1 and scored 0 otherwise.

*Control Variables* - Five controls are used in the analysis.<sup>16</sup> First, controls for other forms of state violence are incorporated into the design to ensure that the analysis does not confuse torture’s effects with the effects of other tools of repression. Specifically, two other forms of state repression are controlled for: selective state killings and state massacres, both measured at time t. Including these tactics (which are commonly lumped together) allows the analysis to identify torture’s effects net of other forms of ongoing repression. Second, a spatial lag of torture is included to control for the spatial clustering of the independent variable (see Ward and Gleditsch, 2008). This variable is measured as the amount of torture occurring at time t in among neighboring municipalities, weighted by the number of neighbors a municipality has. Third, a count of the number of insurgent killings at time t is included to control for the fact that subsequent acts of both state and insurgent

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15 If we only looked locally, the analysis might miss identify torture’s effects because torture lead insurgents to shift locals and engage in violence elsewhere. Neighboring municipalities are defined as those within 10 miles of one another. This metric was used as a cut point to estimate economic and social ties between closely neighboring municipalities. Alternative model specifications identified torture’s effects more broadly by employing unweighted inverted distance measures to measure regional effects. Results proved substantively identical to the models presented below.

16 These variables function as parameterized control variables instead of operating as a part of the non-parametric matching procedure in the second series of models below because the inclusion of additional matching requirements would have coarsened the data too finely, which would have resulted in an excess of treated cases being dropped from the analysis.

violence are often a response to prior insurgent killings (e.g. Davenport, 1995; Moore, 1998). Fourth, the log of the municipality population is included to control for the fact that violence in cities varied systematically from violence in the countryside. Finally, measure of the municipality's percent indigenous controls for the fact that state violence was often directed at the indigenous.<sup>17</sup>

### *Identification Strategy*

To estimate the effects of torture on subsequent patterns of state and insurgent violence, two sets of models are analyzed below: a set of traditional cross-sectional time-series OLS regressions and a set of matched sample difference-in-difference estimations.

*Cross-Sectional Time-Series OLS Regression* - The first series of tests run to estimate torture's effects are a set of OLS regression models employing a cross-sectional time-series design (Beck and Katz 1995). OLS was selected over alternative event-count estimation procedures, such as Poisson or Negative-Binomial regression, both because the OLS models provide a better baseline for comparison against the matched sample difference-in-difference models below and because the structure of the dependent variables analyzed in this study violates a key assumption of count models.<sup>18</sup> At the same time, the binary nature of the treatment variable (torture or no torture) means that the independent variables in the OLS models are saturated. Much

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<sup>17</sup> Alternative model specifications dropped the time-invariant controls and included municipal fixed-effects. Results proved substantively similar.

<sup>18</sup> As Valentino, Huth and Croco, (2006) point out, "count models assume a data-generating process based on a sequence of identical Bernoulli trials in which the outcomes of the trials are binary (for example, success or failure, heads or tails)." As in the Valentino et al. study, this study's dependent variables are not a successive series of individual trials resulting in individual deaths but highly varied processes of violence ranging from the targeting and killing of an individual to the firebombing of entire villages. To check for robustness, the OLS models were also run with bootstrapped standard errors, which uses the observed distribution of the variance to calculate the standard errors eliminates the normal distributional assumptions.

as linear probability models are often selected over Logit or Probit when models are saturated, in this setting OLS provides reasonable estimates based on the observed difference in means between treated and control observations (Agresti, 2007).

There is real concern, however, that the non-random application of torture could bias the results of the OLS estimates. Table I displays the average count of violent events occurring over the preceding six months both for municipality-months experiencing and not experiencing torture. The table shows that the municipality-months experiencing torture witnessed much higher rates of violence in the months leading up to the application of torture. If the higher rates of violence in municipality-months experiencing torture not only helped provoke torture but also inspired increases in other forms of violence, the models could severely over-estimate torture's effects. Alternatively, if the political violence is generally mean stationary, such that levels of violence fluctuate and then revert back to a mean, comparing the more violent periods in which torture was applied to less violent periods where it was not could lead to an underestimate of torture's effects.

