Breaking state impunity in post-authoritarian regimes: Why transitional justice processes deter criminal violence in new democracies

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Abstract

This article claims that cross-national variation in criminal violence in new democracies is highly dependent on whether elites adopt transitional justice processes to address a repressive past. State specialists in violence who repress political dissidents under authoritarian rule often play a crucial role in the operation of criminal markets and in the production of criminal violence in democracy. Some of them defect from the state to become the armed branch of criminal organizations in their deadly fights against the state and rival groups; others remain but protect criminal organizations from positions of state power; and still others use state power to fight criminals through iron-fist policies. When post-authoritarian elites adopt transitional justice processes to expose, prosecute, and punish state specialists in violence for gross human rights violations committed during the authoritarian era, they redefine the rules of state coercion and deter members of the armed forces and the police from becoming leading actors in the production of criminal violence. Using a dataset of 76 countries that transitioned from authoritarian rule to democracy between 1974 and 2005, we show that the adoption of strong truth commissions is strongly associated with lower murder rates; we also find that the implementation of trials that result in guilty verdicts is associated with lower homicide rates only when the trials are jointly implemented with a strong truth commission. In contrast, amnesty laws appear to stimulate criminal violence. Our findings are particularly robust for Latin America and remain unchanged even after addressing selection effects via matching techniques.

Keywords

criminal violence, Latin America, new democracies, transitional justice

One of the most significant developments in the wave of democratization that swept the world in the last quarter of the 20th century is that a number of new democracies today experience uncommonly high levels of criminal violence while others have become peaceful societies.[1]

A few decades after democratization, countries such as Brazil, Honduras, and Mexico experience the harsh realities of large-scale criminal violence and homicide epidemics while Bolivia and Chile remain relatively peaceful (UNDP, 2013). Large-scale criminal violence in new democracies has captured the world's attention because the death toll and lethality of criminal

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[1] We follow the United Nations in defining countries as having 'uncommon' or 'epidemic' levels of homicides when the annual murder rate surpasses 10 per 100,000 population (UNDP, 2013). As we argue below, in these countries organized criminal groups and state security forces account for a significant share of the murder rate.

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conflicts often surpass the death toll associated with civil wars.[2]

Why do some countries that transition from authoritarian rule to democracy experience epidemic levels of post-authoritarian criminal violence while others follow paths of peaceful development? Is there something about the democratization process itself that distinguishes the peaceful countries from the violent ones?

Dominant explanations suggest that socio-economic factors related to poverty and inequality explain differences in crime and violence across cities and countries. Sociologists suggest that the proportion of monoparental households, youth unemployment, and income inequality can give rise to criminal gangs and explain differences in homicide rates across US cities (Sampson, 1993). Economists conclude that increases in income inequality drive the poor to steal from the rich and explain differences in homicide and property theft across nations (Fajnzylber, Lederman & Loayza, 2002). And political economists suggest that criminal violence emerges in countries with failed states where organized criminal groups compete to control illicit markets (Skaperdas, 2001).

While socio-economic factors may be important drivers of variation of criminal violence across countries, these factors alone are insufficient to account for the widely differing experiences of criminal violence in post-authoritarian regimes. Consider the case of Latin America – a region that has contributed many instances to the recent wave of democratization and that shows both the world's highest levels of income inequality and the highest homicide rates. Although income inequality may explain why there is so much criminal violence in Latin America compared to the rest of the world, within Latin America we observe countries with very high levels of inequality that nonetheless have strikingly different rates of criminal violence. For example, El Salvador and Peru have similar levels of poverty and inequality and similarly weak states, and yet by 2010 El Salvador's murder rate was eight times greater than that of Peru (UNDP, 2013).

In his pioneering study of violent crime, Neumayer (2003) shows that political variables can trump key socio-economic factors, including inequality, in explaining variation in murder rates. His study reveals that

political regimes matter: hybrid regimes and new democracies experience higher homicide rates than closed autocracies and established democracies. It also reveals that how state coercive power is exercised can make a difference: countries where states violate human rights and where elites adopt iron-fist policies to fight crime experience higher murder rates. Rivera's (2016) study of homicides in Latin America confirms these findings: countries with unconstrained repressive states experience more criminal violence. Focusing on Central America, Cruz (2011) adds a crucial element: countries where post-authoritarian elites fail to implement comprehensive security-sector reforms to break the legacies of authoritarian repression experience large-scale criminal violence.

We take these political findings as our starting point and develop a new explanation of criminal violence across new democracies by looking into how post-authoritarian elites address past state repression. We claim that a crucial factor distinguishing new democracies that experience uncommonly high levels of criminal violence from those that experience more peaceful development is whether post-authoritarian elites adopt robust transitional justice processes – combining truth commissions and domestic trials – to expose, prosecute, and punish gross human rights violations perpetrated during the authoritarian era. We argue that by breaking state impunity and redefining the rules of state coercion, transitional justice processes can make new democracies less vulnerable to large-scale criminal violence, even when the socio-economic conditions that favor such violence are present.

We build our basic theoretical propositions on an empirical observation: students of organized crime have noted that military and police officers from the authoritarian era often play a crucial role in the development of criminal industries and in the production of criminal violence after democratization. Whether they defect to organized crime, protect criminals from positions of state power, or fight crime through iron-fist policies, authoritarian state specialists in violence are often associated with homicide epidemics that typically follow the unraveling of authoritarian regimes (Trejo & Ley, 2018).

Building on accountability arguments in the transitional justice literature (Roht-Arriaza, 2006; Sikkink, 2011), we argue that robust transitional justice processes can reduce post-authoritarian criminal violence through two mechanisms: removal and deterrence. By imprisoning repressive state specialists in violence through trials, new democratic elites remove potential state allies of criminal groups and prevent them from becoming key

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[2] For example, the death toll associated with Mexican drug wars between 2006 and 2012 (N = 70,000) is more than three times greater than the median death count of all civil wars of the second half of the 20th century (N = 19,000). See Trejo & Ley (2018).

players in the production of criminal violence. By jointly adopting robust truth commissions and trials, new democratic elites convey a powerful signal to all state actors that impunity will no longer be tolerated. To the extent that this message compels military and police forces to update their beliefs about the new rules of democratic accountability, transitional justice processes can become a powerful deterrent that will prevent security officers from taking actions that contribute to swelling murder rates.

We tested our propositions using the Transitional Justice Research Collaborative (TJRC) Dataset (Dancy et al., 2014) – the most complete dataset on transitional justice in the world. Focusing on all countries of the world that transitioned from authoritarian rule to democracy (N = 76) between 1974 and 2005, we assessed whether the adoption of transitional justice mechanisms had any impact on homicide rates. Controlling for the leading explanatory factors in the cross-national literature on violent crime, we show that the adoption of robust truth commissions to identify and expose human rights violators from the authoritarian period is associated with lower murder rates. Our findings reveal that trials of repressive officials that result in guilty verdicts have a deterrent effect on criminal violence only when these trials are jointly implemented with a robust truth commission and are part of a broader transitional justice process. In contrast, we show that the adoption of amnesty laws, by which states provide guarantees of no prosecution for past human rights violations, is associated with higher homicide rates. Our findings are particularly robust for Latin America and remain unchanged after we address selection effects.

The article is structured into five sections. We first review the cross-national literature on criminal violence. In the second and third sections we develop a political explanation of criminal violence in new democracies, and drawing on the transitional justice literature we develop our own propositions. In the fourth section we test our hypotheses, and in the conclusion we discuss the implications of our findings for the study of criminal violence and transitional justice.

Cross-national determinants of criminal violence: Extant explanations

Although the cross-national literature on criminal violence is relatively small, a number of important findings have emerged over the years. Following Becker's (1968) seminal proposition that individuals are less likely to engage in crime when their opportunity cost is high and when authorities are more likely to arrest and punish criminals, scholars have assessed the impact of economic and judicial/policing factors on murder rates. Using cross-regional samples, various studies have found validation for the opportunity cost hypothesis: wealthier countries (Neumayer, 2003) and countries with higher rates of economic growth (Fajnzylber, Lederman & Loayza, 2002) do experience lower homicide rates. Rivera (2016) finds that in Latin America countries with higher educational attainment experience lower homicide rates. In contrast, support for the deterrent effect of prosecution and punishment are mixed: while Rivera (2016) finds that judicial independence correlates with lower homicide rates in Latin America and Fajnzylber, Lederman & Loayza (2002) report a tentative association between police presence and lower murder rates, Neumayer (2003) finds that the death penalty stimulates, rather than deters, violent crime.

Drawing on Durkheim's ([1893] 1997) classic proposition that relative deprivation and social alienation can drive criminal action, scholars have tested for the impact of sociodemographic factors on murder rates. Using income inequality as an indicator of relative deprivation, the evidence is mixed: whereas Fajnzylber, Lederman & Loayza (2002) find a strong association between increases in inequality and higher homicide rates, Neumayer (2003) and Rivera (2016) find no association. The evidence on social alienation is more consistent: countries with a greater proportion of mono-parental families and a younger (male) population experience more violent crime (Neumayer, 2003; Rivera, 2016).

Perhaps because the study of criminal violence has been absent from political science or because there is no seminal political explanation of crime – comparable to Becker's in economics or Durkheim's in sociology – the literature on homicide rates has overlooked political variables. Drawing on the civil war literature, however, a few studies have tested for the impact of political regimes and reveal that homicide rates tend to be significantly higher in anocracies (semi-authoritarian/semi-democratic regimes) than in closed autocracies and open democracies (Fox & Hoelscher, 2015; Neumayer, 2003; Rivera, 2016). They also show that state repression and the use of iron-fist policies are associated with higher murder rates (Neumayer, 2003; Rivera, 2016).

