The Perils of Top-down State Building: Evidence from Colombia's False Positives*

Daron Acemoglu† Leopoldo Fergusson‡
James Robinson§ Dario Romero¶ Juan F. Vargas‖

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Abstract

How should a state which lacks the monopoly of violence go about acquiring it? We investigate the use of high-powered incentives for members of the Colombian army as part of a strategy to combat left-wing guerillas and build the state's monopoly of violence. We show that this top-down state-building effort produced several perverse side effects. Innocent civilians were killed and misrepresented as guerillas (a phenomenon known in Colombia as 'false positives'). Exploiting the fact that Colombian colonels have stronger career concerns and should be more responsive to such incentives, we show that there were significantly more false positives during the period of high-powered incentives in municipalities where a higher share of brigades were commanded by colonels and in those where checks coming from civilian judicial institutions were weaker. We further find that in municipalities with a higher share of colonels, the period of high-powered incentives coincided with a worsening of local judicial institutions and the security situation, with more frequent attacks not just by the guerillas but also by paramilitaries.

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†Massachusetts Institute of Technology, Department of Economics, 77 Massachusetts Avenue, Building E18, Room 269D, Cambridge MA 02142-1347, Email: daron@mit.edu

‡Universidad de los Andes, Facultad de Economía, Cra 1 No. 18A - 12 Bogotá, Colombia. E-mail: lfergusson@uniandes.edu.co

§University of Chicago, Harris School of Public Policy, 1155 E. 60th St., Chicago, IL 60637, Suite 176, Email: jamesrobinson@uchicago.edu

¶IADB, Research Department, 1300 New York Avenue NW, Washington DC 20005, E-mail: `darior@iadb.org`

‖Universidad del Rosario, Department of Economics, Calle 12C No. 4-69, Oficina 315, Bogotá, Colombia, Email: juan.vargas@urosario.edu.co

1 Introduction

The absence of state capacity — the ability of the state to control violence, enforce laws, tax and regulate economic activity, and provide public services — is one of the major problems in many countries today. It is arguably at the root of civil wars (Fearon and Laitin, 2003), and a key difference between economically successful and unsuccessful countries (see, among others, Evans, 1995, Evans and Rauch, 1999, Herbst, 2000, Besley and Persson, 2011, Acemoglu and Robinson, 2012, Acemoglu, García-Jimeno and Robinson, 2015). Many hypotheses have been advanced to explain weak state capacity and lack of efforts towards state building in many nations. These include historical path dependence (Evans, 1995), ecology and population density (Herbst, 2000), and various political economy factors (Besley and Persson, 2011, Acemoglu and Robinson, 2012).

The most influential view on state building, often associated with Huntington (1968), starts with the Weberian conception that the most important attribute of a state is the legitimate monopoly of violence over its territory. Hence such a monopoly needs to be imposed on society before other aspects of state capacity can be developed. This approach, sometimes referred to as the "state first" or "security first" view, is naturally top-down (imposed on society without its consent) and is historically illustrated using the state-building projects of powerful leaders such as Peter the Great, Louis XIV, Kemal Ataturk or Park Chung-Hee (e.g., Huntington and Nelson, 1976, Tilly, 1990, Fukuyama, 2001, 2014). This view reaches much farther than academic circles, and has become the guiding principle for US interventions in Afghanistan and Iraq in recent years. The World Bank (2012, p. 25), for example, states "There is now an emerging consensus that unless a minimum level of security is established across the territory, interventions in other domains may be ineffective or even counterproductive."[1]

State capacity, however, is multidimensional, including not just military control but also fiscal, bureaucratic, administrative and legal capacity. Top-down approaches focusing on military control are typically unidimensional, prioritizing military objectives ahead of all others. Consequently, though attractive to some powerful leaders, the top-down approaches often fail to develop other aspects of state capacity. In fact, we will argue that they may lead to deteriorations in these capacities, particularly when military control is sought by increasing the power of the incentives of the state's agents without an effort to build the capacities of the state more broadly, and when the weakness of these other aspects makes it difficult to keep these agents accountable.[2] Though such a view has a natural affinity to the multi-tasking model of Holmström and Milgrom's (1991), it has

[1] Though this literature recognizes that many aspects of state capacity need to be built (Ghani and Lockhart, 2009), it typically ends up endorsing the security first view (e.g., OECD, 2010, Grävingholt, Leininger and von Haldenwang, 2012).

