

## **Do Human Rights Trials Make A Difference?**

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### **ABSTRACT**

Over the last three decades, transitional states are increasingly using criminal trials to hold former state officials accountable for past human rights violations. This article explores whether these domestic human rights trials have the capacity for decreasing repression in transitional societies. We used our new dataset on human rights trials in 100 countries that experienced transition from non-democratic regime to democracy or from armed conflict to peace. We situate our study within larger theoretical debates in political science, law and sociology on compliance with law and deterrence of crime. Contrary to the recent arguments made by some scholars, we find that human rights trials after transition lead to improvements in human rights protection. Moreover, we also found that trials in neighboring countries with similar culture have a possible deterrence impact beyond the confines of the single country.

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Since the 1980s, states are increasingly addressing past human rights violations using multiple mechanisms including domestic and international human rights trials, truth commissions, reparations, lustration, museums and other memory sites, archives, and oral history projects.<sup>1</sup> This paper focuses on one of the most important of these transitional justice mechanisms: domestic human rights trials. We are interested in whether domestic human rights trials have the capacity for protecting human rights in transitional societies. Because transitional human rights trials are relatively recent phenomena, we still know little about their effects. Although there is a large and important quantitative literature on the causes of human rights violations, it has not addressed human rights trials because previously there was no quantitative data on such trials.<sup>2</sup> We draw on our new dataset on domestic human rights trials to provide the first full quantitative analysis on the impact of such trials on human rights practices in transitional countries.<sup>3</sup>

There has been a lively debate in the political science and international law literature about the desirability and impact of human rights trials. This debate has important policy implications since governments, international organizations, and nongovernmental organizations (NGOs) are engaged in ongoing decision making about whether they should carry out or advocate human rights trials, and if so, what type and level of trials are suitable. Many scholars and practitioners believe that such trials are both legally and ethically desirable and practically

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<sup>1</sup> All these governmental efforts are transitional justice mechanisms, which can be defined as (re)interpretive, retributive, (re)distributive, and/or rectificatory governmental responses to a past severe and systemic human rights violation with a view toward future objectives such as reconciliation, peace, development, human rights protection, and democratic consolidation (Kim, 2007).

<sup>2</sup> Important quantitative researchers of human rights violations are Poe and Carey (2004), Poe and Tate (1994), Poe, Tate, and Keith (1999), Zanger (2000), Davenport and Armstrong II (2004), Anderson, Regan, and Ostergard (2002), Apodaca (2001), Bueno de Mesquita et. al. (2005), Richards, Gelleny, and Sacko (2001), Hafner-Burton (2005), Walker (2006), Regan and Henderson (2002), and Keith (2002).

<sup>3</sup> In other work, we have looked at both truth commissions and trials (Kim, 2007, Sikkink and Walling, 2007), but to keep the article more focused, we will explore the impact of human rights trials in depth. Quantitative measures of the remaining transitional justice mechanisms, including lustration and reparations, do not yet exist and thus can not be included in this analysis.

useful in deterring future human rights violations (Méndez, 1997, Roht-Arriaza, 1995). In his review of the transitional justice literature, Mendeloff (2004: 358, 361) finds many claims about the positive effects of human rights trials and relatively little solid evidence to support those claims. However, some realist scholars have argued that human rights trials will not deter future violations and that in some circumstance they will actually lead to an increase in repression (Goldsmith and Krasner, 2003: 51, Snyder and Vinjamuri, 2003). They argue that the threat of prosecution could cause powerful dictators or insurgents to entrench themselves in power rather than negotiate a transition from authoritarian regimes and/or civil war. The purpose of this article is to contribute to efforts to test claims systematically about the impact of human rights trials on human rights practices.

Debates about human rights trials are often divorced from larger debates in law and politics with which they have much in common. The transitional justice literature has tended to frame debates using evaluative terms like “idealists” and “pragmatists” (Snyder and Vinjamuri, 2004), or “maximalist approach”, “minimalist approach”, and “moderate or pragmatic approach.”<sup>4</sup> These evaluative categories may not be the most useful way to approach these debates. In this article, we seek to situate the transitional justice debates within larger theoretical debates on compliance and deterrence in political science, law, economics, and sociology. We identify specific hypotheses about the impact of trials and link these hypotheses to literature on compliance with law and deterrence of crime. What others call the “idealist” or “maximalist” position, for example, reveals itself to be consistent with basic rational choice hypotheses in the

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<sup>4</sup> Transitional Justice Database Project led by Leigh Payne framed the current approaches to transitional justice mechanisms using these categorizations. Scholars with maximalist approach advocate the use of transitional justice mechanisms for their moral superiority and practical usefulness – Orentlicher (1995), Bassiouni (2002), Roht-Arriaza (1995), and Mendez (1997). In contrast, scholars with minimalist approach stress the possible danger of such mechanisms and oftentimes promote amnesties as a means of reassuring former regime forces (Elster, 2006, Snyder and Vinjamuri, 2003). Others who are both cautious of both approaches and take a pragmatic stance are categorized under moderate or pragmatic approaches – Hayner (2002), Zalaquett (1992), Malamud-Goti (1990), Neier (1990), Nino (1991), Kritz (1996), Minow (1998), and Teitel (1995).

compliance and deterrence literature that increasing enforcement and imposing costs for human rights violations should contribute to more compliance with human rights law.

We consider a variety of these different hypotheses from this compliance and deterrence literature as well as from the more specialized literature on transitional justice. We test major hypotheses using data from a new dataset on human rights trials in transitional countries. We find that transitional countries in which human rights trials have taken place are less repressive than countries without trials, holding other factors constant. Contrary to the arguments made by realists, transitional human rights trials have not tended to exacerbate human rights violations. Our study also shows that countries with more accumulated years of trials after transition are less repressive than countries with fewer accumulated years of trials, *ceteris paribus*. In addition, countries surrounded by more neighbors with transitional trials are less repressive, which may suggest a deterrence impact of trials across the border.

In the first section, we give a brief background on the emergence of human rights trials, review and discuss the various arguments in the transitional justice, compliance, and deterrence literatures that link human rights trials with human rights violations, and present the hypotheses to be tested. In the second section, we explain our dependent and independent variables and discuss our sample, which is defined as transitional states. In the next section, the statistical evidence relating human rights trials to human rights protection is examined. Various statistical techniques were analyzed and implemented in order to check the robustness of the relationship, especially given the concerns about endogeneity. We conclude with a summary and suggestions for the future research.

## Compliance, Deterrence, and Human Rights Trials

The area of human rights has experienced a dramatic increase in legalization in the post WWII period. In 1945, this issue area was virtually unregulated at the international level. By 2000, many detailed treaties involving diverse human rights had been widely ratified and had entered into effect, but these treaties had weak enforcement mechanisms. It was a highly legalized issue area but there were few tools or sanctions to enforce the law. Where accountability existed, it tended to be reputational depending upon moral stigmatization of state violators.<sup>5</sup> In the few cases where stronger enforcement mechanisms existed, especially the regional human rights courts in Europe and the Americas, the focus was on state legal accountability. That is, if a case was decided by the European Court of Human Rights, it found that a *state* was in violation of its obligations under the Convention, and the state was asked to provide some kind of remedy, usually in the way of changed policy.

This model of state accountability with weak enforcement continues to be the main model for international human rights. But for a small set of core human rights and war crimes, states are increasingly using a new model of *individual* legal criminal accountability. This change has emerged gradually over the last twenty years in international, foreign, and domestic judicial processes. It reflects not only an increase in the judicialization of the politics of international human rights, but a specific form of judicialization – one focused on individual accountability. This trend has been described by Lutz and Sikkink (2001) as “the justice cascade,” and by Sriram (2003) as “a revolution in accountability.” This new regulatory model is not for the whole range of civil and political rights, but rather for a small subset of political

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<sup>5</sup> We use Grant and Keohane’s (2005) definition of accountability that implies that “some actors have the right to hold other actors to a set of standards, then judge whether they have fulfilled their responsibility and to impose sanctions if they determine these responsibilities have not been met.” Reputational accountability is one of the seven forms of accountability they discuss.

rights sometimes referred to as rights to physical or bodily integrity, especially the prohibitions on torture, summary execution, disappearance, political imprisonment, and genocide, as well as for war crimes and crimes against humanity.<sup>6</sup>

The model is reflected in the Statute of the International Criminal Court (ICC), in *ad hoc* tribunals for the former Yugoslavia (ICTY) and Rwanda (ICTR) and in the foreign universal jurisdiction cases like that against former Chilean President Augusto Pinochet (Roht-Arriaza, 2005). But the change has not been limited to these high profile international tribunals and foreign cases. It is a more profound shift that also includes changes in domestic institutions. The great bulk of enforcement of core human rights norms now occurs in domestic courts using a combination of domestic criminal law and international human rights law. In democratic states, enforcement of international human rights law may come not only through the structure of the human rights treaty itself, but through strong internal enforcement mechanisms, such as domestic courts (Hathaway, 2003). Our research focuses on domestic human rights trials as institutions that can enforce both international and domestic law. Human rights trials can be seen as a form of enforcement because they carry the possibility of punishment for individuals who violate human rights. Even when trials do not result in convictions and incarceration, they can impose substantial costs on individuals, including the financial costs of litigation, the lost income during preventive detention, and importantly for elites, a loss of prestige and legitimacy.

International relations and international legal scholars have long been concerned with the conditions under which governments comply with their commitments. A common hypothesis in the compliance literature is a rationalist one that argues that an increase in enforcement should

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<sup>6</sup> These include rights from only two or three of the 27 substantive articles of the International Covenant on Civil and Political Rights, those protecting the right to life and prohibiting torture. A number of these rights are the so-called non-derogable rights in the Covenant, which cannot be suspended even in exceptional circumstances or emergencies. The new model also provides enforcement of the Genocide Convention, the Convention against Torture, and those parts of the Geneva Conventions prohibiting war crimes.

lead to an increase in compliance with rules (Downs et al., 1996). Because domestic human rights trials can be conceptualized as a form of enforcement that imposes new costs on state officials, an exploration of whether trials lead states and government officials to comply more with human rights law can help illuminate these larger debates over compliance and enforcement.