#### **Table I About Here**

*Matched Sample Difference-in-Difference* - To address the non-random application of torture and generate better estimates of torture's effects, the study's research design combines difference-in-difference estimation with a non-parametric matching procedure (compare Lyall, 2009; 2010). The design works to first model the dynamics of violence operating in a municipality and then assess how torture affects those dynamics through comparisons of changes in the rates of violence within municipalities experiencing and not experiencing torture. Matching ensures that

observable trends in political violence are similar between municipality-months where torture took place (the treatment group) and municipality months where torture did not take place (the control group). The difference-in-difference models estimate changes in those trends, controlling for any systematic (i.e., time invariant and uniform within group) unobservable differences between the two groups.

Difference-in-difference models estimate torture effects on the dependent variables by comparing trends in violence occurring in municipalities experiencing and not experiencing torture. The idea is to use the amount of change in sites unaffected by torture as an estimate for changes in violence would look like had torture not taken place. To inform such calculations, the models estimate the amount of violence occurring both before and after the intervention (i.e., torture or no torture) and include two novel dummy variables: one identifying the treatment group (municipalities experiencing torture) and one identifying the post-intervention period (one month or six months following the application or non-application of torture). The first dummy variable controls for systematic, time invariant (and unobservable) differences that exist between the treatment and control groups.<sup>19</sup> The second controls for temporal effects that are common between the two groups.

By interacting these two variables, the models identify reliable estimates for the average treatment effects (ATE). The ATE effectively estimates any changes in the dependent variables that follow torture within tortured municipalities, while controlling for both any unobservable differences that exist between the treatment and

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<sup>19</sup> Temporally variant unobservable differences, such as changes that take place in municipalities experiencing torture in the same month in which torture was applied, cannot be addressed using this control, nor can idiosyncratic unobservable differences located in some municipalities in the treatment group.

control groups and for any systematic trends occurring across time periods. Formally the difference-in-difference model is constructed as:

$$Y_{i,s,t} = \alpha + \gamma D_s + \lambda T_t + \delta(D_s * T_t) + \beta X_{i,t} + \xi_i + \mu_t \quad (1)$$

$D$  are treatment group fixed effects,  $T$  are post-intervention fixed effects,  $\delta$  estimates the ATE and  $X$  are control variables. The subscript  $i$  refers to spatial identification,  $t$  refers to time period and  $s$  refers to treatment group.

So long as temporal trends in the treatment and control group are roughly comparable, comparing rates of change within the two groups allows the models to effectively control for any systematic unobservable differences that may exist prior to treatment. However, if the two groups have inconsistent temporal trends, estimates of the treatment effect may remain biased (Angrist and Pischke, 2009). To ensure that temporal trends in the two groups resemble one another, the difference-in-difference models incorporate a matched sample design that matches treated and untreated municipalities with similar pre-intervention trends in observed levels of political violence (Card and Sullivan, 1988). Effectively, combining the matched sample with difference-in-difference estimation allows the study to first model the endogenous dynamics driving political violence in a municipality before assessing how torture affects those dynamics. The matching process ensures that the observable differences in pre-intervention histories of violence are comparable between the treatment and control groups, while the controls in the difference-in-difference design ensure that any systematic unobservable differences between the two groups are not biasing the results.