We take these political findings about regime transitions and state coercion as our starting point and narrow down our inquiry into how post-authoritarian elites regulate the use of state coercion in new democracies. In the next section we outline a *political* explanation of criminal violence that emphasizes the extensive involvement of

repressive state security forces in the rise and development of organized crime in autocracies and how failure to hold them accountable for atrocities can render societies vulnerable to the outbreak of criminal wars and homicide epidemics in democracy.

A political explanation of criminal violence: Regime transitions and state coercion

The cross-national literature on criminal violence has established that murder rates tend to increase as countries outgrow autocracies and recede only after they become established democracies. Focusing on new democracies, various studies have identified outbreaks of violent crime epidemics shortly after democratization in such diverse cases as Brazil, El Salvador, Mexico, Russia, and South Africa. In all these cases, organized criminal groups (OCGs) – including mafias, drug cartels, human smugglers, kidnappers, private militias, racketeers, and street gangs – grew exponentially after the collapse of authoritarian rule and became leading actors in the production of criminal violence in democracy. Strikingly, members of security forces from the authoritarian regime, whether they retained their jobs after democratization or defected to OCGs after the collapse of authoritarian rule, have played a key role in the expansion of criminality and in the production of violence.

Rather than assume that OCGs and the state operate in separate spheres, as most economists and sociologists do (see Becker, 1968), we follow Arias (2006) and Snyder & Durán-Martínez (2009) in suggesting that OCGs require some level of informal state protection – typically provided by corrupt state security agents and by members of the judicial system – to successfully operate illegal markets. Following Trejo & Ley (2018), we recognize the existence of a *gray zone of criminality* in which state forces and OCGs overlap. Politics is relevant in this gray zone, because political regimes define how political power is distributed and how state coercion is exercised. Hence, to understand the terms of engagement between OCGs and the state, we need to contextualize this relationship within specific political regimes.

As the experience of a wide variety of authoritarian regimes shows, security forces charged with policing political dissent often play a major role in the development of criminal markets (Caldeira & Holston, 1999; Cruz, 2011; Snyder & Durán-Martínez, 2009; Varese, 2005). Members of special military and police forces often use their coercive power, access to information, and political impunity for gross human rights violations to protect and regulate the criminal underworld. Because
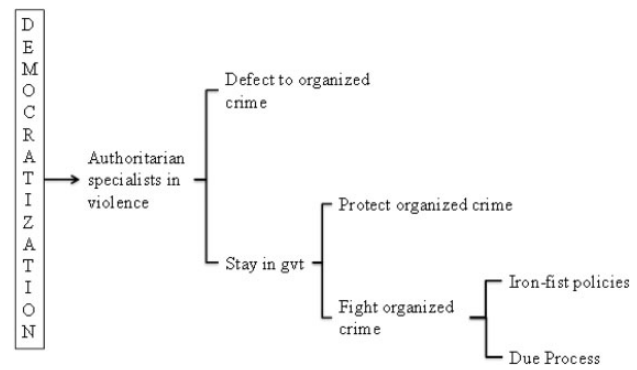

Figure 1. Trajectories of authoritarian specialists in violence after democratization

these specialists in violence can become extremely powerful, authoritarian leaders often buy their loyalty by transforming their *political impunity* into *criminal immunity* and allowing them to make significant economic profits from the criminal underworld. State specialists in violence have incentives to behave like monopolists, facilitate an undisturbed criminal environment, and keep state–criminal networks in the shadows.

Although scholars of democratic transitions have overlooked the criminal activities of authoritarian security forces and have focused on human rights violations, the breakdown of authoritarian regimes and the transition to democracy are likely to have major implications for state–criminal relations and for the operation of the gray zone of criminality. The breakdown of the autocratic order and the uncertainty about informal state protection are likely to generate incentives for OCGs to create their own private militias to defend their turf and conquer rival territories (Trejo & Ley, 2018). And the proliferation of militias is likely to lead to intense criminal competition and to the intensification of criminal violence (Skaperdas, 2001). At the same time, electoral competition in democracy is likely to drive elected officials to fight crime. State attacks against OCGs often lead to short-term increases in murder rates; whether violence subsides or grows depends on the nature of the state intervention and the use of coercion (Lessing, 2015).

Whether transitions to democracy lead to outbreaks of criminal violence or not depends to a large extent on the fate of authoritarian security forces after democratization. Figure 1 outlines different career trajectories that authoritarian specialists in violence can follow after democratization. They can defect from the new democratic government, join the criminal underworld, and become criminal lords or chiefs of private militias that

operate as the armed branch of OCGs. This is the path followed by a number of KGB agents after the collapse of the Soviet Union when they became leaders of the Russian mafia and recruited defectors from security forces and army veterans to serve as their private militias (Varese, 2005). Likewise, a group of elite forces from the Mexican military defected to create the Zetas, a lethal private militia that provided protection to the Gulf Cartel and has been at the forefront of Mexico's most deadly turf wars, killing thousands of civilians (Open Society Justice Initiative, 2016).

Alternatively, as Figure 1 suggests, authoritarian specialists in violence may remain and continue to provide protection to OCGs or seek to control criminal markets from a position of state power in democracy. A number of state-level police forces and subnational judicial authorities in Mexico followed this path and developed informal networks of protection for the country's major drug cartels (Trejo & Ley, 2018). When turf wars broke out, they used state coercive power and access to privileged information to favor their clients in their violent wars, contributing to the swelling of murder rates. High- and mid-ranking army officers in Guatemala also used their coercive power to protect families of criminal organizations engaged in drug trafficking (Cruz, 2011) and facilitated or simply failed to investigate thousands of killings perpetrated by their protégés. Corrupt officers from Brazil's state-level military police forces used their coercive power to violently regulate drug trafficking operations in the favelas of Rio de Janeiro (Arias, 2006) and contributed or failed to deter the growth of murder rates associated with deadly turf wars.

As Figure 1 illustrates, when authoritarian specialists in violence opt instead to fight organized crime, they tend to continue with the iron-fist policies that they previously used against dissidents, including torture and extralegal executions. In combating drug trafficking organizations, the Brazilian state-level military police forces – an institutional legacy from the authoritarian era – have become one of the world's most lethal law enforcement agencies. Police torture and extralegal executions of young black men have been extensively documented as common practices in Brazilian favelas (Caldeira & Holston, 1999). Unaccountable for their abuse of policing power, officers in these forces often capitalize on their impunity to profit from the criminal underworld – they brutally fight some criminal groups by day and protect others by night.

Finally, some authoritarian specialists in violence may decide to stay within the government and adopt new policing practices to fight criminal organizations through lawful democratic means. In democratic Chile, Peru, and Argentina, torture and extrajudicial killings are no longer widespread practices, even when security forces have to combat organized crime. In these cases, law enforcement agents do not use their coercive power to fight turf wars on behalf of OCGs or to allow them to kill their enemies and civilians. And when they combat them, they do not employ the repressive anti-insurgency strategies that were once used to eliminate political dissidents and often contribute to uncommonly high murder rates.

In the next section we explain why the adoption of transitional justice mechanisms can affect the career trajectories of authoritarian specialists in violence and the intensity of criminal violence in democracy.

Why transitional justice can deter criminal violence in new democracies

Over the past 50 years societies emerging from periods of sustained mass atrocities under authoritarian regimes have developed a family of judicial and non-judicial mechanisms to address a repressive past – including domestic trials of perpetrators of gross human rights violations; truth commissions; lustration and vetting; reparations for victims; institutional change to guarantee no repetition; and amnesties. This family of mechanisms constitutes the field of transitional justice. Within this field, three mechanisms have dominated the scholarly debate: amnesties, trials, and truth commissions.

One of the early views that pervaded the field came from scholars concerned about the *stability* of democratization processes. Arguing that domestic prosecutions could turn members of the military and the security forces of the outgoing authoritarian regime into 'democratic spoilers', they argued in favor of amnesty laws that granted official pardons and immunity against future prosecutions to authoritarian specialists in violence (Snyder & Vinjamuri, 2003).

Human rights scholars have forcefully argued that democratic stability should not come at the cost of *accountability* and *justice* (Méndez, 1997). In this view, the criminal prosecution of gross human rights violations is a moral obligation of post-authoritarian elites, as well as a strategic policy to deter future attacks against civilians and democratic institutions (Sikkink, 2011). By prosecuting authoritarian specialists in violence, new democracies send a clear signal that (future) non-democratic behaviors will be punished. As Acuña & Smulovitz (1997: 96) put it, 'the judicial process can establish that the costs of deserting the new rules of democracy are greater than the costs of acting within

it' and thus contribute to the creation of a democratic rule of law. Trials can be exemplary mechanisms that demonstrate that individual state agents will be held accountable for the unlawful use of state coercion.

While amnesties and trials have been associated with both stability and accountability arguments, the status of truth commissions is contested. Defined as formal state-sanctioned bodies that investigate and expose patterns of gross human rights abuses, truth commissions share the key feature that their findings are not legally binding (Hayner, 2001). This has led accountability scholars to suggest that truth commissions can have significant deterrent effects only when combined with trials (Méndez, 1997). In contrast, stability advocates suggest that truth commissions can be acceptable to democratic spoilers when they are part of amnesty programs but not when they are combined with trials (Snyder & Vinjamuri, 2003).