The debate over international trials also relates to an important literature on deterrence in domestic legal systems (Andenaes, 1974, Blumstein et al., 1978, Matsueda et al., 2006, McCarthy, 2002, Nagin, 1998). This literature is similar to the compliance literature in that it is concerned with the degree to which punishment and sanctions influence compliance with laws, but the deterrence literature has focused almost exclusively on how domestic convictions and punishments inhibit individual criminal activity in the country where the punishment occurs. It is now relevant to the debates over human rights trials because these also involve domestic punishment for individuals with the aim of preventing future human rights violations.

Reviews of the deterrence literature conclude that now there is much firmer evidence for a substantial deterrent effect than there was two decades ago (Nagin, 1998). Of particular relevance to the case of human rights trials is the finding that beliefs about *the likelihood or probability of arrest and punishment* have more deterrent effects than increases in the severity of punishment (Bueno de Mesquita, 1995, McCarthy, 2002, Nagin, 1998). Deterrence research also suggests that deterrence is more effective for individuals who have a higher stakes in society or in conventionality (McCarthy, 2002, Nagin, 1998) which would seem to include the kinds of state officials involved in repression.

What has changed dramatically in the realm of international human rights is exactly this likelihood of individual punishment of state officials responsible for violations. Prior to the

1970s, there was a zero likelihood that heads of state and state officials would be held accountable for past human rights violations after transition to democratic regime.<sup>7</sup> In principle, the citizens could have held their past leaders legally accountable, but the continuing power of these leaders and the fear of coups almost always prevented such accountability. Our dataset indicates that in the 1980s and 1990s the observable likelihood of punishment for past human rights violations has increased from zero to some positive number in many countries. Indeed the international realm may provide some kind of natural experiment for deterrence theory, since major change in this key variable, the likelihood of punishment, has occurred in a relatively short period of time in many states. One of our main independent variables, which measures cumulative number of human rights trials years in a country, provides a good indicator of the likelihood of punishment for human rights violations. This likelihood of punishment varies from country to country and from region to region. Nevertheless, deterrence models suggest that this increase in the likelihood of punishment might lead to deterrence of future human rights violations.

Here we draw on the most general hypothesis of the rational choice deterrence literature: all else being equal, increases in punishment should decrease offending (Becker, 1968, McCarthy, 2002). We focus on the increases in the *probability or likelihood* of punishment, an area where there is most agreement among deterrence theorists, and not on increases in the severity of punishment, where there is still substantial disagreement (Tsebelis, 1993).

The compliance literature and the deterrence literature is also consistent with an important rational choice literature on the causes of repression that argues that state officials

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<sup>7</sup> The Nuremberg and Tokyo trials, and other WWII successor trials were the important exceptions to this rule, but they were also exceptions that proved the rule. Only leaders who ordered such crimes and then were unconditionally defeated in war could indeed be held individually criminally responsible for their crimes by the victors of the war or by domestic or foreign courts.

choose repression because the benefits gained from repression exceed the costs (Akhaven, 2001, Poe, 2004, Poe, et al., 1999). From this point of view, the expected benefits of repression may include the political gains for repressing political opponents, and the financial gains of expropriating their wealth and property. Trials lead to sanctions of various sorts (arrest, incarceration, punishment, public shaming, ostracism, discrediting of old regimes), which may increase the perceived costs of repression for state officials. The costs of repression for state officials include both the economic costs of trials and punishment, as well as the stigma and reputational costs that accompany trials, truth commissions, and the naming and shaming tactics used in the human rights realm. Such informal social sanctions may follow from the formal sanctions of human rights trials, and can have important effects in political arenas where reputation is essential. Since human rights trials increase the costs of repression while the benefits remain constant, the main deterrence hypothesis is that trials should lead to a decrease in repression.

The alternative to the rational choice models are norm models that say that sometimes compliance occurs for normative or cognitive reasons even in the absence of strong enforcement. Since issues of state and individual reputation, esteem, and legitimacy are at stake in human rights debates, the processes of the mobilization of shame through advocacy networks and international organizations could lead to behavioral change without stronger enforcement (Risse et al., 1999). But we should be clear that the norms literature *does not say* that stronger enforcement is counterproductive for compliance, just that strong enforcement may not be necessary in all circumstances and that behavioral change might be possible in the absence of strong enforcement mechanisms.

Most legal scholars and political science norms theorists who write on human rights believe that human rights change is usually the result of both normative and coercive factors, such as aid cut-offs or other sanctions (Akhaven, 2001: 13, Cardenas, 2007, Risse, et al., 1999, Weissbrodt and Bartolomei, 1991). Thus, they would tend to agree that the greater enforcement of human rights norms through processes such as trials will complement and enhance the processes of “naming and shaming” that have long been a staple of the human rights movement. Lutz and Sikkink (2001: 30), for example, argue that human rights trials can improve human rights practices by transforming the behavior of current and future political leaders and military and police officers, who observe the costs the trials imposed on their colleagues. The legal and norms literature has an understanding of “costs” that includes both the actual costs of punishment as well as the social effects of punishment as an expression of social disapproval. We do not yet know the exact mechanisms through which trials affect behavior; for now, we assume that both the social or reputation effects of trials as well as the narrower costs of anticipated punishment may be important factors for contributing to improvements in human rights. In sum, the enforcement literature, the deterrence literature, the rational choice literature on the causes of repression, and the norms literature support the hypothesis that human rights trials will lead to a decrease in human rights violations.

But there is a literature that suggests that more enforcement of human rights norms and law can be counterproductive in actually leading to more, rather than fewer violations of the law. Jack Goldsmith and Stephen D. Krasner (2003: 51) contend that “a universal jurisdiction prosecution may cause more harm than the original crime it purports to address.” They argue that states that reject amnesty and insist on criminal prosecution can prolong conflict, resulting in more deaths (Goldsmith and Krasner, 2003: 51). Jack Snyder and Leslie Vinjamuri (2004, 2003)

also argue that human rights trials themselves can increase the likelihood of future atrocities, exacerbate conflict, and undermine efforts to create democracy. They claim that “the prosecution of perpetrators according to universal standards – risks causing more atrocities than it would prevent (Snyder and Vinjamuri, 2003: 5).” These arguments suggest that more enforcement or the wrong kind of enforcement can lead to less compliance with international and domestic law. In particular, they suggest that during civil wars, insurgents will not sign peace agreements if they fear they will be held accountable for past human rights abuses. As a result, these authors claim that the threat of trials can prolong war and exacerbate human rights violations.

Until now, this debate has been carried on through the use of qualitative comparative case studies and counterfactual arguments. This has been necessary because there was no comprehensive dataset of human rights trials. Our new dataset on domestic human rights trials, however, allows us to test quantitatively for the first time two hypotheses that emerge directly from the literatures discussed above.

**H<sub>1</sub>:** Countries that have held human rights trials will see greater improvements in human rights practices than those countries that have not held human rights trials.

**H<sub>2</sub>:** Under situations of civil conflict and civil war, human rights trials will lead to an increase in repression.

Finally, we test an additional hypothesis extrapolated from the deterrence literature. While the existing deterrence literature focuses almost exclusively on deterrence within single countries, there is reason to suspect that human rights trials might have a deterrence impact beyond the confines of the single country within which they are held. We will refer to these

possible phenomena as “deterrence across borders” and will examine it by exploring the impact that the experience of human rights trials in a region will have on human rights practices in other countries within that region.

**H<sub>3</sub>:** The cumulative number of trials in a region will have an impact on human rights practices of countries within that region, even on countries that have not themselves held human rights trials.

### **Research Design**

To test the hypotheses discussed above, we use our new data on transitional human rights trials in the world during the period 1980~2004. This dataset uses country year as its unit of analysis. The data analysis uses a variety of time-series cross-sectional models in order to test the relationship between trials and repression, including models that allow us to address a reciprocal relationship between trials and human rights protection.

We include all states which have experienced a transition between 1974 and 2004. Countries with three types of transition were considered: democratic transition, transition from civil war, and transition by state creation. Democratic transition occurs when a country changes from a repressive and closed regime into an open and decentralized government. Transition from civil war occurs when a state recovers from instability and turmoil of a domestic armed conflict. States experience transition when a state is created by independence or reunion. We determined our sample using the Polity IV dataset and found 100 transitional countries.<sup>8</sup>

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<sup>8</sup> The regime transition variable (Regtrans), which was derived from the yearly changing values of the Polity score, was examined to determine our sample. We began with all 192 countries in the world in 2004 and excluded 32 countries with population less than 500,000. We went through three steps to determine states with transition. First, the Regtrans is a 6-point scale regime change variable, which ‘+3’ means a major democratic transition, ‘+2’, a minor democratic transition, and so forth until ‘-2’, an adverse regime transition. These were coded considering

Our sample includes all transitional countries, both those that have used human rights trials and those that have not. We chose to study these cases because the hypotheses we wish to test are about the role of human rights trials in transitional countries. We exclude stable authoritarian and fully democratic countries from this study, although we also have a database of human rights trials in these countries which we hope to examine in the future. But few authors claim that human rights trials would have an important positive or negative impact in fully consolidated democracies. Nor do we or other authors believe that genuine human rights trials are possible in fully authoritarian regimes.<sup>9</sup> Thus transitional countries appear to be the appropriate cases to test the main hypotheses. Our dataset comprise of 100 countries in the period from 1980 to 2004 and a list of countries is in Appendix 1. Each country is split into yearly observations for a total number of 1,475 country-years. Missing values for some of the explanatory variables limit the number of observation under investigation to 1,314 (89%) country-years for 95 countries.

### *The Dependent Variable – Repression*

We are most interested in the impact that human rights trials have on a core set of human rights violations, in particular, on torture, summary execution, disappearances and political imprisonment. Since enforcement exists mainly for these rights, the dependent variable we will

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both the Polity score changes and duration of the change. States with '+3' and '+2' scores between 1974 and 2004 were examined and we found 68 countries with democratic transition. Second, state failure is coded as '-77' and states were categorized to have experienced a transition from civil war if that country has exited from the state failure period by a regime change ('0' or '1'), interruption ('-66'), or transition ('-88') and found 16 countries. Third, state creation was recorded as state creation ('99') or state transformation ('97') and found 28 countries. We found 12 counties with multiple types of transition and all transitions in a chronological order were included.