The method of coarsened exact matching (CEM) is used to match treated municipalities (i.e. those experiencing torture) with control municipalities (i.e. those not experiencing torture) based on their experiences with political violence over the previous six months (Iacus, King & Porro, 2012). For a given municipality month, each of the prior six months was coded dichotomously for experience with violence, with experience coded as 1 regardless of who perpetrated the violence. Using a technique developed by Card and Sullivan (1988), the six variables were then concatenated into a single variable that was used to categorically match treated and control units.<sup>20</sup> 1:N matching is employed, where N signifies the number of municipality months that fit within each matching stratum.<sup>21</sup> Returning to Table I to compare the trends for the matched sample reveals that the two groups had exactly comparable conflict histories.<sup>22</sup>

As an example of how this process works, Santa Cruz, which experienced torture in June 1981, was matched with Uspatan, which did not experience torture in September 1981, because the sequence of violence for Santa Cruz from December 1980-May 1981 (100001) exactly matched the sequence of violence for Uspatan from

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20 Categorical measures of “conflict histories” were selected over a single scalar measure because categorical measures are able to capture the appearance and disappearance of violence in a municipality with greater nuance. A scalar measure, such as the change in levels of violence over the previous six months or average number of killings, would fail to capture whether violence had been increasing or decreasing in more recent months, as well as whether violence was constant or sporadic in the municipality.

21 The matching procedure further stipulated that each treated municipality month only be matched with untreated municipality months that experienced similar conflict histories in the same department and the same year as the instance of torture. To ensure that members of the treatment group were not smuggled into the control group at later points in time, during the matching procedure months without torture in treated municipalities were dropped if they occurred within the six months prior to or following the instance of torture. This is an adaptation of the method used in Simmons and Hopkins (2005), who match only within panels, dropping the rest of the data during the matching process.

22 Causal effects cannot be identified in settings where there are no treated members of the population in a stratum or where all members of the stratum population are treated. As a correction, the matching process prunes off observations for which common support is not available. Greater detail on pruning rates across strata is provided in Appendix II.



March 1981-August 1981 (100001).<sup>23</sup> The models then compare rates of change before and after June 1981 in Santa Cruz to rates of change before and after September 1981 in Uzputan by incorporating the matching strata into the difference-in-difference design. Conditioning the estimates on the additional term  $\partial V_m$ , where  $V$  signifies the fixed-effects of each conflict history matching strata, generates estimates of torture's effects on changes in political violence based on comparisons between municipalities that have exactly matched conflict histories. The full model is specified as:

$$Y_{i,s,t,m} = \gamma D_s + \lambda T_t + \delta(D_s * T_t) + \beta X_{i,t} + \partial V_m + \xi_i + \mu_t \quad (2)$$

Including conflict history fixed effects in the matched sample models also helps to control for processes in the data collection efforts that may have led to systematic under reporting in specific locations or during specific time periods. By conditioning the estimates on the conflict history fixed-effects, the matched sample models ensure that estimates are based on comparisons between cases with identical reporting patterns in the recent past.<sup>24</sup> The matched sample models can thus be interpreted as using the amount of violence recently recorded in the dataset as an estimate of the processes generating the reports in order to ensure that violence in the control and treatment groups had a similar probability of being reported. To produce reliable results, these models do not require the assumption that reporting was

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<sup>23</sup> See Appendix I.

<sup>24</sup> Much as traditional fixed-effects models include unit-level dummy variables to estimate deviations from the unit mean, the matched sample models include strata-level fixed effects to estimate relative changes conditional on the mean rate of observations recorded within each conflict history matching strata (Angrist and Pischke, 2009).

uniform across the entire set of municipality months but only that it was uniform within matching strata for short periods of time.

## **Results and Analysis**

Table II presents the results of the cross-sectional time-series OLS models estimating torture's effects on state and insurgent violence. For each independent variable included the coefficient estimates are presented along with the standard error and level of statistical significance.