With the benefit of time and new cases, the initial propositions have given way to important theoretical reformulations. Scholars increasingly (1) recognize that a mechanism may have positive effects on one dimension (e.g. democratization) but not on others (e.g. human rights) (Olsen, Payne & Reiter, 2010); (2) advocate for the use of multiple mechanisms to satisfy demands for justice (Roht-Arriaza, 2006); and (3) encourage assessments that view transitional justice as a cumulative process whose results take several years to materialize (Sikkink, 2011).

Arguments in favor of or against transitional justice have only recently been put to empirical test with quantitative data. Scholars have focused on testing the impact of transitional justice mechanisms on human rights and democratic survival. They initially used dichotomous measures of the different mechanisms and more recently have produced more disaggregated data that allow for more nuanced testing.

Accountability arguments received strong empirical support in the initial tests. Using an original dataset of transitional justice mechanisms, Kim & Sikkink (2010) found that human rights prosecutions or truth commissions can each on their own deter human rights violations. But the joint implementation of prosecutions and truth commissions has a more powerful deterrent effect than either alone.

A qualified version of the stability argument – the justice balance approach – received initial support. Using their own database of transitional justice mechanisms, Olsen, Payne & Reiter (2010) found that selective human rights prosecutions can have a deterring effect on human rights violations when they are jointly implemented with amnesties. They claim that when democratic elites appease democratic spoilers through amnesties and punish only those involved in the most egregious human rights violations, transitional justice processes actually improve human rights conditions while at the same time increasing the prospects for democratic survival.

Led by accountability scholars and advocates of the justice balance approach (Dancy et al., 2014), the development of a new dataset – the TJRC – that measures the multiple dimensions of different transitional justice mechanisms has enabled a new round of more refined empirical testing. Dancy et al. (2015) show that domestic prosecutions and truth commissions do deter human rights violations – as argued by the accountability approach – but have no impact on democratic collapse or survival. In contrast, while amnesties can improve the prospects of democratic survival – as argued by stability and justice balance scholars – they compromise respect for human rights (even when jointly implemented with truth commissions).

Transitional justice as a deterrent of criminal violence

While scholars have focused mainly on the impact of transitional justice on human rights violations and democratic survival, we argue further that when post-authoritarian elites adopt transitional justice processes to investigate, expose, prosecute, and punish human rights violations committed during the authoritarian era, their actions can also make countries less vulnerable to epidemics of criminal violence. Transitional justice works through two mechanisms: removal and deterrence.

By initiating human rights prosecutions and sentencing members of elite military and police forces to prison, newly elected democratic incumbents de facto *remove* these specialists in violence from the criminal underworld. Consider the contrasting cases of two lethal military units that led anti-insurgency operations – the Grupo Colina in Peru and the Federal Security Directorate (DFS) in Mexico. Through an exemplary transitional justice process, combining a robust truth commission and a number of trials of repressive officials that resulted in conviction, members of Grupo Colina were exposed, prosecuted, and incarcerated for crimes against humanity committed during President Fujimori's anti-insurgency campaign in the 1990s (González-Ocantos, 2016). In contrast, crimes against humanity committed by elite military units affiliated to the DFS in southern Mexico in the 1970s remained unpunished, and DFS officers capitalized on this impunity to become

key players in the rise of Mexico's drug trafficking industry (Trejo & Ley, 2018). After the DFS disappeared, the lack of any punishment for past human rights violations allowed former DFS agents and a new generation of elite military forces – the Special Forces Airmobile Group (GAFE) – to enter the gray zone of criminality in the late 1990s. As Mexico transitioned to democracy, a significant number of GAFE commanders defected from the military and created the Zetas – the armed branch of the Gulf Cartel and Mexico's most lethal private militia.

By adopting robust truth commissions and implementing trials, new democratic incumbents send a powerful signal that influences state specialists in violence to change their expectations about the unlawful use of force (Sikkink, 2011) and update their beliefs about the high probability of being caught, exposed, and punished if they defect to, collude with, or protect OCGs or if they use extrajudicial methods to murder criminals. By investigating 'who did what to whom, how, and why', truth commissions not only expose global patterns of gross human rights violations (Hayner, 2001) and identify the institutions and specialists in violence who committed these violations in the past but also alert democratic officials to who should be kept under strict controls to avoid the expansion of the gray zone of criminality. By imposing criminal sanctions on past violations, transitional justice processes show that exposure will go beyond public moral shaming and will translate into material punishment (Sikkink, 2011). The simultaneous pursuit of truth and justice has deterred members of the military in Argentina, Chile, and Peru from joining organized crime and becoming key players in the production of criminal violence.

Consistent with the accountability approach, we would expect that:

> *H1:* Countries that adopt robust truth commissions to expose past human rights violations are likely to experience *lower* levels of criminal violence than those that adopt weak truth commissions or no truth revelation mechanism at all.
>
> *H2:* Countries that adopt meaningful trials of repressive officials for gross human rights violations that result in conviction are likely to experience *lower* levels of criminal violence than those that do not prosecute past atrocities.
>
> *H3:* Countries that jointly implement robust truth commissions and trials as part of a broad transitional justice process are likely to experience *lower* levels of criminal violence than those that adopt isolated mechanisms.

Because amnesties only partially expose but do not prosecute or punish human rights violators, they do not prompt specialists in violence to update their beliefs about the probability of being caught and punished if they protect or join the criminal underworld or if they use extralegal methods to fight crime in democracy. We would expect that:

> *H4:* Countries that adopt amnesty laws to exonerate past human rights violations are likely to experience *higher* levels of criminal violence than those that do not grant any amnesty.

In the following section we test these propositions in a global sample of countries that transitioned to democracy between 1974 and 2005.

Testing for the impact of transitional justice on homicide rates in new democracies

We tested our main theoretical propositions using the universe of countries that transitioned from authoritarian rule to democracy between 1974 and 2005. We classified cases based on a minimalist criterion: when a national executive leader was elected through free and fair elections, we provisionally classified the country as a case of transition to democracy. In our final list, however, we only included countries that remained democratic for five consecutive years after the first free election.[3] Following this procedure we identified 76 cases of democratic transition (see Table A.I in the appendix). Because we wanted to establish a baseline for analysis, we included information for all our countries' covariates starting five years prior to each country's transition and up until 2010 – five years after the last transition in our sample. For example, because Greece transitioned to democracy in 1974, for Greece we included data from 1969 to 2010. But since Mexico transitioned to democracy in 2000, for Mexico we entered data from 1995 to 2010. Note that the largest number of transitions took place in Europe, Africa, and Latin America, but we have more country-year observations from Europe and Latin America because democracy was more stable in these two world regions (see Table A.II in the appendix).

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[3] We relied on information from Polity IV (http://www.systemicpeace.org/polity/polity4.htm), Cheibub, Gandhi & Vreeland (2010), and Hyde & Marinov (2012). See appendix for details.

The dependent variable: Homicide rates
We used homicides per 100,000 population – as reported by the World Health Organization (WHO),[4] the United Nations Office on Drugs and Crime (UNODC),[5] the World Bank (WB),[6] and the Homicide Monitor (HM)[7] – as our indicator of criminal violence. While we acknowledge that the homicide rate can be a problematic metric of criminal violence because it reports murders committed by citizens with no connections to criminal organizations as well as by members of OCGs, more fine-grained quantitative information that identifies murders that can be attributed to OCGs is only exceptionally available for a few countries. In the Mexican case, for example, various estimates suggest that up to two-thirds of overall homicides can be attributed to conflicts between OCGs or between the state and OCGs (Calderón, Rodríguez & Shirk, 2018). In Brazil police reports reveal that up to 80% of overall homicides in the state of Rio de Janeiro can be attributed to conflicts associated with OCGs (O Globo, 2018). Other studies report that in El Salvador, Guatemala, Honduras (Cruz, 2011), and South Africa (Samara, 2011), the majority of murders can be attributed to conflicts associated with OCGs.

Based on these studies and on country profiles reported by the UNDP (2013), we follow the United Nations in suggesting that in countries such as Mexico, Brazil, El Salvador, Guatemala, Honduras, and South Africa, where homicide rates are above 10 per 100,000 population, the majority of murders can be attributed to OCGs. But in countries such as Chile, Argentina, and Uruguay, where homicide rates are under ten per 100,000, most murders can be attributed to ordinary criminals. This means that in violent countries the homicide rate captures a large share of organized-crime violence.

We combined information from four different sources reporting homicide statistics because no single institution covers the entire period under analysis for all relevant countries (see appendix for a detailed explanation). Although the WHO is the most reputable source, reporting homicide statistics since 1950, their data have significant information gaps for relevant countries and relevant time periods. Because interinstitutional information sharing is common, the four sources are highly

correlated (0.96), with no significant outliers. Rather than select a single source and impute the missing data, we averaged the information from all four sources to increase coverage as much as possible, and we left the missing data unaltered.

Despite the use of multiple sources, there is a persistent problem of missing data (see appendix for a detailed discussion). In the final analysis, 22 of the 76 cases dropped out from the analysis entirely due to missing homicide data. All of them were from Africa and Asia. We did not lose any case from Europe or Latin America (with the exception of Grenada) (see Table A.I and A.II in the appendix).

Because democracy proved to be more stable and because homicide statistics are more complete in Europe and Latin America, the majority of the cases are from these two world regions. Without missing data, Europe and Latin America should account for 60% of the sample. Due to missing data, however, they represent 84% of the cases. Despite this geographic unbalance, we present results from a global test because several countries in the Latin American sample share important features with countries from other world regions that are typical of developing countries. We believe this global sample captures dynamics that were prevalent in the cases that we seek to explain: late 20th-century new democracies.