<sup>9</sup> We exclude fully authoritarian countries because these countries for the most part do not hold free and fair trials, nor do they have an independent judiciary. Thus we could not be confident that genuine trials of state officials for human rights violations can be held. Without such confidence, we cannot say that there has been an increase in enforcement that might be expected to deter future crimes. In the absence of an independent judiciary, for example, what might appear on the surface to be a human rights trial might instead be a vendetta through which the authoritarian leader punishes his political opponents.

be using to test the impact of transitional trials is a measure of violations of these physical integrity rights. Following Poe and his co-authors (Poe and Tate, 1994, Poe, et al., 1999), we will refer to this core set of human rights violations as repression. We operationalize it using the physical integrity rights index from the Cingranelli-Richards human rights database (Cingranelli and Richards, 2004). The Cingranelli-Richard database allows more variability than the other main measure of repression, the political terror scale (Cingranelli and Richards, 1999, Gibney, 2004, Gibney and Dalton, 1996). We reversed the original physical integrity rights index into a 9-point scale where “8” indicates the highest level of repression (no respect for physical integrity rights) and “0” indicates an absence of repression (full respect).<sup>10</sup>

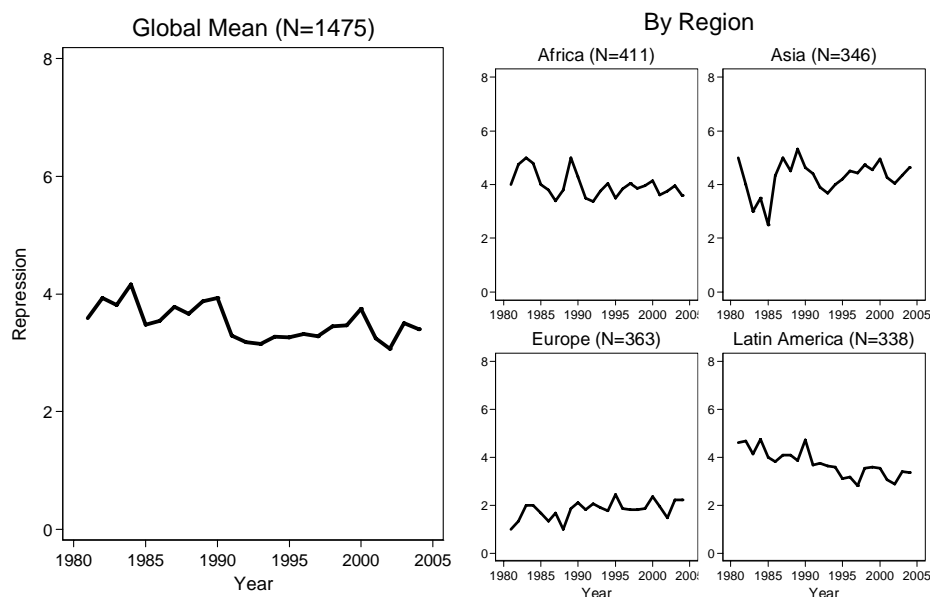


Figure 1. Change in the Average Level of Repression over Time Globally and by Region

<sup>10</sup> One concern is that both measures - the physical integrity rights index and transitional human rights trials – use the U.S. Department of State Country Reports on Human Rights Practices. However, these two measures are coded from different parts of the document, and we could not find any evidence that trials or judicial activities were used in coding any of four elements – political/extrajudicial killings, disappearance, torture, and political imprisonment – of the physical integrity rights index (Cingranelli and Richards, 2004).

Figure 1 summarizes the change in the mean score of repression in transitional societies over time. The graph on the left represents the mean score of the all transitioned countries. The average level of repression for transitional countries is fairly constant over time but we can see a slight drop in the level of repression over time. There are visible discrepancies when we examine the mean level of repression by region. While European countries show quite stable low level of repression, we can witness substantial decrease in the level of repression over time in Latin American transitional societies.

### *The Independent Variable - Transitional Human Rights Trials*

While information is readily available on the relatively small number of human rights trials in international tribunals, the data on domestic trials is very decentralized and difficult to quantify. To address this problem, we have created a new database of domestic transitional human rights trials by coding from the U.S. Department of State Country Reports on Human Rights Practices for all countries from 1979 to 2004.<sup>11</sup> *All domestic judicial proceedings in transitional countries for human rights violations during previous regimes committed by government officials or their agents* were considered. To be included in the dataset the judicial activity discussed in the report must inflict costs on a government agent accused of having *individual criminal responsibility* for human rights violations.<sup>12</sup> Judicial proceedings can be initiated either by governments themselves or by individuals or groups.

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<sup>11</sup> These reports are generally considered to be a reliable source of information on states' human rights practices (Poe et al., 2001).

<sup>12</sup> Judicial proceedings may include the following: indictment, arrest, detention of a suspect (whether in house or in prison), or an extradition request that is being actively pursued, the initiation of a trial, or the continuance of a trial so long as there is active progress being made in the case, or a ruling in a trial. Civil trials, the granting of reparations, apologies, or purely administrative inquiries, investigations, or punishments and the like do not count as judicial activity in our dataset. For more detailed description of the dataset, refer to Sikkink and Walling (2007).

When the Human Rights Country Reports mentioned a judicial proceeding that met the above criteria, a country is coded “1” for having transitional human rights trials in a given year (a country trial year). If there was no mention of a human rights trial, a country was coded “0” for that year. This coding decision gives us a level of precision we believe is appropriate to the available data. It is almost impossible to count the actual number of judicial proceedings in any particular country, even in those countries where we have conducted field research, such as Argentina. Therefore, we believe it would be misleading to try to count the number of trials in transitional countries based on the data in the Country Reports, which do not presume to be exhaustive. This has the effect of underestimating the number of trials in many countries since, no matter how many trials are occurring, a country is still only coded “1” for that year. But, countries that are carrying out many trials tend to have a greater accumulated number of trial years over time than countries that carry out fewer trials, something we refer to as “cumulative trial years.”

Two measures of transitional trials were used in this study. First, a dichotomous variable was created to track whether a country had a transitional trial at any point after transition. Second, a continuous measure of cumulative trial years, which provides more extensive within-state and between-state variation, was also used. The cumulative number of trial years captures the persistence and frequency of trials in the country. Transitional human rights trials experience (TRT) and cumulative number of trial years (CTRT) are the two main independent variables for the quantitative analysis undertaken in this article. Our cases include 41 countries with domestic transitional human rights trial experience, including 27 countries with two or more cumulative trial years. A list of countries is in Appendix 2.

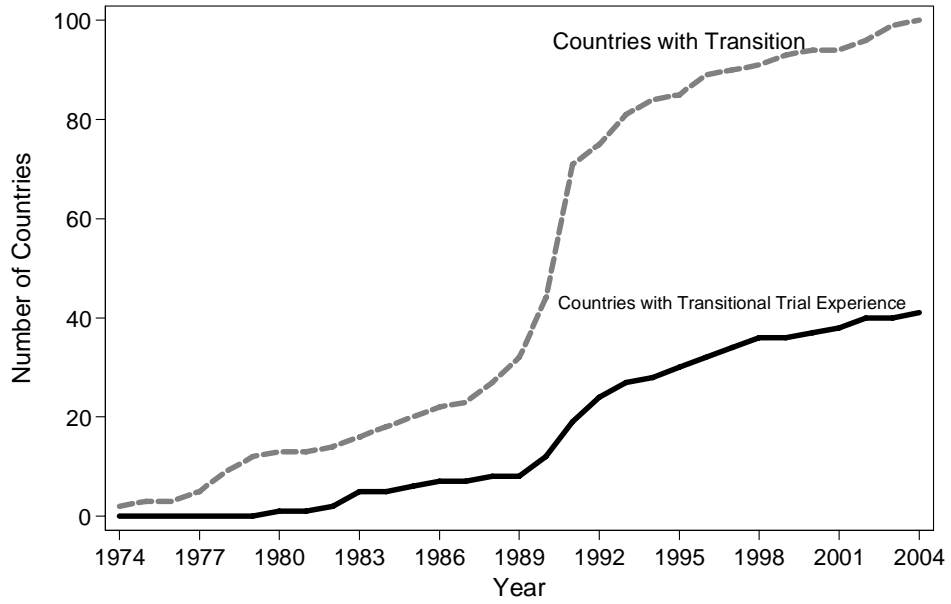


Figure 2. The Number of States with Transition and Transitional Trial Experience

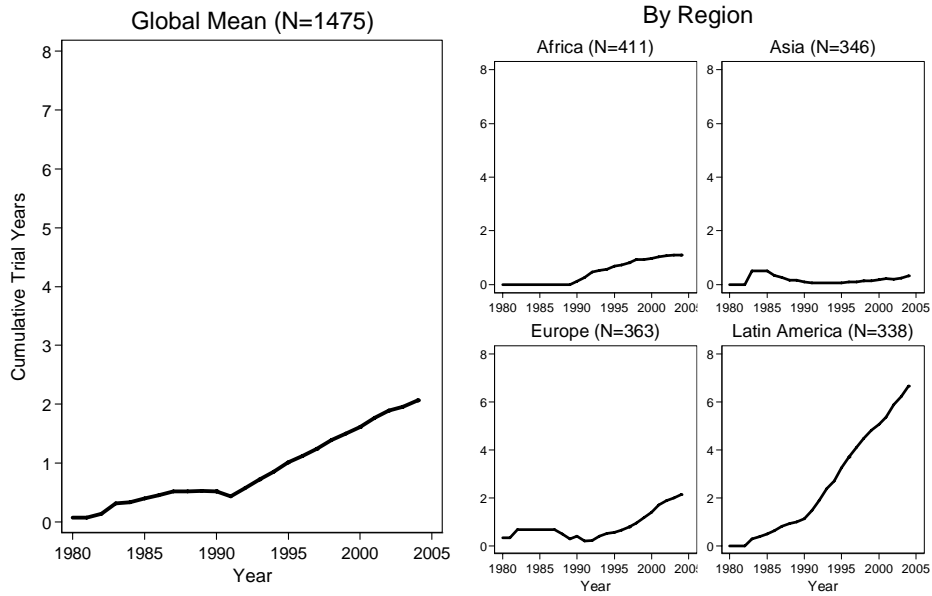


Figure 3. Changes in the Average Cumulative Trial Years (CTRT) over Time by Region

Figure 2 and 3 provide a snapshot of our sample and independent variables. The dashed line in Figure 1 shows the number of countries experiencing transition between 1974 and 2004.

We can examine a steady increase in the number before 1989 and a sudden rise between 1989 and 1992 reflecting the collapse of the Soviet Union and the Eastern European regimes. The lower solid line shows the steady increase of states with transitional trial experience after 1980. Figure 3 shows the change in the average cumulative trial years over time globally and by region. There is an increase in the mean score of cumulative trial years globally. Both figures support our argument that the likelihood of punishment increased globally over the last 30 years. Latin America certainly is leading the trend, but average cumulative trial years in Africa and Europe are also increasing over time.