### **Table II About Here**

Models 1a and 1b estimate torture's effects on the number of individuals killed by the state for one month following and six months following intervention, respectively. The first thing to note is that the beta coefficient for torture's effects on the number of killings committed by state agents six months after torture is positive and statistically significant. From the model, it is estimated that state agents are, on average, going to kill nearly 100 more individuals after engaging in torture. Even in Guatemala, where the state was responsible for killing hundreds of thousands of civilians, this is a very high figure. During the period under review, the state killed an average of 1 person in a municipality every two months. An estimated increase of nearly 100 state killings in the six months following torture provides strong evidence that engaging in torture motivates state agents to commit more killings. Model 1a, by reference, predicts an average increase of 32 state killings in the month following torture. And while the coefficient for this variable does not approach conventional standards of statistical significance, the predicted P value is less than 0.12.

Analyzing the effects of torture on insurgent violence, Models 2a, 2b, 3a, and 3b in Table III estimates how torture affects the number of individuals killed by the insurgents. If torture is an effective instrument of counter-insurgency, we should expect to see a decrease in the number of individuals killed by the insurgents locally (Models 2a and 2b) and in a municipality's neighborhood (Models 3a and 3b) following torture.<sup>25</sup> But the results of the cross-sectional time-series models provide the first challenge to arguments that torture is effective for reducing insurgent activity. Estimates of insurgent violence do not change significantly following torture. This is true at the municipal and regional level for both time periods analyzed, which suggests torture has no significant impact on insurgent abilities to commit violence

Other forms of repressive violence do appear to influence subsequent insurgent behavior. Most significantly, in Models 2b and 3b state massacres are positively related to increased insurgent killings during the six months after the massacre. Such evidence validates arguments that indiscriminate state repression does not work to reduce insurgent killings (e.g., Kalyvas and Kocher, 2007). At the same time, the other forms of political repression included as controls are positively and significantly related to subsequent state killings. Such evidence suggests that, just like torture, other forms of repression have habituation effects that can escalate subsequent state violence (see Gurr 1986). Combined, this evidence helps sustain several of micro-dynamics supporting the theoretical arguments above. As argued,

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<sup>25</sup> The low  $r^2$  values for these models may be of concern to some scholars, as a low  $r^2$  is often interpreted as a sign of poor model fit. However, this challenge is difficult to sustain since for each dependent variable there is no a priori way of estimating the distribution of  $r^2$  values and thus no way of knowing whether a given model is significantly poorly suited for estimating that variable (Achen, 1982).

engaging in repression begets more repression while engaging in repression targeting civilians as well as insurgents is ineffective.

### **Table III About Here**

The results from the matched sample difference-in-difference model, presented in Table III, are consistent with the findings from the cross-sectional OLS models and provide further confirmation that state violence increases following torture, while levels of insurgent violence are apparently unaffected. The models include two important controls—one for systematic unobservable differences between the treatment and control group and one for systematic differences that exist in both groups before and after the intervention. Interestingly, across the models the “Treatment Group” variable is only significant in one model (Model 4b), which suggests that there are few significant systematic unobservable differences between the matched samples of municipality-months experiencing and not experiencing torture. The “Post-Intervention Period” control is significant across four of the six models. This suggests that while the matching process has identified comparable samples of treatment and control municipality-months, both groups are experience common temporal dynamics would bias the results if not controlled for.

Turning to the substantive results, Models 4a and 4b estimate torture’s effects on subsequent state killings. The proposition that state violence will increase following torture receives robust support in both models. As in the OLS models above, state killings are estimated to significantly increase following torture. Substantively, the predicted number of state killings in a municipality is expected to

increase by 32 individuals in the month following torture and by 24 individuals over the next six months. That the estimated effect over six months is lower than the estimated effect over one month may seem contradictory to some readers. But if we consider the idea that once state agents engage in violence following torture it changes the incentives for engaging in additional acts of state violence, it is easy to reconcile these findings.<sup>26</sup> State agents may conclude that there are fewer potential insurgents remaining following the initial targeted attacks or these agents may move on to other municipalities after having identified and killed dozens of presumed insurgents. In any case, what is important to recognize is that both models predict that following acts of torture state agents will target and kill dozens of additional individuals.