Explanatory variables: Transitional justice mechanisms
To test for the impact of different transitional justice mechanisms on homicide rates in new democracies, we used information from the TJRC Dataset (Dancy et al., 2014). TJRC covers truth commissions, domestic prosecutions, and amnesties and provides extensive information about a number of characteristics of these mechanisms, allowing scholars to go beyond simply testing for the presence or absence of a mechanism and to create ordinal measures that distinguish between weak and robust transitional justice processes.

Following Dancy et al. (2015), we created an index of truth commissions that distinguishes weak from robust cases depending on: the number of years that a commission was active; the commission's budget and personnel; the number of gross human rights violations stipulated in its mandate; whether the commission took testimonies of victims; the level of publicity and recognition by state authorities of the commission's final report; and whether the commission recommended legal action against perpetrators of violence. We identified three levels – weak (1), medium (2), and robust (3) – and constructed an ordinal scale (see Table A.III in the

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[4] http://www.who.int/healthinfo/mortality_data/en/.
[5] http://www.unodc.org/unodc/en/data-and-analysis/homicide.html.
[6] http://data.worldbank.org/indicator/VC.IHR.PSRC.P5.
[7] http://homicide.igarape.org.br.

appendix). For example, we ranked a truth commission as robust when its mandate lasted more than two years, it had a large budget and personnel, and it focused on more than two gross human rights violations. A robust truth commission published a final report with recommendations for judicial action against perpetrators and the report was acknowledged by high-ranking state authorities. If not all features ranked at the same level, our ranking decision was based on three features: mandates, testimonies, and publicity. At least two of these features had to be present for the case to remain on the rank. According to this classification, only 20% of all truth commissions qualify as robust, including those of Argentina, Chile, Peru, Lithuania, and South Africa. Examples of weak truth commissions include those of Honduras, Serbia, and the Central African Republic.

To assess the impact of human rights prosecutions on criminal violence, we categorized trials according to the outcome and used a dummy variable to identify those country years in which there was at least one trial that resulted in a guilty verdict (regardless of the ranking of the violator). The reference category is the sum of no trials and trials that resulted in no guilty verdict. We are interested in capturing the signaling effect of convictions for past atrocities to the country's security forces. In 7% of the country years in our sample there was at least one guilty verdict. To understand the importance of identifying robust trials, consider the contrasting cases of Mexico and Peru. Although Mexican authorities adopted multiple isolated trials for human rights violations, between 2000 and 2010 Mexico only had *one year* with at least one guilty verdict. In contrast, in Peru, where post-authoritarian elites launched a broad transitional justice program, during the same time period the country scored *six years* with at least one guilty verdict.

To assess the impact of amnesty laws on criminal violence, we used a dummy variable to identify countries that granted amnesties to authorities and security officials from the outgoing authoritarian regime. Several amnesty laws were passed by democratic regimes (e.g. El Salvador and Argentina) but, most importantly, the overwhelming majority of amnesty laws passed in autocracies were not abrogated by newly elected democratic elites (e.g. Brazil). We consider authoritarian amnesty laws that persisted in democracy and those passed in democracy to be equivalent. As Sikkink (2011) suggests: regardless of who passes the law, 'amnesties are designed to prevent trials'. Amnesties were prevalent in 40% of the cases, particularly in Latin America and Africa.

Because transitional justice processes take time to transform government behavior and institutional practices, following Dancy et al. (2015) we assessed both the

immediate and the cumulative effects. We tested for the impact of each mechanism in the year in which it was implemented (the immediate effect) and labeled these variables as *Truth commission*, *Trials*, and *Amnesties*. We also tested for the cumulative effect by adding the yearly scores for each mechanism over time with a 1% annual depreciation rate to capture the declining impact that these mechanisms may have over time (Gerring, Thacker & Alfaro, 2012). For example, Peru implemented a Truth Commission that operated for three years and that we classified as 'robust' (level 3). For y1 we assigned a value of 3, for y2 a value of 5.97 (3 plus 3 minus 1% depreciation), and for y3 a value of 8.91 (5.97 plus 3 minus 1% depreciation). After y3, when the commission's work had been formally completed, we assume that Peru's truth-seeking process had reached its highest value, and thereafter simply depreciated 8.91 by 1% annually.[8] We labeled these variables *Truth commission$_{cumulative}$*, *Trials$_{cumulative}$*, and *Amnesties$_{cumulative}$*. To test for H3 we created an interaction of *Trials$_{cumulative}$* × *Truth commission$_{cumulative}$*.

We used different specifications of the transitional justice mechanisms to reflect debates in the literature. While scholars initially emphasized the immediate effect of isolated mechanisms on democracy (e.g. trials stimulating coups), over time they increasingly underscored the long-term cumulative effect of combined mechanisms on democracy and human rights and began speaking about *transitional justice processes*. Because these mechanisms are high-profile events, as soon as authorities publicly announce a truth commission or a trial for gross human rights violations these actions are likely to operate as accountability mechanisms that have an immediate effect on the behavior of state specialists in violence. Whether these changes prove to be permanent, however, may depend on the joint and persistent implementation of these mechanisms over time.

Controls

We controlled for the leading explanatory factors from the criminal violence and transitional justice literatures. Table A.IV in the appendix summarizes the descriptive statistics for all control variables.

To address economic concerns about the opportunity cost of engaging in violent crime, we controlled for a country's economic wealth (*Ln GDP per capita*)[9] and

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[8] See Figure A.I in the appendix.
[9] http://databank.worldbank.org/data/reports.aspx?%20source= world-development-indicators.

economic growth (*GPD growth rate*).[10] To confront issues of relative deprivation, we controlled for income inequality (*Gini index*),[11] and to address concerns about social alienation, we included a measure of the percentage of young males.[12] To control for the efficacy of judicial systems and for the probability of arrest, we tested for judicial independence.[13] In the final analysis we dropped judicial independence because it was highly correlated with GDP per capita.

We also controlled for a series of political and conflict variables. Based on a detailed assessment of all 76 transitions, we used a dummy variable to identify the cases of regime rupture – transitions to democracy in which the actors and political institutions of the authoritarian regime collapsed. Controlling for this type of political transition is important, because authoritarian security forces can play a crucial role in post-authoritarian criminal violence. Following this logic, we also controlled for state repression using the Political Terror Scale (PTS).[14]

Drawing information from the UCDP/PRIO Armed Conflict Database (Themnér & Wallensteen, 2014) we also controlled for the cumulative number of peace-years per country, starting with the fifth year prior to the transition to democracy. PRIO uses a threshold of 25 annual battle-deaths to identify a civil conflict and uses a value of 1 to identify country-year conflicts. We define a peace-year as one in which a country did not experience a civil conflict and use a value of 1 to identify peace-years. Armed insurgency and civil war – or their absence – are likely to affect murder rates. Because there is an inertial component in criminal violence (Fajnzylber, Lederman & Loayza, 2002), following a widespread practice in the literature we controlled for a one-year lag of the homicide rate.

For purposes of statistical modeling we used Generalized Least Squares (GLS) with country and year fixed effects. GLS allows us to relax the classic linear regression assumption that errors are independent and identically distributed. We included country and year fixed effects (FE) to control for unobservable characteristics within countries that may impact the outcome variable. FE removes the impact of these characteristics so we can assess the net effect of our covariates on the homicide

rate.[15] We clustered the standard errors by country. Because a country's ability to produce homicide statistics can improve over time, we ran additional tests with a time-trend variable. The results (not shown) remain unchanged.

Results

Our findings for the global sample, summarized in Table I, provide support for the accountability argument. As results in Models 1–3 show, truth commissions have a consistent cumulative deterrent effect on the murder rate but no immediate impact. Estimations based on Model 3 show that as we move from the lowest to the highest score of cumulative truth, on average the predicted homicide rate drops from 10.75 homicides per 100,000 population to 6.67. Amnesties are consistently associated with higher murder rates in the year when the amnesty law goes into effect. Estimations based on Model 3 show that the predicted homicide rate on average increases from 9.81 when there is no amnesty to 11.34 when an amnesty law is in force. While trials have no discernible effect in Models 1–3, Model 4 shows that when trials are conducted in isolation they can be associated with higher murder rates. But when they are implemented in the context of a robust truth commission, trials can become a long-term deterrent of criminal violence.

Economic controls follow predictable patterns: wealthier countries and countries with higher economic growth rates experience lower murder rates. Sociological controls show some unusual results. The percentage of young males is not statistically significant (perhaps this effect is more effectively captured using subnational units) and inequality is negatively associated with the homicide rate (perhaps because our sample only includes new democracies). Political controls show mixed effects. As expected, countries in which the authoritarian regime collapsed experience lower criminal violence. But state repression and peace-years have no discernible effect across models. Finally, murder rates seem to have a strong inertial component.

Because Latin America is the world region with the greatest number of truth commissions, trials, and amnesty laws (Sikkink, 2011), we conducted additional tests focusing exclusively on Latin American cases. As Table II reports, the results for Latin America are fairly consistent with the results from the global sample.[16]

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[10] http://databank.worldbank.org/data/reports.aspx?%20source=world-development-indicators.
[11] Solt (2014).
[12] http://databank.worldbank.org/data/reports.aspx?%20source=world-development-indicators.
[13] Rios-Figueroa & Staton (2013).
[14] http://www.politicalterrorscale.org/.

[15] A Hausman test confirmed that the use of FE is more appropriate than random effects.
[16] Results from the control variables are also consistent. See Table A.V in the appendix.