### *Model Specification*

The basic structure of the data is an unbalanced time-series cross-sectional dataset. Three sets of models were implemented to study the relationship between trials and repression. First, in line with most studies of repression, this study uses pooled ordinary least squares (OLS) regression with panel corrected standard errors (PCSE) and a lagged dependent variable (Beck and Katz, 1995, Poe, et al., 1999).<sup>13</sup> The basic PCSE model is:

$$Y_{i,t} = \beta_0 + \beta_1 Y_{i,t-1} + \beta_2 X_{i,t} + \gamma Z_{i,t} + e_{i,t}, \quad (1)$$

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<sup>13</sup> Due to the ordinal character of the dependent variable, some researchers use ordered probit or ordered logit to estimate the model (Hafner-Burton, 2005, Walker, 2006). We agree with them and also estimated ordered probit models. The results for single equation models, which are available upon request, were generally the same without major differences. However, there are two reasons for our hesitation in using ordered probit models for this study. First, although we tried both models with a lagged dependent variable and a Markov transition model, the problem of temporal autocorrelation cannot be easily handled in ordered models. Second and more importantly, techniques are not readily available to estimate simultaneous equation models for ordered probit, which is crucial for our study.

where  $Y_{i,t}$  represents our measure of repression,  $Y_{i,t-1}$  is a one year lag of the dependent variable, and  $X_{i,t}$  is the respective measure of trial – either TRT or CTRT,  $Z_{i,t}$  is a vector of control variables, and  $e_{i,t}$  is a error term.<sup>14</sup>

A set of control variables is included in our models to isolate the true effect of human rights trials from all other factors that might affect the level of repression. Guided by previous studies on repression, seven control variables – democracy, international wars, civil wars, economic standing, economic growth, population size, and population growth – were included (Poe and Tate, 1994, Poe, et al., 1999). We anticipate that factors that were proven to be important in the previous studies will continue to be important. Additionally, in order to control for any spatial and temporal discrepancies, we included regional dummy variables and a linear time variable.<sup>15</sup> Finally, we included a truth commission experience variable (Kim, 2007).<sup>16</sup>

Second, country-specific fixed-effects regression models were used. Fixed effects regression is an effective way to examine how changes in human rights trials affect repression in a given state. The basic fixed effects model is:

$$Y_{i,t} = \beta_0 + \beta_1 Y_{i,t-1} + \beta_2 X_{i,t} + \gamma Z_{i,t} + u_i + e_{i,t}, \quad (2)$$

<sup>14</sup> Some scholars showed concerns about using a lagged dependent variable in time-series cross-sectional data (Achen, 2000, Keele and Kelly, 2006). To address this, we estimated models using Prais-Winsten regression with panel corrected standard errors assuming a first-order autoregressive error process. There is no substantial difference and the results are available upon request.

<sup>15</sup> A model with the regional dummy variables and the linear time trend variable is better than Poe's original model with our sample. By including those variables, adjusted R<sup>2</sup> increased from 0.68 to 0.69 and the variables measuring the impact of these regional and temporal trend jointly are statistically significant ( $\chi^2=34.99$ ,  $df=5$ ,  $p<0.001$ ). We also examined other specification of time trend using both year dummy variables and nonlinear time trend variables.

<sup>16</sup> Democracy was measured using the Polity score from Polity IV project. International and civil conflicts were measured using the PRIO/Uppsala Armed Conflict Dataset. The log of the GDP per capita was used to measure economic standing and the annual rate of GDP growth (%) was used to measure economic development. The log of population was used to measure population size and the annual percentage of population change (%) was used to measure population change. The economic and population data were from the U.N. Common Database (except Taiwan, which we used the National Statistics data of Taiwan <http://eng.stat.gov.tw/mp.asp?mp=5>). The regional dummy variables were created using the U.N. regional division of the world – Africa, Asia, Oceania, Europe, and Latin America. (Latin America was used as an omitted base category.) The linear time variable was created as a variable counting a year since 1980. Truth commission experience dummy was coded using the Truth Commission in the World database (Kim, 2007) and a list of countries are available in Appendix 2.

where  $u_i$  is the country-specific fixed effects and other notations are the same as PCSE model.<sup>17</sup>

The final model deals with the possibility of reverse causation in our previous equations. It is possible that the levels of repression itself could affect the likelihood and persistence of trials in transitioned country. Many think that there is possible endogeneity because the main function of trials is to address past human rights violations. Thus the current level of repression possibly determines whether a country will have transitional trial, and for how many years it will continue to hold trials. If this effect were true, then the resulting endogeneity of our human rights trials variable will bias our coefficient estimates in previous models. We employed a two-stage estimation of simultaneous equations. The basic model comprises of two equations:

$$\begin{aligned} Y_{i,t} &= \beta_0 + \beta_1 Y_{i,t-1} + \beta_2 X_{i,t} + \gamma Z_{i,t} + e_{i,t} \\ X_{i,t} &= f(K_{i,t}, v_{i,t}), \end{aligned} \quad (3)$$

, where  $K_{i,t}$  in the second equation is a vector of variables that are theoretically argued to affect either the use of trials or the number of years a state use trials,  $v_{i,t}$  is an error term of the second equation, and all other notations are the same as PCSE model. Depending on the function ( $f$ ) – *probit* for the dichotomous variable (TRT) and *linear regression* for the continuous variable (CTRT) – two-stage probit least squares (2SPLS) (Alvarez and Glasgow, 1999, Amemiya, 1978, Heckman, 1978, Maddala, 1983) or generalized two-stage least squares (G2SLS) was estimated.<sup>18</sup>

In two-stage models, we first estimate the reduced form equation by regressing  $X_{i,t}$  using all of the exogenous variables in the system ( $Y_{i,t-1}$ ,  $Z_{i,t}$ , and  $K_{i,t}$ ). Then, from the reduced form

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<sup>17</sup> There are concerns about using both fixed effects and a lagged dependent variable in the model (Nickell, 1981). To address this issue, we additionally ran all fixed effects regression without a lagged dependent variable but found the substantial result unchanging in our study.

<sup>18</sup> The first equation can contain the country-specific fixed effects and then the fixed-effects generalized two-stage least squares models are estimated instead of generalized two-stage least squares models.

estimates, we compute the linear predictors for the transitional trial variable and substitute these values for the endogenous regressors in the second-stage structural equation. The results of this second stage analysis will produce consistent estimates of the model parameters but the standard errors should be corrected because they are based on the predicted values of the endogenous explanatory factor. Following Kim's (2007) previous study, the second equation was estimated using the percentage of regional precedents of human rights trials, truth commission experience, democracy, the number of domestic human rights NGOs, commitment to international human rights treaties, the British common law tradition, and types of transition.

### Empirical Analysis

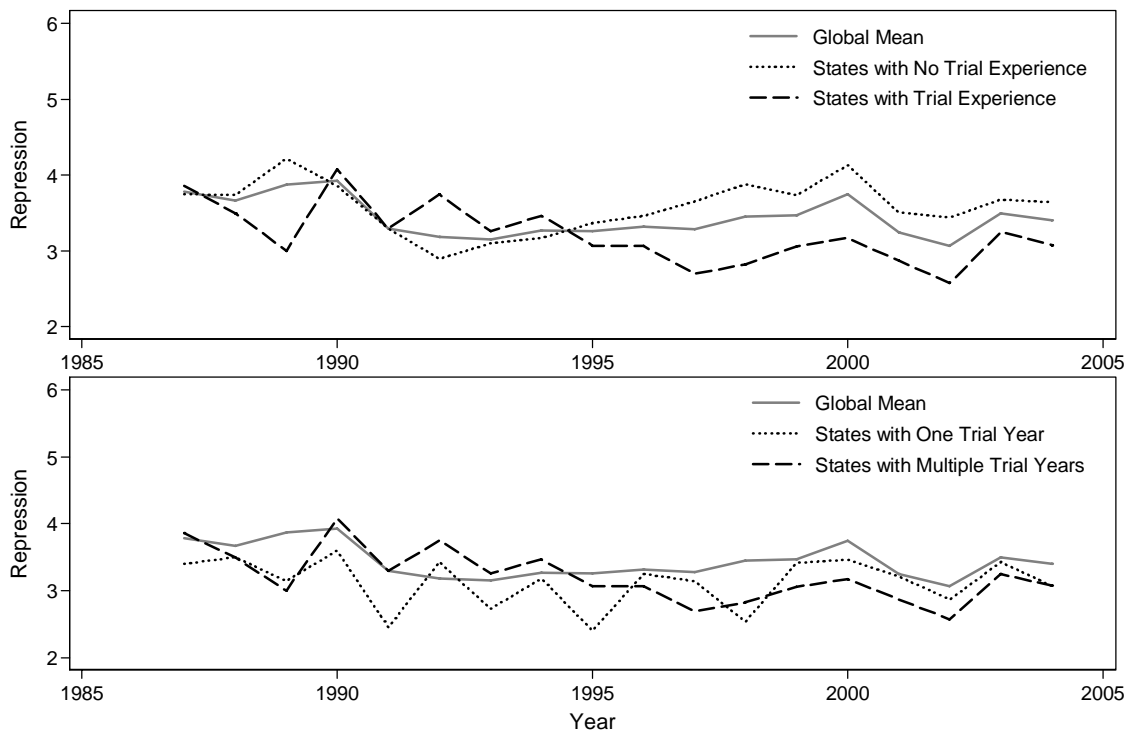


Figure 4. Changes in Mean Score of Repression by Transitional Trial Experience

Figure 4 shows the changes in the average repression score by trial experiences. Within each graph, a gray solid line indicates the changes in the yearly mean of the repression score. In the upper graph, we compared the yearly mean repression score of countries with trial experience (dash) to those without any trial experience (dot). The distinction between the two lines becomes clear and stable after 1994. The mean repression score of countries without trials are constantly above the average while the mean repression score of countries with trials are below the average. The lower graph compares the mean repression scores of countries with one trial year (dot) to those with multiple trial years (dash). While countries with one trial year for most of the time period have a below average mean repression, states with multiple trial years tend to have more stable and lower repression scores than the average after 1996.