Finally, Models 5a and 5b estimate torture's differential effects on local levels of insurgent killings, while Models 6a and 6b estimate torture's differential effects on neighboring insurgent killings. These matched sample difference-in-difference estimates provide further challenge to those arguing that torture can decrease insurgent violence. Torture is shown to either produce an increase in insurgent killings or have no significant effect on the insurgent violence. In Model 5a, the predicted number of insurgent killings in a municipality's neighborhood increases significantly in the month following torture. But it should be noted that while torture is shown to have positive and statistically significant effect on estimates of insurgent killings in neighboring municipalities, the substantive interpretation of effect is

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<sup>26</sup> To confirm these suspicions, difference-in-difference estimates for changes in state violence three months following torture were also generated. As predicted, these estimates fell between the six month and one month figures. Over the three months following torture, torture is predicted to increase the number of selective state killings by 28 individuals.

negligible. The state would have to engage in more than 40 acts of torture in a municipality to inspire an additional insurgent killing in that municipality's neighborhood the next month. But when we combine the positive effects of torture in this model with the insignificant effects of torture on insurgent violence estimated in Models 5a, 5b and 6b, the evidence from the matched sample difference-in-difference analysis provides strong evidence to support the theory that torture is ineffective for reducing insurgent violence.

In summation, while state violence increases significantly following acts of torture, torture has no discernable effect on insurgent violence.

## **Conclusion**

Using micro-level data on political violence from Guatemala and a research design that combined matching procedures with difference-in-difference estimation, this study estimated the effects of torture on subsequent patterns of state and insurgent killings. The results of the tests provide strong evidence that torture is an ineffective tool for counter-insurgency. Levels of insurgent killings both locally and within the surrounding region appear unaltered following the state's use of torture. Meanwhile, the state was shown to kill dozens of additional individuals after it engaged in torture. The conclusions to be taken the study are that not only is torture an ineffective tool for reducing domestic threats, but also that it leads to unnecessary and futile violations of human rights.

In addition to shining light on the effects of this controversial policy, these results have important implications for research on the relationship between repression and dissent. By estimating torture's effects, while controlling for other

forms of state repression, the results challenge existing work that lumps torture in with other policies and push such work to begin disaggregating the effects of different repressive tactics. Recent studies have investigated the effects of military sweeps (Lyall, 2010), indiscriminate shelling (Kocher, Pepinsky and Kalyvas 2011; Lyall, 2009), siege operations (LaFree et al., 2009), leadership decapitation (Bob and Nepstad, 2007) and arrests (Barkan, 2006). Collectively, this research has shown that the tactical choices made by the state produce divergent responses on the side of the insurgents. So far, however, research into repressive tactics has failed to reference either other studies in this vein or repression research more generally. As a result, the links between such work are unclear and we do not know how different repressive policies might interact, substitute for one another or have varying effects depending on the context in which they are applied.

Integrating the findings of policy level studies, such as this one, with the broader literature on repression would contribute to our understanding of how states and challengers interact in two ways. First, this line of research would provide clues as to why states have been shown to uniformly increase levels of repression in response to acts of dissent, even where repression could potentially prove counterproductive. Second, such studies will help to account for when and where repression does succeed in limiting dissent. States are the victors of an overwhelming majority of domestic conflicts. This leads to the conclusion that some policies of repression must be effective, but at present we know little about what form these policies take or what contexts make various types of repressive activity more or less effective.