Table I. The impact of transitional justice mechanisms on the homicide rate in new democracies, 1974–2005 (GLS fixed effects models)

	Model 1 (Immediate)	Model 2 (Cumulative)	Model 3 (Imm.+Cum.)	Model 4 (Interaction)
Transitional justice mechanisms				
Trials	0.307 (0.211)		−0.061 (0.158)	−0.121 (0.154)
Trials$_{cumulative}$		0.230 (0.172)	0.258 (0.178)	0.356† (0.210)
Truth commission	−0.001 (0.337)		0.080 (0.268)	0.043 (0.262)
Truth com.$_{cumulative}$		−0.124* (0.05)	−0.136** (0.047)	−0.052 (0.064)
Amnesties	1.500* (0.577)		1.531* (0.577)	1.406* (0.566)
Amnesties$_{cumulative}$		0.038 (0.029)	0.026 (0.029)	0.030 (0.029)
Trials$_{cumulative}$ × Truth com.$_{cumulative}$				−0.029† (0.015)
Controls				
Ln GDP per capita	−1.787* (0.867)	−1.738† (0.939)	−1.685† (0.951)	−1.942* (0.960)
GDP growth	−0.097† (0.053)	−0.093† (0.05)	−0.100† (0.051)	−0.098† (0.051)
Income inequality	−0.154* (0.076)	−0.142* (0.071)	−0.161* (0.075)	−0.166* (0.075)
% Young males	0.109 (0.203)	0.112 (0.208)	0.118 (0.195)	0.150 (0.197)
Regime rupture	−1.761* (0.776)	−1.477* (0.711)	−1.479* (0.618)	−1.417* (0.636)
State repression	0.405 (0.320)	0.383 (0.357)	0.449 (0.368)	0.465 (0.369)
Years of peace	0.068* (0.029)	0.042 (0.027)	0.041 (0.028)	0.044 (0.028)
Homicide rate (lagged)	0.725** (0.046)	0.718** (0.047)	0.715** (0.049)	0.715** (0.049)
Constant	20.37** (7.351)	20.30* (7.965)	19.85* (8.049)	21.70** (8.126)
Observations	750	750	750	750

$^\dagger p < 0.1$; $^* p < 0.05$; $^{**} p < 0.01$; standard errors clustered by country in parentheses.

Table II. The impact of transitional justice mechanisms on the homicide rate in Latin America, 1974–2005 (GLS fixed effects models)

Transitional justice mechanisms	Model 5 (Immediate)	Model 6 (Cumulative)	Model 7 (Imm.+Cum.)	Model 8 (Interaction)
Trials	0.302 (0.315)		0.163 (0.290)	0.118 (0.311)
Trials$_{cumulative}$		0.004 (0.125)	0.024 (0.120)	0.379† (0.187)
Truth commission	−0.664* (0.230)		−0.421* (0.170)	−0.630* (0.219)
Truth com.$_{cumulative}$		−0.275* (0.123)	−0.253* (0.108)	0.161 (0.145)
Amnesties	1.803* (0.660)		1.856* (0.740)	1.437* (0.600)
Amnesties$_{cumulative}$		0.065 (0.112)	0.028 (0.096)	0.016 (0.077)
Trials$_{cumulative}$ × Truth com.$_{cumulative}$				−0.096** (0.033)
Controls	YES	YES	YES	YES
Constant	24.46 (20.35)	30.27 (24.65)	26.52 (23.08)	18.82 (21.07)
Observations	277	277	277	277

$^\dagger p < 0.1$; $^* p < 0.05$; $^{**} p < 0.01$; standard errors clustered by country in parentheses.
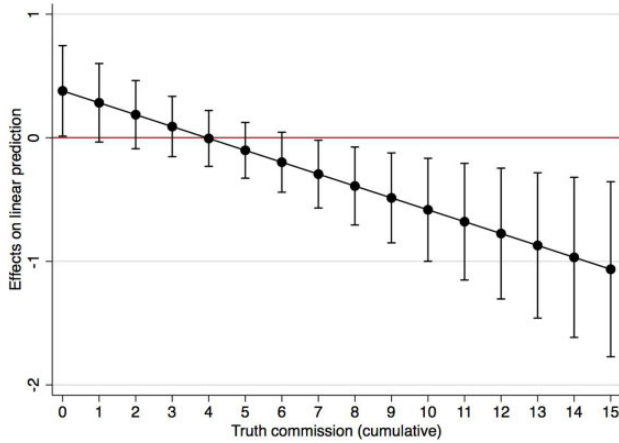
Figure 2. Average marginal effects of *Trials*$_{cumulative}$ at different levels of *Truth commission*$_{cumulative}$, using the Latin American sample (95% confidence intervals)

Figure 3. Homicide rate (linear prediction) at different levels of *Truth commission*$_{cumulative}$ and *Trials*$_{cumulative}$ using the Latin American sample (95% confidence intervals)

Truth commissions continue to be associated with lower murder rates; amnesties are associated with higher homicide rates; and trials become a deterrent of criminal violence only when jointly implemented with truth commissions. We find, however, one significant difference from the global sample: as Models 5–7 show, truth commissions in Latin America have both immediate and cumulative negative effects on the homicide rate.

Drawing on the results from Model 8, Figure 2 provides a visual illustration of the joint effect of trials and truth commissions on criminal violence in Latin America. It specifically shows the conditional impact of *Trials*$_{cumulative}$ on the murder rate at different country scores of *Truth commission*$_{cumulative}$. The figure reveals that in countries with no truth commissions, the implementation of trials can backfire and become a stimulant of criminal violence. But when countries adopt robust truth commissions, trials become a powerful deterrent of criminal violence. As Figure 2 shows, the deterrent impact of trials begins after the truth commission variable reaches a score of 7. As discussed above, a country adopting a robust truth commission would reach this score between the second and third year in operation, when the commissioners would have probably finished their investigation and drafted the report, and would be preparing to make it available to the judicial system and to the public.

To assess substantive effects, Figure 3 presents results of predicted homicide rates for countries with no trials (*Trials*$_{cumulative} = 0$) and with the highest levels of domestic prosecution (*Trials*$_{cumulative} = 11$) at different cumulative scores of truth. Holding all other variables at
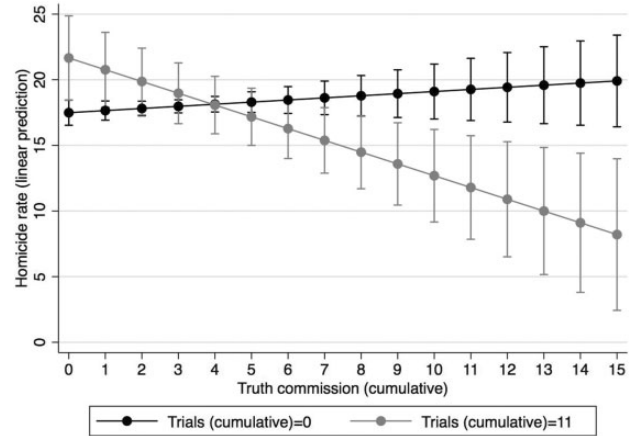
their mean values, our estimations based on Model 8 show that, on average, in a country with no prosecution for past human rights violations the implementation of truth commissions has no significant effect on the homicide rate. In contrast, in a country with high scores of domestic prosecution the implementation of a robust truth commission can over time have a powerful deterrent effect on the homicide rate. As Figure 3 shows, prior to the initiation of a truth commission (*Truth commission*$_{cumulative} = 0$) on average a country with high levels of prosecution would have a predicted homicide rate of 21.66 homicides per 100,000 population. But after trials and truth have reached their highest scores, the predicted murder rate drops to 8.2 – below the conventional threshold of epidemic violence.

Our results show that in isolation trials for past human rights violations may have no effect or even detrimental effects. Trials can backfire because they are centered on individual perpetrators, and even if they reach conviction they do not serve as representative evidence of the occurrence of gross human rights violations as *generalized* and *systemic* problems (Hayner, 2001). Hence, domestic prosecutions in isolation may inform repressive security forces that they are free to continue using their coercive power with impunity.

But when trials are part of a robust truth-seeking process, by which a truth commission documents generalized and systemic patterns of gross human rights violations, judicial prosecution becomes an exemplary punishment of state abuses. As Gloria Cano – the Peruvian attorney who led a team of lawyers in the trials against President Fujimori, Vladimiro Montesinos (head of the secret service), and members of the

paramilitary Grupo Colina – reported to us,[17] the findings of the truth commission, revealing patterns of gross human rights violations during the Fujimori administration, were crucial in the conviction of Fujimori and his colleagues. In the context of a widely publicized truth commission report, their conviction served as an *accountability shock* to the system.

The results from the global sample and from Latin America provide important evidence in favor of H1. New democracies that adopt robust truth commissions are likely to experience lower levels of criminal violence. This result would suggest that a crucial factor in explaining why ten years after democratization Peru experienced significantly *lower* levels of criminal violence than Mexico – despite a history of drug production and political violence in both countries – is Peru's adoption of an ambitious truth commission at the end of Fujimori's authoritarian era and Mexico's failure to look into the country's repressive past. This result would also suggest that a key factor explaining the differing trajectories in criminal violence between Lithuania (below epidemic levels) and Russia (above epidemic levels) during the first decade following the collapse of the Soviet Union is the adoption of a robust truth commission in the Baltic country but no truth-seeking process in Russia.[18]

Although our results disconfirm H2 – trials do not have an individual deterrent effect on the homicide rate – the findings in Model 4 and Model 8 show that the impact of trials on criminal violence is conditional on the joint existence of a robust truth commission, supporting H3. This result would suggest that the adoption of an ambitious transitional justice process in Peru, in which post-authoritarian elites jointly adopted a robust truth commission with a significant number of trials of repressive officials that resulted in conviction, explains why the Andean country has had murder rates below 10 per 100,000 population while Mexico has been trapped in major violence epidemics.