(Table 1)

Table 1 reports the results of estimating the effect of trials on repression using PCSE models. Model 1 is an extension of Poe, Tate, and Keith's 1999 model to our sample. Although we examined transitional countries for a longer period (1980~2004), our findings are consistent with Poe, Tate, and Keith's major findings from their study of all countries in the world (1976~1993). Democracy, civil war, economic standing, population size, and past level of repression have a statistically significant and substantively important impact on the level of repression. Our regional dummy variables and time variable all turned out to be statistically significant, which suggests that geographic location and time matters and the regional and temporal clustering exists in the pattern of repression.

Model 2a and 2b are our baseline models. The variable measuring trial experience (TRT) or cumulative trial years (CTRTR) is included along with the truth commission experience variable. The result in Table 1 reveals that transitional human rights trials have a strong and

statistically significant impact on government's respect for personal integrity rights.<sup>19</sup> When controlled for all other relevant factors, the level of repression in countries which have ever had transitional trials is 0.16 points lower than that of countries which never had trials in a 9-point repression scale. Moreover, not only trial experience itself but also the persistence and frequency of human rights trials matter. In Model 2b, the repression score of a country with 19 years of trial experience (maximum) is 0.34 points lower than that of a country with no trial experience. In other words, if a country were to move from zero trial years to the maximum possible number of trial years, this would make a difference of about 0.34 points in the repression score, *ceteris paribus*.

The coefficients of the trial variables represent the short-term effect of trials on repression. The short-term effects of trials are the estimated impact of a trial in the same year that the trial occurs. Due to the inclusion of the lagged dependent variable, the total effects are also dynamically distributed across time. The long-term effects are those that are accrued over time through the operation of the lagged dependent variable. These long-term effects are permanent effects that become realized after several years.<sup>20</sup>

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<sup>19</sup> There are bivariate relationships between trials and repression without any control variable included. The regression estimates of trial variables with no control variables for the Model 2a is  $\beta = -0.533$  (SE=0.121,  $p < 0.001$ ) and for Model 2b,  $\beta = -0.279$  (SE=0.082,  $p = 0.001$ ).

<sup>20</sup> The long-term effects can be calculated by  $\beta / (1 - \alpha)$ , where  $\beta$  is the coefficient on the trial variables and  $\alpha$  is the coefficient on the lagged dependent variable.

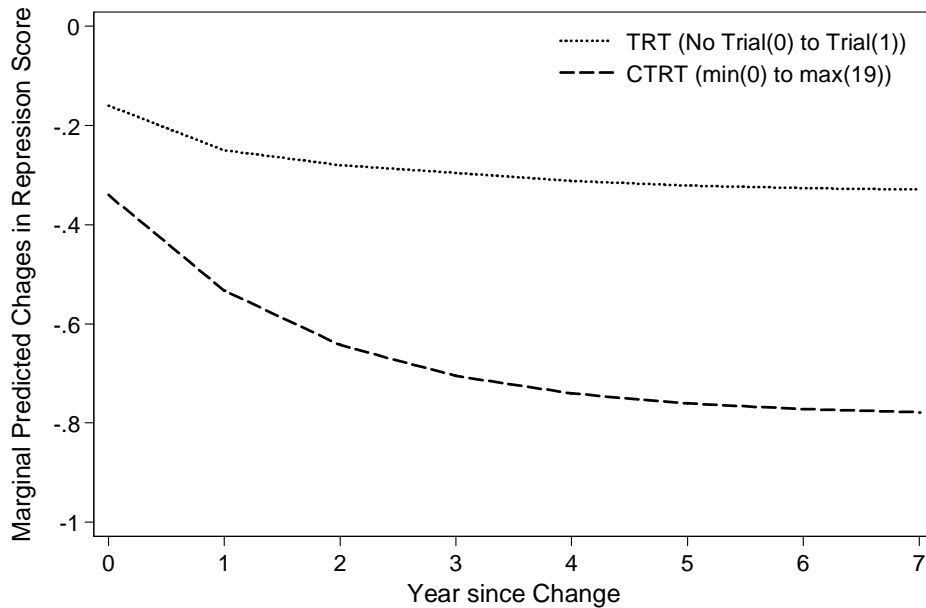


Figure 5. Effect of Transitional Trials in Transitioned Society over Time

Figure 5 presents the estimated dynamic effects of human rights trials using the coefficients taken from Model 2a and 2b if the average country were to move from no trials to one trial (dot) or from no trials to the maximum number of trial years (dash). Note that by five years most of the impact is predicted to have been realized and from seven year onwards there is negligible change. If a country starts to have transitional trials, other factors being equal, the repression score would decrease by 0.16 points in the immediate year and about 0.32 points after 5 years as a result of the lagged effect, stabilizing at a few lags thereafter. In other words, the move from no trials to trial would bring about a third point improvement in the physical integrity rights index over seven years after the initiation. The movement from zero years of trial to the maximum possible number of trial years (19) starts with the 0.34 points decrease in repression score, and stabilizes at about 0.78 points decrease after several lags.

The truth commission experience variable was included in the model both as a control variable to estimate true effect of human rights trials and as an independent variable to see whether it has an independent effect on the level of repression. Truth commission experience also is contributing to improved human rights protection in transitional societies. Our model shows that truth commission experience brings about 0.17~0.18 points decrease in the repression score in a short-term and 0.38~0.41 points decrease in a long-term, *ceteris paribus*. Although we are hesitant to argue as confidently due to higher *p*-values (0.066~0.085), truth commissions have an independent impact on human rights protection in transitional societies. If a country were to have both human rights trial and truth commission experience after transition, that would bring about 0.34 points decrease in the repression score ( $\beta=-0.338$ ,  $SE=0.124$ ,  $p=0.006$ ) in the short-term.

In sum, we found that countries with human rights trials after transition have better human rights practices than countries without trials, *ceteris paribus*. In addition, transitional countries that have experienced more human rights trials over time (and thus a greater likelihood of punishment for past human rights violations) have better human rights practices, holding other factors constant, than transitional countries that have not had or had fewer human rights trials. Contrary to the arguments made by realists, transitional human rights trials have not tended to exacerbate human rights practices.

(Table 2)

The models in Table 2 test hypothesis #2 – the impact of human rights trials on repression in civil war situations – by including interaction terms of the trial variables and the civil war variables. There are two pathways civil war situation could influence the impact of

human rights trials on repression. The most important of these is that in situations of civil war the demand for prosecution for human rights violations can delay a peace agreement. Because civil war in particular is associated with human rights violations, anything that prolongs war could have the effect of exacerbating human rights abuses (Poe, et al., 1999). A second possible causal logic is that in countries that have experienced civil war, the demand for trials for past human rights violations can lead to military coups against democratic regimes or another civil war. Once again, because nondemocratic regimes and civil war are more likely to cause higher levels of human rights violations, if the demand for trials undermines democracy and peace, it could lead to an increase in repression (Poe, et al., 1999).

In order to test these hypotheses, we included two interaction terms in our baseline models. First, we included interaction terms of the civil war variable and human rights trial variables. This specification allows us to test whether transitional trials combined with any civil war situation may have different impact on repression. It is possible to test the first causal logic because this allows us to distinguish the different impact of trials under civil wars, if any. Second, we included interaction terms of the civil war transition variable and human rights trial variables. This interaction term distinguishes the different impact of trials in countries that have gone through transition from civil wars, in other words, countries that are most vulnerable to another civil war or military coup. This specification allows us to test the second causal logic by examining whether trials in such countries may have different impact on repression compared to countries that are less vulnerable to those threats.

When the interaction terms were added, the statistical significances and magnitude of other control variables remain constant. The effects of trials on repression are still statistically significant and important even though the  $p$ -values of the coefficients of the trial variables

dropped a little. The more interesting aspect is the coefficient of the interaction terms, which represents the net effect of trials under a civil war situation. If Snyder and Vinjamuri's (2003) argument were valid, we would expect to see a positive sign in the coefficients and find statistical significance. However, we do not find this in our results. First, human rights trials under civil wars do not have a different impact on repression than those in peace. Moreover, Model 3b suggests that human rights trials during civil wars may even lead to greater improvements in human rights protections than trials in peace. The impact of more frequent and persistent human rights trials is proven more effective when a country is undergoing civil conflicts. Second, our result shows that trials in countries with a transition from civil war experience may lead to more repression but statistical evidence is too weak to make this argument. In sum, although the involvement in civil wars certainly exacerbates governmental repression, past trial experiences still appears to have a positive impact on human rights protection in those situations compared to states with no human rights trials experience.

(Table 3)

The models in Table 3 test hypothesis #3 – the possible deterrence effect across borders. We included two different variables measuring the effects of human rights trials in neighbors. Two concepts of neighbor were used to understand the mechanisms of cross border deterrence effect. First, a state can be affected by trials of geographically proximate states. This concept of neighbor is supported by many policy diffusion studies where geographic proximity is proven to be an important determinant of diffusion (Berry and Berry, 1990, Gray, 1973, Mooney, 2001, Simmons, 2000, Walker, 1969). We defined geographic proximity using the U.N. sub-regional

divisions of the world.<sup>21</sup> Second, we examined whether states can be affected by trials that occurred in culturally similar states in the same continent. We used five religion categories to define cultural neighbors – Protestant, Catholic, Orthodox, Muslim, or Buddhism.<sup>22</sup> Some examples will help clarify the differences between these two conceptualizations of a neighbor. Argentina is classified in the South America category of the U.N. sub-regional divisions. Using the geographic concept, trials in Bolivia, Brazil, Chile, Ecuador, Guyana, Paraguay, Peru, and Uruguay are relevant. When the cultural concept is used, trials in Bolivia, Brazil, Chile, Dominican Republic, Ecuador, El Salvador, Guatemala, Haiti, Honduras, Mexico, Nicaragua, Paraguay, Peru, and Uruguay matter, since these are Latin American states where 50% or more of the population is Catholic.<sup>23</sup> By using these two conceptualizations, we wish to distinguish and grasp different paths of influence.<sup>24</sup>

The impact of human rights trials in neighbors differs depending on the conception of neighbors. Model 4a and 5a shows that the domestic trial variables remain robust when the trials in geographic neighbor variables are included. Its magnitude and statistical significance is unchanging and the trials in neighbors do not affect the level of repression. However, this

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<sup>21</sup> The U.N. sub-regional divisions are the Eastern, Northern, Western, Southern, Middle Africa, Caribbean, Central, South, Northern America, Central, East, Southern, Southeastern, Western Asia, Eastern, Northern, Southern, Western Europe, Australia and New Zealand, Melanesia, Micronesia, and Polynesia.