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**Table I: Average Counts of Violent Events Experienced in Previous Months**

		<b>N</b>	<b>Six Months Prior</b>	<b>Five Months Prior</b>	<b>Four Months Prior</b>	<b>Three Months Prior</b>	<b>Two Months Prior</b>	<b>One Month Prior</b>
<b>Full Sample Average</b>	Municipality- Months Not Experiencing Torture	62,258	.017	.017	.017	.017	.017	.017
	Municipality- Months Experiencing Torture	142	.408	.324	.380	.430	.408	.422
<b>Matched Sample Strata Weighted Average</b>	Municipality- Months Not Experiencing Torture	6,011	.087	.101	.072	.130	.130	.101
	Municipality- Months Experiencing Torture	69	.087	.101	.072	.130	.130	.101

**Table II: The Effects of Torture: Cross-sectional Time-Series Estimates**

	Torture's Effects on Selective State Killings		Torture's Effects on Local Insurgent Killings		Torture's Effects on Regional Insurgent Killings	
	Model 1a t+1 Month	Model 1b t+6 Months	Model 2a t+1 Month	Model 2b t+6 Months	Model 3a t+1 Month	Model 3b t+6 Months
<b>Torture</b>	32.483 (20.836)	98.833* (42.986)	-6.002 (6.235)	49.291 (49.188)	-3.618 (2.598)	-5.172 (10.001)
<b>Selective State Repression</b>	17.813*** (5.321)	86.825*** (13.900)	5.120 (5.025)	14.294 (9.754)	2.941 (2.089)	9.756 (5.212)
<b>State Massacres</b>	47.698*** (13.904)	183.357*** (28.405)	15.969 (15.960)	76.866* (35.106)	9.329 (6.634)	34.792* (14.179)
<b>Spatial Lag</b>	2.689 (6.662)	25.767 (35.391)	-13.895 (13.936)	23.069 (46.742)	-2.003 (1.478)	9.753 (12.512)
<b>Insurgent Violence</b>	-1.683 (7.829)	175.222* (83.215)	-3.990 (4.175)	-24.391* (10.676)	-2.400 (1.759)	8.123 (20.368)
<b>Log Population</b>	0.006 (0.050)	0.611 (0.444)	-0.047 (0.047)	-0.197** (0.075)	-0.028 (0.027)	-0.047 (0.089)
<b>% Indigenous</b>	0.232** (0.088)	2.613* (1.047)	-0.016 (0.017)	0.016 (0.306)	0.009 (0.007)	0.286 (0.376)
<b>Constant</b>	-0.206 (0.452)	-6.159 (4.182)	0.405 (0.409)	1.619* (0.739)	0.237 (0.238)	0.263 (0.893)
<b>N</b>	62,400	62,400	62,400	62,400	62,400	62,400
<b>R<sup>2</sup></b>	.05	.13	.01	.02	.01	.02

\*p<.05, \*\* p<.01, \*\*\* p<.001 (two-tailed test). Robust Standard Errors in Parentheses.  
Models include annual and departmental fixed effects.

**Table III: The Effects of Torture: Matched Sample Difference-in-Difference Estimates**

	Torture's Effects on Selective State Killings		Torture's Effects on Local Insurgent Killings		Torture's Effects on Regional Insurgent Killings	
	Model 4a t+1 Month	Model 4b t+6 Months	Model 5a t+1 Month	Model 5b t+6 Months	Model 6a t+1 Month	Model 6b t+6 Month
<b>Average Treatment Effect (ATE)</b>	32.602*** (3.394)	24.524** (7.933)	0.015 (0.017)	0.056 (0.102)	0.024*** (0.005)	0.046 (0.026)
<b>Treatment Group</b>	0.362 (2.469)	13.300* (5.767)	-0.001 (0.012)	-0.023 (0.074)	-0.002 (0.003)	-0.035 (0.019)
<b>Post-Intervention Period</b>	-1.022** (0.362)	-5.597*** (0.845)	-0.003 (0.002)	-0.012 (0.011)	-0.002*** (0.000)	-0.012*** (0.003)
<b>Selective State Violence</b>	4.981*** (0.914)	48.656*** (2.135)	-0.000 (0.005)	0.021 (0.027)	-0.000 (0.001)	0.034*** (0.007)
<b>State Massacres</b>	35.695*** (2.395)	62.080*** (5.597)	-0.004 (0.012)	0.030 (0.072)	-0.003 (0.003)	-0.002 (0.018)
<b>Spatial Lag</b>	1.129 (2.201)	7.433 (5.145)	-0.002 (0.011)	-0.015 (0.066)	0.013*** (0.003)	0.037* (0.017)
<b>Insurgent Violence</b>	-2.086 (14.413)	-20.778 (33.682)	0.165* (0.072)	0.887* (0.433)	-0.001 (0.003)	-0.024 (0.109)
<b>Log Population</b>	0.202 (0.215)	2.546*** (0.504)	0.001 (0.001)	0.0078 (0.006)	0.001 (0.000)	0.005** (0.002)
<b>% Indigenous</b>	1.737*** (0.537)	11.709*** (1.256)	0.000 (0.002)	0.010 (0.016)	0.001 (0.000)	0.009* (0.004)
<b>N</b>	12,160	12,160	12,160	12,160	12,160	12,160
<b>R<sup>2</sup></b>	.05	.08	.01	.01	.01	.01