The results provide strong evidence in favor of H4 – the adoption of amnesty laws is consistently associated with higher levels of post-authoritarian criminal violence. In the global sample and in Latin America, amnesties have an immediate but not long-term impact. Because few countries adopted amnesty laws in isolation, a crucial question is whether amnesties might have

neutralized or even reversed the long-term deterring effect of accountability mechanisms. Consider El Salvador, a country with one of the highest murder rates in the world. Although El Salvador adopted a robust truth commission as part of a UN-sponsored peace agreement, civilian authorities in the democracy granted a generous amnesty to exonerate military and security forces from past human rights violations (Cruz, 2011). Results in Table A.VI in the appendix reveal that when countries combine truth commissions with amnesties, the long-term cumulative deterrent effect of truth on criminal violence wanes, as happened in El Salvador.

Robustness checks
A potential challenge to our results is that countries that adopt transitional justice mechanisms could be different from those that do not and these differences could explain levels of criminal violence. To address selection effects, using coarsened exact matching (CEM) techniques we created a new sample of countries that have equivalent probabilities of having adopted transitional justice mechanisms. The use of CEM allowed us to create a quasi-experimental sample of comparable treatment and control units (Iacus, King & Porro, 2012). Because the adoption of robust truth commissions was the transitional justice mechanism that consistently had the most significant and substantive influence on post-authoritarian criminal violence, we dichotomized *Truth commission*$_{cumulative}$ and used it as our treatment variable. With this preprocessed dataset, we re-estimated all the models from the global sample.

The results, reported in Table III, are consistent with our key findings using the global sample and Latin America. Truth commissions continue to have immediate and cumulative deterrent effects on the murder rate, and amnesties are associated with higher immediate levels of violence. One difference is that trials are consistently associated with a higher murder rate. As in previous models, we have evidence of the deterrent effect of trials on criminal violence when they are jointly implemented with a robust truth commission. As shown in Models 12 and 13, however, the joint impact of truth and justice is only statistically significant when we reduce the model to the most basic structural determinants of criminal violence and reach a more balanced sample (as in Model 13; for a graphical representation of this results see Figure A.II in the appendix).

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[17] Mexico City, 21 October 2016.
[18] See Pettai & Pettai (2015) on transitional justice in the Baltics.

Table III. The impact of transitional justice mechanisms on the homicide rate in new democracies, 1974–2005 (GLS fixed effects models; subsample defined using coarsened exact matching; Treatment = *Truth com.*$_{cumulative}$)

Transitional justice mechanisms	Model 9 (Immediate)	Model 10 (Cumulative)	Model 11 (Im.+Cum.)	Model 12 (Interaction)	Model 13 (Reduced)
Trials	0.623*		0.622*	0.602*	0.629*
	(0.258)		(0.262)	(0.263)	(0.253)
Trials$_{cumulative}$		0.004	–0.038	0.021	0.096
		(0.087)	(0.09)	(0.107)	(0.104)
Truth commission	–0.520**		–0.415**	–0.412**	–0.316†
	(0.141)		(0.146)	(0.146)	(0.164)
Truth com.$_{cumulative}$		–0.169**	–0.153**	–0.106	–0.02
		(0.043)	(0.044)	(0.064)	(0.062)
Amnesties	1.257**		1.393**	1.332**	0.738†
	(0.439)		(0.441)	(0.445)	(0.432)
Amnesties$_{cumulative}$		–0.022	–0.025	–0.021	0.037
		(0.038)	(0.037)	(0.037)	(0.036)
Trials$_{cumulative}$ × Truth com.$_{cumulative}$				–0.014	–0.027†
				(0.014)	(0.014)
Controls	YES	YES	YES	YES	YES^{η}
Constant	10.07	11.52	13.88†	14.03†	14.88*
	(8.115)	(8.334)	(8.212)	(8.213)	(7.417)
Observations	579	579	579	579	662

$^{†}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; standard errors clustered by country in parentheses. $η$: To increase the overall balance in the sample, we matched on all covariates except for *GDP growth* and *% Young males*.

Conclusion

One of the most important findings in the study of political violence in recent years suggests that the adoption of transitional justice mechanisms to investigate, expose, and punish gross human rights violations perpetrated during a previous authoritarian era renders citizens less vulnerable to state repression in democracy (Sikkink, 2011; Dancy et al., 2015). In this article we have extended this finding to the sphere of criminal violence and have shown that the adoption of broad transitional justice processes can also make new democracies less vulnerable to large-scale criminal violence.

Our findings have important implications for the study of criminal violence and transitional justice. First, whereas the study of criminal violence has been dominated by arguments about the socio-economic motivations that drive *citizens* to engage in violent crime and about political and judicial institutions that may deter *citizens* from engaging in illegal activities, our theoretical reformulations and empirical findings seek to shift the focus of attention from citizens to *state specialists in violence*. Contrary to arguments claiming that stronger security forces can suppress criminal violence through iron-fist policies, our findings underscore the importance of state accountability. Rulers in authoritarian regimes often allow state specialists in violence to transform their

political impunity (to repress political dissidents) into criminal immunity (to control the criminal underworld), but we have shown that when post-authoritarian elites adopt robust transitional justice processes to end political impunity, societies become less vulnerable to large-scale criminal violence.

Second, unlike studies that view truth commissions as symbolic measures that assuage moral indignation but have no impact on the development of peaceful societies, our findings strongly suggest that a robust truth commission can be the centerpiece of a broad transitional justice process and the single most important mechanism for rendering post-authoritarian societies less vulnerable to violence epidemics. Because truth commissions generate invaluable information about generalized and systemic patterns of gross human rights violations that can be used by citizens and judicial authorities to expose and prosecute abusive state officials, they can become a powerful tool to deter state specialists in violence from becoming major actors in the production of criminal violence.

Third, our results show that transitional justice mechanisms can have different and sometimes opposing effects. In our models, accountability mechanisms – truth commissions and trials combined with truth commissions – are associated with less criminal violence, but

stability mechanisms – amnesty laws – are associated with more violence. While amnesty laws can facilitate transitions to democracy by neutralizing democratic spoilers (Olsen, Payne & Reiter, 2010), by exonerating authoritarian specialists in violence new democratic elites can unwittingly stimulate the violent expansion of the criminal underworld.

In the end a clear pattern emerges: whether we look at truth commissions, trials, or amnesties, our results consistently show that policies aimed at breaking state impunity for past human rights violations make new democracies less vulnerable to epidemics of criminal violence and contribute in fundamental ways to the development of peaceful democracies.

Replication data

The Online appendix, dataset, and do-files for the empirical analysis in this article can be found at http://www.prio.org/jpr/datasets.

Acknowledgements

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Appendix

Classifying democratic transitions

Defining the universe of democratic transitions for the 1974–2005 period is not a simple task because there are major conceptual and measurement disagreements between the two most widely used datasets that classify political regimes: the Polity and the Cheibub, Gandhi & Vreeland (CGV) datasets.

Using Polity's –10 to +10 scale of levels of electoral competition, political contestation, citizen participation, and checks and balances, authors classify countries as democratic when they score a level of 7 or more. On this scale, a democratic transition is identified when a country score moves beyond 6. Alternatively, using a minimalist definition of democracy by which countries are classified as democratic only when leaders are elected through free and fair multiparty elections *and* when there is rotation of parties in power, CGV classifies countries as democracies and dictatorships (Cheibub, Gandhi & Vreeland, 2010). A democratic transition is identified only when there is evidence of competitive elections and when parties alternate in office. For the 1974–2005 period Polity and CGV only agree on classifying transitions to democracy in one-third of the cases. This means that whether we use one classification or the other is likely to affect results in fundamental ways.

Rather than using the two datasets independently, we used the information from Polity and CGV to identify a new list of cases of democratic transition. Consistent with our definition, we identified a case of democratic transition when a national executive leader was elected for the first time through free and fair competitive elections. If the leader was no longer elected through free and fair elections, we no longer considered it to be a democracy and dropped it from the dataset. We only included as successful transition cases those countries that remained democratic for at least *five years* following the first democratic election.

In classifying countries, we proceeded as follows. If both Polity and CGV agree that country X experienced a transition to democracy and both agree on the year of the transition, we included the case in our list of democratic transitions. If both sources agree that there was a transition but disagree on the year in which transition took

Table A.I. Democratic transitions, 1974–2005

Country	Transition Year	Country	Transition Year
Greece	1974	Poland	1991
Portugal	1976	*São Tomé and Príncipe*	1991
Spain	1977	Slovenia	1991
Dominican Republic	1978	Suriname	1991
Solomon Islands	1978	Ukraine	1991
Ecuador	1979	*Zambia*	1991
Bolivia	1982	*Congo, Republic of*	1992
Honduras	1982	Croatia	1992
Argentina	1983	*Fiji*	1992
Cyprus	1983	Guyana	1992
Turkey	1983	*Mali*	1992
El Salvador	1984	*Central African Republic*	1993
Grenada	1984	*Ghana*	1993
Brazil	1985	*Madagascar*	1993
Nicaragua	1985	Mongolia	1993
Uruguay	1985	Paraguay	1993
Bangladesh	1986	Malawi	1994
Guatemala	1986	Namibia	1994
Philippines	1986	South Africa	1994
Korea, Republic of	1988	*Taiwan*	1996
Pakistan	1988	Kenya	1998
Czechoslovakia	1989	Sierra Leone	1998
Panama	1989	Indonesia	1999
Sri Lanka	1989	*Nigeria*	1999
Bulgaria	1990	Mexico	2000
Chile	1990	*Niger*	2000
Hungary	1990	Russia	2000
Romania	1990	*Senegal*	2000
Albania	1991	Serbia	2000
Armenia	1991	Peru	2001
Benin	1991	East Timor	2002
Cape Verde	1991	*Lesotho*	2002
Estonia	1991	*Comoros*	2004
Latvia	1991	Georgia	2004
Lithuania	1991	*Guinea-Bissau*	2004
Macedonia	1991	*Burundi*	2005
Moldova	1991	Kyrgyzstan	2005
Nepal	1991	Lebanon	2005

Italics denote countries that were dropped from the analysis due to missing homicide data (see discussion below).

place, we relied on the National Elections across Democracy and Autocracy (NELDA) dataset (Hyde & Marinov, 2012) and on secondary sources to determine when the first free and fair multiparty election was held. If Polity and CGV disagree on whether a transition took place (e.g. Polity considers South Africa 1994 and Namibia 1994 to be cases of democratization but CGV does not), we used a wide variety of alternative sources, including NELDA, international electoral reports, and domestic news reports, to define whether there was sufficient evidence of free and fair multiparty

elections. Table A.I reports our full list of democratic transitions.