<sup>22</sup> Religion and language are two central elements representing culture. Both religion and language were considered as possible definitions of cultural neighbors but religion was chosen because language is more diverse than religion and overestimates the diversity rather than the commonality of culture. The statistical results were not different when language was used as an indicator of culture. To create the cultural neighbor variable, each state was categorized by a certain religion if more than 50% of populations believe in it using the CIA World Factbook. Five major religions were considered along with categories of ‘others’ and ‘not distinguishable.’

<sup>23</sup> Another example is the Democratic Republic of Congo, classified under the category of Middle Africa in the U.N. sub-regional divisions. By the geographic concept of neighbor, trials in Angola, Central African Republic, Chad, and Republic of Congo are relevant. However, when the cultural concept of neighbor is used, trials in Burundi, Eritrea, and Rwanda matter since these are African states where 50% or more of the population is Catholic.

<sup>24</sup> The percentage of states having experienced transitional trials within a state *i*'s sub-regional division by year and the percentage of states having experienced transitional trials within state *i*'s cultural neighbors by year were used in Model 4. The average number of cumulative trial years in state *i*'s sub-regional division (not including state *i*'s cumulative trial years) by year and the average number of cumulative trial years in state *i*'s cultural neighbors (not including state *i*'s cumulative trial years) by year were used in Model 5.

changes when the trials in cultural neighbor variables are included. The impact of the domestic trial variables decrease and are no longer statistically significant as previous models ( $p=0.102$  for Model 4b;  $p=0.167$  for Model 5b). On the other hand, trials in cultural neighbors become an important factor affecting the levels of repression, which may suggest a possible deterring effect of transitional trials across borders. This suggests that human rights trials in neighbor matter affecting the level of repression, and deterrence may be occurring not simply between geographically adjacent states but among culturally similar countries.

From Model 4b, we could learn that a state with no trials of its own may experience a similar deterrent effect of human rights trials as a state with its own trial experience if 40% or more of its cultural neighbors already had human rights trials. A country with no trial of its own that has 50% or more of its cultural neighbors with trials ( $\beta=-0.172$ ,  $SE=0.090$ ,  $p=0.058$ ) may experience an equivalent deterrence effect of trials as a country with its own trial experiences which has only 10% of its cultural neighbors that already had trials ( $\beta=-0.163$ ,  $SE=0.077$ ,  $p=0.036$ ). In sum, a country with no trial experience at all can achieve the same deterrence effect of human rights trials on repression if that country has enough neighbors with cultural similarity using transitional trials.

### *Further Discussion*

Table 4, 5, and 6 provide us with a means to check the robustness of our findings. Table 4 presents the estimation from two-stage models that deal with possible reverse causality between human rights trials and repression. Model 6a and 6b are the results of generalized two-stage least squares estimation. In Model 6a, we treated the dichotomous independent variable (TRT) as a linear probability model, which is problematic. Thus, we estimated a two-stage

probit least squares model in which one of endogenous variable is continuous and the other variable is dichotomous.<sup>25</sup> Since we substituted the linear predictors from the reduced form equation for our original trial variables, it is difficult to understand the magnitude of our imputed trial variables in the structural equation and rescaling is needed to understand the meaning of the coefficients. Nevertheless, the human rights trial variables still have statistical significance and the directions of influence are negative. In other words, transitional human rights trial experience and an increase in cumulative trial years lead to an improvement in human rights practices, other things being equal.

Table 5 and 6 are the results from the country-specific fixed-effects regression models suggested by Green, Kim, and Yoon (2001). Table 5 is a single equation model and Table 6 is a two-stage generalized fixed-effects model with simultaneous equations. In fixed-effects regressions, the important control variables – democracy, civil war, international war, and past level of repression – maintain their magnitude, direction, and significance. However, some other variables either lost their statistical significance (economic standing), changed direction of influence (population size) or gained statistical significance (economic development). This implies that while economic standing is important in explaining the difference in repression *between* states, it explains very little in terms of the difference in repression *within* states by year. On the contrary, even though economic development was not an important factor explaining difference in repression between states, it is important factor explaining the difference in the level of repression of a country over time. The changed direction of influence may suggest that the effect of population size on repression is actually negative but obtains a spurious positive relationship when units are pooled together (Green, et al., 2001: 445), which is a possible topic for the future research.

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<sup>25</sup> We used *cdsimeq* command in Stata 9.2/SE to estimate this model (Keshk, 2003).

The coefficients in the fixed-effects models are larger and provide more efficient estimations compared to the PCSE models in Table 1. Again, transitional human rights trials have a negative, strong, and statistically significant impact on the level of repression. The level of repression in countries which had at least one year of transitional trials is 0.49 points lower than that of countries which never had transitional trials. Moreover, the repression score of a country with the maximum years of trials (19) is 1.27 points lower than that of a country with zero trial years. Long-term effects are more dramatic. If a country initiates trials, the repression score would decrease by 0.68 points after several lags. The movement from zero to the maximum number of trial years is associated with a 1.27 point decrease in the repression score, stabilizing at a 1.76 point decrease after several lags. In general, the magnitude of the trial effect is almost double in the fixed-effects model compared to the PCSE model. This indicates that although transitional human rights trials explain between-country variation in repression, trials have more powerful effect in explaining within-country variation of repression over time. In line with two-stage models in OLS regressions, fixed-effects two-stage regression also showed the statistical significance of the transitional human rights trial variables.

Truth commission experience also turned out to have a very strong effect in all country-specific fixed-effects models (Table 5 and 6). The coefficients range from -0.33 to -0.49 and *p*-values range from 0.077 to 0.005, which indicate strong statistical significance of its impact. This suggests that truth commission experience brings about a 0.4 point decrease in repression score, holding all other factors constant. The difference in PCSE model and fixed-effects model suggests that although truth commission experience explains between-country variation in repression, truth commission experience is also a more powerful factor explaining the changes in repression of one country over time (cross-temporal difference).

## **Conclusion**

Our findings refute the realist argument that human rights trials lead to more repression and demonstrate that trials and truth commissions have a positive effect on human rights practices. We conceptualize human rights trials as a type of enforcement of existing human rights norms, and our findings are generally supportive of various theories that posit that an increase in enforcement of norms, in the likelihood of punishment, and in an increase of costs of repression should lead to a decrease in human rights violations. But our findings are also consistent with the norms literature (Risse, et al., 1999) which has stressed that both normative and coercive factors are important for human rights change. Human rights trials are not only instances of punishment or enforcement, but also high profile symbolic events that communicate and dramatize norms. It is difficult to separate these normative and performative aspects of trials from their punishment and enforcement effects. Our findings do not dispute that the normative effects of trials as well as the punishment may also be important for deterrence of future human rights violations.

We can not yet specify the exact mechanisms through which human rights trials contribute to improved human rights practices. If trials and truth commissions have an effect on future human rights practices, through what mechanisms are they working? The transitional human rights trials database shows a significant increase in the number of human rights trials in the world since the 1980s. In the past, state officials faced few costs for committing human rights violations; now they face more, especially in those countries that have been persistent in carrying out human rights trials. This suggests that there has been an increase in enforcement

which, in turn, heightens the expected costs of repression for government officials who make choices about how much repression to use. At the same time, there is no reason to believe that the benefits of repression have increased. So, if the benefits of repression have remained constant and the formal and informal costs of repression have increased, it seems reasonable to suggest that trials deter future human rights violations by increasing the perception of the possibility of costs of repression for individual state officials.

In particular, we are interested if trials deter actors capable of carrying out future human rights violations – mainly the security forces. By increasing sanctions, trials can modify the strategic context within which security forces operate. For security forces that have already carried out human rights abuses, the strategic landscape is straightforward: it is in their interests to prevent both truth telling and especially prosecution for past human rights violations. These are the so-called spoilers, who are often willing to go to great lengths to prevent prosecution. We do not believe that either trials or truth commission change their calculations very much. States can only affect their actions by preemption: by physically removing them from power through detention or imprisonment (Mendeloff, 2004). Given a choice, spoilers will always prefer no transitional justice at all, preferably guaranteed by an amnesty. They very often succeed in blocking domestic trials, through threats, coup attempts, blocked peace processes, and the like. This group has been the main concern of scholars like Snyder and Vinjamuri, who point out the obvious difficulties of such a strategic situation. What they miss, however, is the bigger and longer strategic game that trials can set in motion.

In the longer term, we believe that the actual deterrence of future human rights violations happens through the impact of trials on the new generations of military and police officers, and on civilian political leaders. Young officers who were not involved in the last round of

repression may look at their past leaders and draw conclusions about their future choices. They observe their past leaders, perhaps in jail through domestic trials, or with tattered international and domestic reputations. Future military officers may decide that trials have made repression and coups too costly for use in the future. How these leaders calculate costs may vary. Thus, some may interpret the costs of trials mainly in terms of the costs to the reputation and honor of the military or police as an institution; others may calculate the costs in a more individual fashion. Trials and truth commission created sanctions that did not exist before, including not only the possibility of imprisonment, but also damage to honor and national and international reputation.

We do not yet know the mechanisms through which trials have an impact, in part because insufficient research has been done on the reactions of the military and police to trials. Future research needs to focus on identifying the mechanisms through which human rights trials lead to an improvement in human rights practices. We currently cannot distinguish among the various costs imposed by human rights trials, but we believe that the economic and political costs of the formal sanctions (lost wages, litigation fees, inability to participate in elections while on trial or in jail, etc) and the informal social and political costs that arise from the publicity surrounding the trials (lost of reputation or legitimacy, and the resulting loss of political and social support) need to be taken into account in future research. Eventually trials may be one factor that can contribute to cultural change and the internalization of human rights norms among state officials, but that is still not the case in most countries, as witnessed by the revival of debates about the legitimacy of torture in the United States after 9/11.

Complying with human rights norms may require transitional countries to make substantial structural changes in the nature of their domestic institutions. Such changes are not

easy to make. Human rights trials are only one of the many factors that can contribute to positive human rights change. While they are not a panacea for human rights problems, they appear to be one form of sanction that can contribute to the institutional and political changes necessary to limit repression.