p<.05, \*\* p<.01, \*\*\* p<.001 (two-tailed test). Robust Standard Errors in Parentheses.  
Matching Strata Coefficients Omitted for Presentation Purposes

**Supplemental Material – To Be Made Available Online**

**Appendix I: An Example of a Matched Pair – Violence by Month**

	<b>t-6</b>	<b>t-5</b>	<b>t-4</b>	<b>t-3</b>	<b>t-2</b>	<b>t-1</b>	<b>t</b>
	Dec 1980	Jan 1981	Feb 1981	Mar 1981	Apr 1981	May 1981	Jun 1981
<b>Santa Cruz</b>	1	0	0	0	0	1	Torture
	Mar 1981	Apr 1981	May 1981	Jun 1981	Jul 1981	Aug 1981	Sep 1981
<b>Uspatan</b>	1	0	0	0	0	1	No Torture



## Appendix II: Conflict Histories and the Use of Torture

Conflict History	Municipality Months Experiencing Torture		Municipality Months Not Experiencing Torture	
	Total	Matched	Total	Matched
000000	47	47	58,758	5,956
000001	1	1	381	4
000010	3	2	354	5
000011	2	1	49	1
000100	2	1	354	2
000101	3	0	43	0
000110	0	0	44	0
000111	2	0	22	0
001000	5	3	355	6
001001	0	0	38	0
001010	1	0	36	0
001011	1	1	20	1
001100	0	0	0	0
001101	0	0	0	0
001110	1	0	21	0
001111	0	0	18	0
010000	3	2	362	12
010001	1	0	33	0
010011	0	0	36	0
010100	0	0	12	0
010101	0	0	36	0
010110	1	0	6	0
010111	0	0	15	0
011000	1	1	18	1
011001	1	1	47	1
011010	3	0	10	0
011011	0	0	6	0
011100	0	0	10	0
011101	0	0	16	0
011110	1	1	17	1
011111	1	0	12	0
100000	2	1	27	1
100001	3	3	390	16
100010	1	0	23	0
100011	1	1	40	1
100100	1	0	16	0
100101	2	0	40	0
100110	1	0	11	0
100111	0	0	9	0
101000	0	0	16	0

101001	3	0	36	0
101010	1	0	9	0
101011	0	0	6	0
101100	0	0	13	0
101101	0	0	21	0
101110	0	0	3	0
101111	0	0	12	0
110000	3	0	21	0
110001	2	1	46	2
110010	1	0	23	0
110011	0	0	18	0
110100	0	0	13	0
110101	1	0	11	0
110110	0	0	12	0
110111	0	0	9	0
110111	2	0	15	0
111000	2	1	22	1
111001	4	0	14	0
111010	0	0	18	0
111011	1	0	15	0
111100	5	1	21	1
111101	2	0	14	0
111110	1	0	28	0
111111	23	0	104	0
Total	142	69	62,258	6,011