Table A.II summarizes data about the transition cases used in the analysis by world region. The first two columns identify the absolute number and proportion of transitions. The data report that Europe (28.95%), Africa (27.63%), and Latin America (23.68%) experienced the largest number of transition cases. Because transitions in Europe and Latin America occurred earlier and proved to be more stable than in Africa – that is, democracy more commonly persisted after five years –

Table A.II. Transitions from authoritarian rule to democracy by world region, 1974–2005

	Countries	%	Country-years	%	% Obs. used*
Africa	21	27.63	417	22.56	6.40
Asia	15	19.74	326	17.64	8.82
Europe	22	28.95	618	33.44	47.19
Latin America & Caribbean	18	23.68	487	26.35	37.60
Total	76	100		100	100

*Country-years used due to data availability on homicide rates.

Table A.III. Robustness of truth commission index

Level	No. of years	Budget and personnel	Gross human rights viol.	Testimonies	Publicity	Recommendation
1 (Weak)	Less than 1 yr	Not specified	Not specified	Not specified	Not specified	Not specified or no recomm.
2 (Medium)	B/w 1 and 2 yrs	Fewer than five commissioners *or* under 1 million dollars	Fewer than two	Either victims *or* perpetrators	Published	Recommend legal changes and/or reparations
3 (Robust)	More than 2 yrs	More than five commissioners *or* more than 1 million dollars	More than two	Both victims *and* perpetrators	Published *and* acknowledged by state authorities	Recommend judicial action against perpetrators

when we analyze country-years (see third and fourth columns) these two world regions account for 59.79% of the cases. However, as reported in the fifth column, due to missing homicide data from Africa and Asia, in our final analysis Europe and Latin America account for 84.79% of the cases. We analyze missing data in detail below.

For purposes of statistical analysis, we included in our dataset information for each democratic country, starting five years prior to the date when it became a democracy, and extended the information until 2010, as long as the country continued to elect government authorities by means of free and fair elections. Because not all transitions take place in the same year, the number of country-years varies by country. Our dataset ends in 2010 because we have countries that democratized in 2005 (Burundi, Kyrgyzstan, and Lebanon) and our rule for inclusion dictates that we consider a country to be democratic if five years after the first free and fair election it continues to elect government authorities by democratic means. Because the first transition took place in Greece in 1974, our dataset begins in 1969, that is, five years prior to the Greek transition.

Merging homicide data

Homicide data are difficult to compile because not all countries have the ability to record and code homicides and because definitions of homicides have not been standardized. For example, some countries compute intentional and unintentional homicides together.[19] Despite these divergences in practices, homicide data are widely used as a proxy for criminal violence, but authors have adopted different modeling techniques to address disparities in the quality of homicide data across countries (see Neumayer, 2003).

Rather than focus on one single information source, we compiled information from the most reliable and widely accepted sources. This allows us to ensure completeness and reliability to the best extent possible. To that end, we used information on homicide rates from four sources: the United Nations Office on Drugs and Crime (UNODC), the World Health Organization (WHO), the World Bank (WB), and the Homicide Monitor (HM). All four organizations compile homicide data from a variety of sources and provide information on homicide rates, which are defined in all cases as homicides per 100,000 population.

The WHO compiles data on homicides from reports provided by national state authorities. National justice and healthcare systems report information from death

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[19] For specific information on homicide data compilation and quality, see http://www.genevadeclaration.org/fileadmin/docs/GBAV2/GBAV2011_CH2.pdf.

certificates. Countries report this information using a specific nomenclature, the International Classification of Diseases (ICD). This nomenclature is revised periodically, and countries are expected to report based on the new revisions (the current ICD revision is 10). The database contains information dating back to 1950, but the time series has significant information gaps for several countries.

The UNODC compiles information on homicides in its Global Study on Homicide.[20] Currently, the available data comprise the 2000–14 period. The UNODC resorts to different sources: the WHO, and also local sources (national police departments, health authorities, and nongovernmental organizations [NGOs]).

The WB reproduces the same information that the UNODC collects and adds information from other sources. Currently, the bank's time series contains information from 1995 to the present.

Finally, one of the most recent sources of information on homicides is the HM, an online data visualization tool developed by the Igarape Institute, a think-tank in Brazil, that reproduces information from the UNODC and collects information directly from primary sources from countries in Latin America and the Caribbean (the organization collects data at the national and subnational level). Their time series comprises the period 2000–12.[21]

Given that there is a fair amount of information sharing across these different institutions, the homicide rate reported by the four sources is highly correlated (0.96), with no significant outliers. In particular, the comparison between UNODC data and HM data does not show any major differences in mean, standard deviations, minima or maxima.

Because these four information sources have shared standards of data gathering and the quality of the datasets is comparable, we decided to average the information provided from the four sources for each country-year. By averaging four sources of relatively good information quality, our goal was to increase our coverage as much as possible. In cases when only one of the four sources provided information for a specific country, we used that information for a given country-year. This procedure yielded 988 observations at the country-year level of analysis for our sample of 76 countries, for the period 1969–2010 (see Table A.IV).

Although the use of multiple information sources reduces information gaps, missing data remain a serious problem. As is the case for all international datasets, countries from some world regions provide accurate information while others report limited data or no data at all. Homicide statistics are particularly limited for several countries in Africa and Asia. As a result, we lose nearly one-half of our country-year observations (N x T = 1,848) due to information shortages (see Table A.IV). Because homicide rates are the metric of our dependent variable, we decided to accept the information gaps rather than engage in an exercise of multiple imputation.

Tables A.I and A.II above present information about geographic patterns in the missing data by world region. Table A.I identifies with italics the countries that dropped out from the analysis entirely due to missing homicide information. As the information shows, most of the dropped cases came from Africa plus a few from Asia, but not a single case dropped from the sample of Europe and Latin America (except for Grenada). Because Europe and Latin America provided more country-year cases of democratization (59.79%, see column 4 in Table A.II) and because homicide statistics were more complete in countries from these regions than in other parts of the world, Europe and Latin America account for 84.79% of the country-year cases in the analysis (see column 5 in Table A.II). As we discussed in the article, several countries in the Latin American sample share with other world regions, including Africa, important features that are typical of developing countries. Even with incomplete information, our global sample reflects realities of the types of cases that we seek to explain: namely, new democracies 1974–2005.

Indices

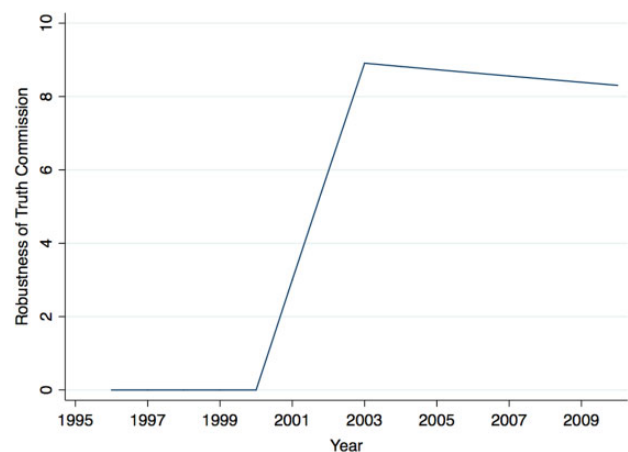Table A.III outlines the six dimensions we use to classify truth commissions – number of years in

Figure A.1. *Truth commission*$_{cumulative}$ in Peru, 1995–2010

[20] See http://www.unodc.org/gsh/en/data.html.
[21] See http://homicide.igarape.org.br/.

operation, budget and personnel, whether the mandate included an investigation on crimes against humanity and the number of crimes under investigation, whether the commission took testimonies of victims and perpetrators, the level of publicity of the report, and whether the commission made recommendations for legal changes, reparations, and judicial prosecution. We identify three levels of strength: weak (1), medium (2), and robust (3).

Interpretation of cumulative variables (truth commission)

To further explain the construction of our cumulative variables, we use the case of *Truth commission$_{cumulative}$*. Recall that in our statistical analysis we test for the cumulative effect of transitional justice by adding the yearly scores for each mechanism over time with a 1% annual depreciation rate to capture the declining impact that these mechanisms may have over time.