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## Appendix 1. List of Countries with Transition

### Democratic transitions (68 Countries)

Portugal 1974~; Greece 1974~; Spain 1975~; Thailand 1977~1991, 1992~; Burkina Faso 1977~1980; Peru 1978~1992, 1993~; Ghana 1978~1981, 1996~; Dominican Republic 1978~; Nigeria 1978~1984, 1998~; Uganda 1979~1985; Ecuador 1979~; El Salvador 1979~; Honduras 1980~; Bolivia 1982~; Argentina 1983~; Turkey 1983~; Guatemala 1984~; Brazil 1985~; Uruguay 1985~; Haiti 1986~1991, 1994~; Philippines 1986~; Republic of Korea 1987~; Hungary 1988~; Chile 1988~; Cambodia 1988~1997; Pakistan 1988~1999; Poland 1989~; Paraguay 1989~; Czechoslovakia 1989~1992; Panama 1989~; Romania 1989~; Benin 1990~; Nicaragua 1990~; Comoros 1990~1995, 1996~; Bulgaria 1990~; Fiji 1990~; Mongolia 1990~; Nepal 1990~2002; Albania 1990~1996, 1997~; Republic of Congo 1991~1997; Mali 1991~; Central African Republic 1991~2003; Niger 1991~1996, 1999~; Bangladesh 1991~; Madagascar 1991~; Zambia 1991~1996; Guinea-Bissau 1991~1998, 1999~; Azerbaijan 1992~; Guyana 1992~; South Africa 1992~; Taiwan 1992~; Lesotho 1993~1998, 1999~; Malawi 1993~; Mexico 1994~; Mozambique 1994~; Sierra Leone 1996~1997, 2001~; Iran 1997~2004; Armenia 1998~; Indonesia 1998~; Djibouti 1999~; Cote d'Ivoire 1999~2002; Senegal 2000~; Serbia and Montenegro 2000~; Kenya 2002~; Macedonia 2002~; Algeria 2004~;.

### Transition from civil war (16 Countries)

Chad 1984~; Lebanon 1990~; Ethiopia 1991~; Angola 1993~; Rwanda 1994~; Bosnia and Herzegovina 1995~; Burundi 1996~; Comoros 1996~; Liberia 1996~; Afghanistan 1996~; Guinea-Bissau 1999~; Lesotho 1999~; Sierra Leone 2001~; Iraq 2003~; Democratic Republic of Congo 2003~; Solomon Island 2003~;.

### Transition of state creation (28 Countries)

Namibia 1990~; Yemen 1990~; Germany 1990~; Georgia 1991~; Croatia 1991~; Slovenia 1991~; Serbia and Montenegro 1991~; Belarus 1991~; Moldova 1991~; Azerbaijan 1991~; Kyrgyzstan 1991~; Uzbekistan 1991~; Estonia 1991~; Latvia 1991~; Lithuania 1991~; Tajikistan 1991~; Macedonia 1991~; Armenia 1991~; Turkmenistan 1991~; Ukraine 1991~; Kazakhstan 1991~; Bosnia and Herzegovina 1992~; Russia 1992~; Czech Republic 1993~; Slovakia 1993~; Ethiopia 1993~; Eritrea 1993~; East Timor 2002~;.

## Appendix 2. List of Countries with Transitional Human Rights Trials and Truth Commissions

### Countries with Transitional Human Rights Trials (41 Countries)

Argentina 1983-90, 1993-96, 1998-04; Benin 1991-93; Bolivia 1983, 1995; Bosnia and Herzegovina 1995, 1999-2004; Bulgaria 1993, 1994, 1996; Burundi 1996; Chile 1991-2004; Croatia 1992-94, 1998-2004; Czech Republic 1997, 1998, 2001; Ecuador 1992-95, 1997; El Salvador 1990-92, 1998; Eritrea 1991, 1993; Ethiopia 1991-92, 1994-2003; Guatemala 1988, 1991-94, 1996-2003; Haiti 1986-87, 1989, 1995-97; Honduras 1992-93, 1996-97, 1999-2002, 2004; Hungary 1993, 1999, 2000-01; Indonesia 2000-04; Iraq 2004; Republic of Korea 1996; Lithuania 1997-2002; Malawi 1995; Mali 1991, 1993; Mexico 2002-2004; Namibia 1990; Nicaragua 1992-96; Niger 1992; Panama 1991-99, 2002, 2004; Paraguay 1989, 1991-92, 1994-99, 2002-04; Peru 1985, 1990, 1993-95, 2001-04; Poland 1990, 1993-94, 1996-2001; Portugal 1980; Romania 1990; Rwanda 1994-2004; Serbia and Montenegro 2001~04; South Africa 1992; Spain 1982; Thailand 1998; Turkey 1983; Uruguay 2002.

### Countries with Truth Commissions (28 Countries)

Argentina 1983; Bolivia 1982; Burundi 1996; Central African Republic 2003; Chad 1990; Chile 1990; Democratic Republic of Congo 2004; East Timor 2002; Ecuador 1996; El Salvador 1992; Germany 1992; Ghana 2002; Guatemala 1997; Haiti 1995; Indonesia 1999; Republic of Korea 2000; Liberia 2003; Nepal 1990; Nigeria 1999; Panama 2001; Paraguay 2003; Peru 2001; Philippines 1986; Serbia and Montenegro 2002; Sierra Leone 2002; South Africa 1995; Uganda 1986; Uruguay 1985

Table 1: Baseline Models: Impact of Human Rights Trials on Repression

	Model1(Base)		Model2a(TRT)		Model2b(CTRT)	
	Coef. (Std.Err.)	p-value	Coef. (Std.Err.)	p-value	Coef. (Std.Err.)	p-value
Trial Experience			-0.160 (0.078)	0.040		
Cumulative Trial Years ( <i>ln</i> )					-0.113 (0.058)	0.049
Truth Commission Experience			-0.178 (0.097)	0.066	-0.167 (0.097)	0.085
Repression $t-1$	0.572 (0.024)	<0.001	0.565 (0.024)	<0.001	0.565 (0.024)	<0.001
Democracy	-0.057 (0.008)	<0.001	-0.054 (0.008)	<0.001	-0.055 (0.008)	<0.001
International Conflict	-0.173 (0.087)	0.046	-0.177 (0.086)	0.039	-0.173 (0.086)	0.046
Civil Conflict	0.469 (0.051)	<0.001	0.489 (0.052)	<0.001	0.486 (0.052)	<0.001
GDP per capita ( <i>ln</i> )	-0.085 (0.042)	0.042	-0.072 (0.043)	0.091	-0.076 (0.042)	0.071
Annual GDP Growth Rate	-0.008 (0.006)	0.202	-0.008 (0.006)	0.175	-0.008 (0.006)	0.180
Population ( <i>ln</i> )	0.537 (0.075)	<0.001	0.570 (0.076)	<0.001	0.573 (0.076)	<0.001
Annual Rate of Population Change	0.010 (0.038)	0.799	0.019 (0.039)	0.624	0.019 (0.039)	0.616
Africa	-0.481 (0.131)	<0.001	-0.549 (0.129)	<0.001	-0.596 (0.133)	<0.001
Oceania	-0.593 (0.193)	0.002	-0.743 (0.196)	<0.001	-0.759 (0.198)	<0.001
Asia	-0.247 (0.114)	0.031	-0.361 (0.116)	0.002	-0.400 (0.122)	0.001
Europe	-0.490 (0.120)	<0.001	-0.594 (0.125)	<0.001	-0.622 (0.127)	<0.001
Year	0.016 (0.006)	0.012	0.020 (0.006)	0.002	0.022 (0.007)	0.001
Constant	-1.609 (0.489)	0.001	-1.846 (0.502)	<0.001	-1.845 (0.499)	<0.001
R <sup>2</sup>	0.686		0.688		0.688	
$\chi^2$	3511.291		3521.593		3523.868	
N	1314		1314		1314	
Number of states	95		95		95	

Note: Table entries are OLS regression estimates corrected for panel-specific autocorrelation and p-values in Stata 9.2/SE. Panel corrected standard errors are included in the parentheses.

Table 2: Impact of Human Rights Trials on Repression under Civil Conflict Situation

	Model3a(TRT)		Model3b(CTRT)	
	Coef. (Std.Err.)	p-value	Coef. (Std.Err.)	p-value
Trial Experience	-0.174 (0.085)	0.041		
Civil War×Trial Experience	-0.027 (0.092)	0.769		
Civil War Transition×Trial Experience	0.199 (0.345)	0.564		
Cumulative Trial Years ( <i>ln</i> )			-0.118 (0.061)	0.054
Civil War×Cum. Trial Years			-0.156 (0.079)	0.047
Civil War Transition×Cum. Trial Years			0.244 (0.196)	0.213
Truth Commission Experience	-0.204 (0.117)	0.080	-0.208 (0.117)	0.076
Civil War×Truth Commission	-0.037 (0.107)	0.730	-0.061 (0.106)	0.563
Civil War Transition×Truth Commission	0.223 (0.350)	0.523	0.238 (0.352)	0.499
Repression $t_{-1}$	0.556 (0.024)	<0.001	0.554 (0.024)	<0.001
Democracy	-0.053 (0.008)	<0.001	-0.055 (0.008)	<0.001
International Conflict	-0.193 (0.084)	0.022	-0.191 (0.084)	0.022
Civil Conflict	0.493 (0.070)	<0.001	0.541 (0.068)	<0.001
Transition from Civil War	0.288 (0.208)	0.166	0.298 (0.209)	0.153
GDP per capita ( <i>ln</i> )	-0.061 (0.044)	0.163	-0.060 (0.043)	0.164
Annual GDP Growth Rate	-0.009 (0.006)	0.131	-0.009 (0.006)	0.142
Population ( <i>ln</i> )	0.596 (0.077)	<0.001	0.604 (0.077)	<0.001
Annual Rate of Population Change	0.018 (0.038)	0.642	0.021 (0.037)	0.579
Africa	-0.629 (0.137)	<0.001	-0.704 (0.140)	<0.001
Oceania	-0.778 (0.203)	<0.001	-0.810 (0.204)	<0.001
Asia	-0.354 (0.117)	0.002	-0.415 (0.122)	0.001
Europe	-0.636 (0.129)	<0.001	-0.685 (0.129)	<0.001
Year	0.019 (0.006)	0.003	0.022 (0.007)	0.001
Constant	-2.040 (0.507)	<0.001	-2.103 (0.506)	<0.001
R <sup>2</sup>	0.690		0.691	
χ <sup>2</sup>	3615.824		3614.788	
N	1314		1314	
Number of states	95		95	

Note: Table entries are OLS regression estimates corrected for panel-specific autocorrelation and p-values in Stata 9.2/SE. Panel corrected standard errors are included in the parentheses.

Table 3: Impact of Human Rights Trials in Neighboring States on Repression

	TRT in Neighboring States				CTRT in Neighboring States			
	Model4a		Model4b		Model5a		Model5b	
	Coef. (Std.Err.)	<i>p</i> -value	Coef. (Std.Err.)	<i>p</i> -value	Coef. (Std.Err.)	<i>p</i> -value	Coef. (Std.Err.)	<i>p</i> -value
Trial Experience	-0.166 (0.078)	0.033	-0.129 (0.079)	0.102				
Trial Experience (Geographic Neighbors)	-0.295 (0.248)	0.233						
Trial Experience (Cultural Neighbors)			-0.344 (0.182)	0.058				
Cum. Trial Years( <i>ln</i> )					-0.114 (0.058)	0.049	-0.083 (0.060)	0.167
Cum. Trial Years( <i>ln</i> ) (Geographic Neighbors)					-0.184 (0.120)	0.125		
Cum. Trial Years( <i>ln</i> ) (Cultural Neighbors)							-0.178 (0.094)	0.059
Truth Commission	-0.194 (0.097)	0.046	-0.185 (0.097)	0.056	-0.183 (0.097)	0.060	-0.167 (0.097)	0.087
Repression $t_{-1}$	0.563 (0.024)	<0.001	0.563 (0.024)	<0.001	0.560 (0.024)	<0.001	0.563 (0.024)	<0.001
Democracy	-0.052 (0.008)	<0.001	-0.055 (0.008)	<0.001	-0.054 (0.008)	<0.001	-0.056 (0.008)	<0.001
International Conflict	-0.178 (0.086)	0.038	-0.168 (0.086)	0.051	-0.172 (0.086)	0.046	-0.168 (0.086)	0.051
Civil Conflict	0.488 (0.052)	<0.001	0.484 (0.052)	<0.001	0.487 (0.052)	<0.001	0.480 (0.052)	<0.001
GDP per capita( <i>ln</i> )	-0.074 (0.043)	0.084	-0.063 (0.042)	0.136	-0.078 (0.042)	0.066	-0.067 (0.042)	0.114
GDP Growth Rate	-0.008 (0.006)	0.193	-0.009 (0.006)	0.148	-0.008 (0.006)	0.193	-0.009 (0.006)	0.144
Population( <i>ln</i> )	0.572 (0.076)	<0.001	0.567 (0.076)	<0.001	0.585 (0.076)	<0.001	0.577 (0.076)	<0.001
Population Change	0.019 (0.039)	0.618	0.030 (0.040)	0.453	0.022 (0.039)	0.567	0.031 (0.040)	0.440
Africa	-0.630 (0.150)	<0.001	-0.692 (0.152)	<0.001	-0.749 (0.171)	<0.001	-0.757 (0.160)	<0.001
Oceania	-0.914 (0.247)	<0.001	-0.940 (0.221)	<0.001	-0.989 (0.254)	<0.001	-0.958 (0.225)	<0.001
Asia	-0.491 (0.167)	0.003	-0.522 (0.146)	<0.001	-0.608 (0.190)	0.001	-0.576 (0.156)	<0.001
Europe	-0.671 (0.144)	<0.001	-0.690 (0.129)	<0.001	-0.756 (0.157)	<0.001	-0.746 (0.137)	<0.001
Year	0.026 (0.008)	0.001	0.026 (0.007)	<0.001	0.032 (0.009)	0.001	0.030 (0.008)	<0.001
Constant	-1.784 (0.508)	<0.001	-1.803 (0.503)	<0.001	-1.849 (0.500)	<0.001	-1.870 (0.501)	<0.001
R <sup>2</sup>	0.688		0.688		0.688		0.688	
$\chi^2$	3534.155		3581.160		3527.263		3554.171	
N	1314		1314		1314		1314	
Number of states	95		95		95		95	

Note: Table entries are OLS regression estimates corrected for panel-specific autocorrelation and p-values in Stata 9.2/SE. Panel corrected standard errors are included in the parentheses.

Table 4: Impact of Human Rights Trials on Repression(Two-Stage Estimations)

	TRT instrumented				CTRTR instrumented	
	Model6a(G2SLS)		Model7(2SPLS)		Model6b(G2SLS)	
	Coef. (Std.Err.)	<i>p</i> -value	Coef. (Std.Err.)	<i>p</i> -value	Coef. (Std.Err.)	<i>p</i> -value
Trial Experience (instrumented)	-0.460 (0.220)	0.037	-0.105 (0.049)	0.034		
Cumulative Trial Years (instrumented, <i>ln</i> )					-0.283 (0.155)	0.068
Truth Commission Experience	-0.176 (0.112)	0.116	-0.163 (0.095)	0.087	-0.146 (0.109)	0.179
Repression $t_{-1}$	0.490 (0.024)	0.000	0.551 (0.002)	<0.001	0.513 (0.023)	<0.001
Democracy	-0.055 (0.009)	0.000	-0.051 (0.009)	<0.001	-0.058 (0.009)	<0.001
International Conflict	-0.218 (0.093)	0.020	-0.170 (0.091)	0.062	-0.189 (0.093)	0.042
Civil Conflict	0.556 (0.056)	0.000	0.524 (0.053)	<0.001	0.533 (0.054)	<0.001
GDP per capita ( <i>ln</i> )	-0.070 (0.049)	0.156	-0.062 (0.042)	0.139	-0.077 (0.046)	0.094
Annual GDP Growth Rate	-0.007 (0.006)	0.220	-0.008 (0.006)	0.169	-0.007 (0.006)	0.228
Population ( <i>ln</i> )	0.681 (0.094)	0.000	0.592 (0.078)	<0.001	0.653 (0.089)	<0.001
Annual Rate of Population Change	0.028 (0.041)	0.491	0.032 (0.038)	0.395	0.030 (0.040)	0.454
Africa	-0.629 (0.158)	<0.000	-0.585 (0.127)	<0.001	-0.731 (0.179)	<0.001
Oceania	-1.013 (0.415)	0.015	-1.338 (0.452)	0.003	-0.985 (0.395)	0.013
Asia	-0.532 (0.177)	0.003	-0.486 (0.148)	0.001	-0.589 (0.200)	0.003
Europe	-0.754 (0.161)	<0.001	-0.629 (0.132)	<0.001	-0.772 (0.165)	<0.001
Year	0.024 (0.007)	0.001	0.024 (0.007)	<0.001	0.028 (0.009)	0.001
Constant	-2.260 (0.659)	0.001	-2.202 (0.566)	<0.001	-2.153 (0.613)	<0.001
$\chi^2$	1926.880		528.10		2203.533	
N	1281		1281		1281	
Number of states	94		94		94	

Note: Table entries are G2SLS and 2SPLS estimates and p-values in Stata 9.2/SE. Corrected standard errors are included in the parentheses.

Table 5: Fixed-effects Models: Impact of Human Rights Trials on Repression

	Model8a(TRT)		Model8b(CTRT)	
	Coef. (Std.Err.)	p-value	Coef. (Std.Err.)	p-value
Trial Experience	-0.493 (0.159)	0.002		
Cumulative Trial Years ( <i>ln</i> )			-0.424 (0.106)	<0.001
Truth Commission Experience	-0.491 (0.175)	0.005	-0.417 (0.177)	0.019
Repression $t_{-1}$	0.280 (0.026)	<0.001	0.273 (0.026)	<0.001
Democracy	-0.046 (0.013)	<0.001	-0.046 (0.013)	<0.001
International Conflict	-0.347 (0.097)	<0.001	-0.332 (0.096)	0.001
Civil Conflict	0.534 (0.069)	<0.001	0.524 (0.069)	<0.001
GDP per capita ( <i>ln</i> )	-0.050 (0.141)	0.722	0.003 (0.141)	0.985
Annual GDP Growth Rate	-0.014 (0.005)	0.008	-0.014 (0.005)	0.008
Population ( <i>ln</i> )	-3.375 (1.415)	0.017	-2.944 (1.405)	0.036
Annual Rate of Population Change	-0.058 (0.052)	0.265	-0.056 (0.051)	0.281
Year	0.047 (0.015)	0.001	0.049 (0.014)	0.001
Constant	26.338 (10.243)	0.010	22.918 (10.173)	0.024
$R^2$	0.253		0.257	
N	1314		1314	
Number of states	95		95	

Note: Table entries are fixed effect regression estimates with standard errors in parentheses and p-values in Stata 9.2/SE. British law tradition variable and regional dummies are dropped and the estimates of country-specific dummies are not reported.

Table 6: Impact of Human Rights Trials on Repression(Two-Stage Estimations with Fixed-Effects)

	TRT instrumented Model9a(G2SLS)		CTRTR instrumented Model9b(G2SLS)	
	Coef. (Std.Err.)	<i>p</i> -value	Coef. (Std.Err.)	<i>p</i> -value
Trial Experience (instrumented)	-1.172 (0.332)	<0.001		
Cumulative Trial Years (instrumented, <i>ln</i> )			-0.740 (0.205)	<0.001
Truth Commission Experience	-0.417 (0.181)	0.022	-0.330 (0.187)	0.077
Repression $t_{-1}$	0.256 (0.028)	<0.001	0.252 (0.028)	<0.001
Democracy	-0.039 (0.013)	0.004	-0.041 (0.013)	0.002
International Conflict	-0.362 (0.098)	<0.001	-0.328 (0.097)	0.001
Civil Conflict	0.536 (0.071)	<0.001	0.526 (0.071)	<0.001
GDP per capita ( <i>ln</i> )	-0.042 (0.145)	0.773	0.056 (0.146)	0.701
Annual GDP Growth Rate	-0.013 (0.006)	0.019	-0.013 (0.006)	0.021
Population ( <i>ln</i> )	-3.694 (1.465)	0.012	-2.661 (1.429)	0.063
Annual Rate of Population Change	-0.057 (0.052)	0.280	-0.057 (0.052)	0.273
Year	0.059 (0.016)	<0.001	0.056 (0.016)	<0.001
Constant	28.646 (10.622)	0.007	20.616 (10.364)	0.047
$\chi^2$	11905.428		12068.918	
N	1281		1281	
Number of states	94		94	

Note: Table entries are two-stage fixed-effects regression estimates with standard errors in parentheses and p-values in Stata 9.2/SE. British law tradition variable and regional dummies are dropped and the estimates of country-specific dummies are not reported.