We use the case of Peru for illustration. As Figure A.I shows, prior to the implementation of the Peruvian Truth Commission in the years 2001–03, *Truth commission$_{cumulative}$* takes a value of zero. Because Peru implemented a Truth Commission that operated for three years and that we classify as robust (level 3) following the classification in Table IV, for y1 we assigned a value of 3; for y2 a value of 5.97 (3 plus 3 minus 1% depreciation); and for y3 a value of 8.91 (5.97 plus 3 minus 1% depreciation). After 2003, when the commission was no longer in session and the work had been formally completed, we assume it has reached its highest score and from that point on we no longer add any value but simply depreciate the highest value (8.91 for Peru) by 1% annually.

Additional statistical information and results

Table. A.IV. Descriptive statistics

Variable	Observations	Mean	Std. dev.	Min.	Max.
Homicide Rate	988	9.933	14.143	0	139.132
Trials	1,848	0.071	0.257	0	1
Trials$_{cumulative}$	1,848	0.878	1.610	0	11.516
Truth commission	1,848	0.139	0.572	1	3
Truth com.$_{cumulative}$	1,848	1.230	3.582	0	36.744
Amnesties	1,848	0.290	0.454	0	1
Amnesties$_{cumulative}$	1,848	3.562	6.895	0	37
Ln GDP per capita	1,727	7.665	1.235	4.968	10.352
GDP growth	1,694	2.055	4.769	−40.747	30.342
Income inequality	1,402	39.387	9.865	16.493	69.345
% Young males	1,828	9.213	1.291	5.175	12.207
Regime rupture	1,848	0.010	0.098	0	1
State repression	1,747	2.433	1.032	1	5
Years of peace	1,716	2.092	4.517	0	30

Table A.V. The impact of transitional justice mechanisms on the homicide rate in Latin America, 1974–2010 (GLS fixed effects models; full model with controls)

	Model 5 (Immediate)	Model 6 (Cumulative)	Model 7 (Imm. + Cum.)	Model 8 (Interaction)
Transitional justice mechanisms				
Trials	0.302		0.163	0.118
	(0.315)		(0.290)	(0.311)
Trials$_{cumulative}$		0.00443	0.0235	0.379†
		(0.125)	(0.120)	(0.187)
Truth commission	−0.664*		−0.421*	−0.630*
	(0.230)		(0.170)	(0.219)
Truth com.$_{cumulative}$		−0.275*	−0.253*	0.161
		(0.123)	(0.108)	(0.145)

(continued)

Table A.V. (continued)

	Model 5 (Immediate)	Model 6 (Cumulative)	Model 7 (Imm. + Cum.)	Model 8 (Interaction)
Amnesties	1.803* (0.660)		1.856* (0.740)	1.437* (0.600)
Amnesties$_{cumulative}$		0.0646 (0.112)	0.0284 (0.0964)	0.0155 (0.0769)
Trials$_{cumulative}$ × Truth com.$_{cumulative}$				−0.0962** (0.0327)
Controls				
Ln GDP per capita	−0.945 (1.896)	−1.396 (2.315)	−1.046 (2.104)	−0.367 (1.869)
GDP growth	−0.134* (0.0495)	−0.113* (0.0460)	−0.133* (0.0476)	−0.130* (0.0445)
Income inequality	−0.483† (0.256)	−0.485† (0.259)	−0.532† (0.268)	−0.569* (0.268)
% Young males	0.770 (0.625)	0.786 (0.663)	0.952 (0.692)	1.308† (0.711)
Regime rupture	0.0436 (0.709)	−1.305* (0.512)	−0.512 (0.500)	0.136 (0.631)
State repression	0.193 (0.181)	−0.238 (0.302)	−0.0363 (0.274)	0.115 (0.261)
Years of peace	0.115* (0.0452)	0.109 (0.0851)	0.122 (0.0804)	0.111 (0.0648)
Homicide rate (lagged)	0.761** (0.0108)	0.763** (0.0121)	0.762** (0.0121)	0.761** (0.0121)
Constant	24.46 (20.35)	30.27 (24.65)	26.52 (23.08)	18.82 (21.07)
Observations	277	277	277	277

$^{†}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; standard errors clustered by country in parentheses.

Table A.VI. The impact of truth commissions and amnesties on the homicide rate in new democracies, 1974–2005 (GLS fixed effects model)

Transitional justice mechanisms	Model 1
Trials	−0.080 (0.162)
Trials$_{cumulative}$	0.286 (0.192)
Truth commission	0.016 (0.189)
Truth com.$_{cumulative}$	−0.112* (0.044)
Amnesties	1.477* (0.593)
Amnesties$_{cumulative}$	0.034 (0.031)
Truth com.$_{cumulative}$ × Amnesties$_{cumulative}$	−0.004 (0.005)
Controls	YES
Constant	20.14** (8.113)
Observations	750

$^{†}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$.; standard errors clustered by country in parentheses.

Table A.VII. The impact of trials and amnesties on the homicide rate in new democracies, 1974–2005 (GLS fixed effects model)

Transitional justice mechanisms	Model 1
Trials	−0.036 (0.115)
Trials$_{cumulative}$	0.174 (0.276)
Truth commission	0.090 (0.26)
Truth com.$_{cumulative}$	−0.138** (0.045)
Amnesties	1.641** (0.564)
Amnesties$_{cumulative}$	0.019 (0.026)
Trials$_{cumulative}$ × Amnesties$_{cumulative}$	0.004 (0.011)
Controls	YES
Constant	19.46* (8.347)
Observations	750

$^{†}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; standard errors clustered by country in parentheses.

Table A.VIII. The impact of transitional justice mechanisms on the homicide rate in new democracies, 1974–2005 (GLS fixed effects models; subsample defined using coarsened exact matching; Treatment = *Truth commision$_{cumulative}$*)

	Model 9 (Immediate)	Model 10 (Cumulative)	Model 11 (Imm. + Cum.)	Model 12 (Interaction)	Model 13 (Interaction)
Transitional justice mechanisms					
Trials	0.623*		0.622*	0.602*	0.629*
	(0.258)		(0.262)	(0.263)	(0.253)
Trials$_{cumulative}$		0.00365	−0.0382	0.0209	0.0956
		(0.0874)	(0.0895)	(0.107)	(0.104)
Truth commission	−0.520**		−0.415**	−0.412**	−0.316†
	(0.141)		(0.146)	(0.146)	(0.164)
Truth com.$_{cumulative}$		−0.169**	−0.153**	−0.106	−0.0195
		(0.0434)	(0.0443)	(0.0642)	(0.0623)
Amnesties	1.257**		1.393**	1.332**	0.738†
	(0.439)		(0.441)	(0.445)	(0.432)
Amnesties$_{cumulative}$		−0.0217	−0.0254	−0.0211	0.0371
		(0.0376)	(0.0372)	(0.0374)	(0.0356)
Trials$_{cumulative}$ × Truth com.$_{cumulative}$				−0.0138	−0.027†
				(0.0137)	(0.0139)
Controls					
Ln GDP per capita	−1.041	−0.927	−1.519	−1.572	−1.684†
	(0.953)	(0.975)	(0.968)	(0.969)	(0.893)
GDP growth	−0.0727**	−0.0703**	−0.0812**	−0.0787**	
	(0.0207)	(0.0206)	(0.0206)	(0.0208)	
Income inequality	−0.00391	−0.0354	−0.0273	−0.0301	0.0363
	(0.0434)	(0.0459)	(0.0453)	(0.0453)	(0.0448)
% Youth males	0.159	0.0689	0.235	0.267	
	(0.211)	(0.211)	(0.211)	(0.214)	
Regime rupture	−1.170	−1.922*	−1.437†	−1.395†	−2.279**
	(0.822)	(0.832)	(0.843)	(0.844)	(0.691)
State repression	0.128	−0.0680	0.0837	0.0952	0.0590
	(0.154)	(0.152)	(0.156)	(0.156)	(0.146)
Years of peace	0.0758†	0.0679†	0.0935*	0.0930*	0.148**
	(0.0387)	(0.0395)	(0.0397)	(0.0397)	(0.0403)
Homicide rate (lagged)	0.815**	0.795**	0.789**	0.790**	0.756**
	(0.0315)	(0.0329)	(0.0323)	(0.0323)	(0.0274)
Constant	10.07	11.52	13.88†	14.03†	14.88*
	(8.115)	(8.334)	(8.212)	(8.213)	(7.417)
Observations	579	579	579	579	662

Full model with controls. $^{†}p < 0.1$; $^{*}p < 0.05$; $^{**}p < 0.01$; standard errors clustered by country in parentheses.
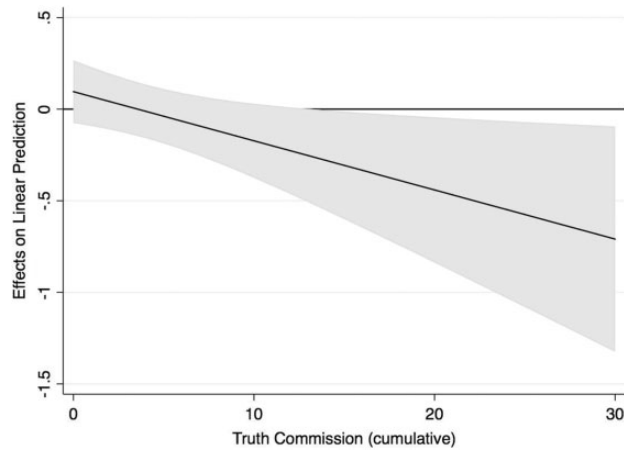
Figure A.2. Average marginal effect of trials on the homicide rate at different levels of *Truth commission*$_{cumulative}$ using the *Matched World* sample; Treatment = *Truth commission*$_{cumulative}$