[2] This is not to deny that in extreme cases such as following the collapse of the communist Najibullah-regime in 1992 in Afghanistan, or the fall of Siad Barre in Somalia in 1991, there is such chaos that some modicum of security and stability has to be prioritized, whatever its costs. Nevertheless, even in such an apparently intractable case as Iraq, Berman, Shapiro and Felter (2011) show that policies complementary to those emphasizing security are not just feasible but effective, even if such policies have not typically been implemented.

not been proposed or articulated in the context of state building.

In this paper, we investigate the consequences of the efforts to establish the state's monopoly of violence in Colombia following the election of Álvaro Uribe as president in 2002. The Colombian state has been not only weak fiscally, administratively and legally, but has also been unable to establish anything approaching a monopoly of violence, with as much as one third of the country being under the control of guerillas and paramilitaries at the end of the 1990s (see Acemoglu, Robinson and Santos, 2013). President Uribe formulated a classic top-down state building project, focusing on combating non-state armed actors, particularly the left-wing guerillas. This project comprised of two main pillars: expanding the size of the military, and increasing their incentives to fight the guerillas.

A major consequence of these higher-powered incentives was a surge in 'false positives'[3] — the murder of civilians falsely portrayed by the army to be guerilla members (Figure 1).[4] False positives had long existed in Colombia, but increased massively following President Uribe's state building project, and started declining only after media revelations of the extent of civilian killings in 2008. During this period as many as 5,000 innocent civilians may have been executed (according to the UN High Commissioner for Human Rights). Figure 2 shows the distribution of false positives in the territory, revealing that the practice was widespread throughout the country, and not just driven by a few military units.

To clarify the relationship between the structure of incentives, other dimensions of state capacity and the unintended consequences of top-down unidimensional state-building efforts, we start with a simple extension of Holmström and Milgrom's (1991) multi-tasking framework. In our model, agents (army members) can exert good effort, which produces 'true positives' and helps establish the state's monopoly of violence, and bad effort, which produces false positives. The extent to which false positives can be portrayed as true positives is determined by the weakness of local judicial institutions. We establish a number of comparative statics which guide our empirical work. First, more powerful incentives for military personnel to kill guerillas will not only achieve this objective, but also increase false positives. Second, this effect will be more pronounced for brigades led by colonels because they have more powerful career concerns (the promotion from colonel to general is a difficult step in the Colombian army). Third, the effect will also be more pronounced in municipalities where local judicial institutions are weak, and less able to investigate and hold accountable military units and their commanders. Crucially, judicial institutions will impact false positives but not necessarily true positives. We show that this asymmetry is present in our data, which bolsters our interpretation that what we are documenting is not just unavoidable

[3]The phenomenon is more technically known as 'homicides of protected persons', and is also sometimes referred to as extrajudicial executions. The euphemism 'false-positives' was introduced by the political magazine *Cambio* in September 2007.

[4]The figure shows both incidents producing false positives and the number of people killed in these events. As in our analysis below, we use bianual information, and in this and the next figure, we plot a three-period moving average of the raw numbers for ease of inspection.

collateral damage from a successful state-building project, but systematic bad effort by military units directed towards killing civilians and portraying them as guerilla combatants. Finally, with limited risk aversion and noise in performance measures, the state's agents obtain sufficiently high returns from exerting bad effort that their overall utility is higher when the quality of judicial institutions is lower; and under such circumstances, if they are sufficiently powerful, they may take actions to further weaken the local judiciary.

To better understand the implications of this top-down state-building project and investigate the aforementioned theoretical predictions, we build a municipality-level panel dataset on the incidence of false positives, the rank of brigade commanders, and the quality of local judicial institutions from 2000 to 2010. We identify the introduction of pay for performance with Uribe's flagship "Democratic Security" initiative and associated policies and directives aimed at rewarding army members for killing guerilla combatants, which were in effect between 2003 and 2008. Our empirical strategy is to investigate the impact of the interaction between the share of brigades commanded by colonels and the quality of (initial) local judicial institutions with higher-powered incentives for the military on true and false positives.

The results are consistent with the implications outlined above. In the time series, we see a pronounced increase in false positives during the period of higher-powered incentives (Figure 1). True positives, in contrast, start increasing sharply several years before the onset of high-powered incentives, and then decline during this period, in part because of the collapse of the peace process initiated by Uribe's predecessor, President Pastrana, and in part because the guerilla withdrew to remoter areas during this period (Figure 3). The contrast between the time-series behaviors of true and false positives already suggests that the increase in false positives is not just a consequence of collateral damage. We then show that this increase is more pronounced in municipalities where the share of brigades led by colonels is greater and local judicial institutions are weaker. Though these estimates do not correspond to causal effects and we cannot rule out alternative, time-varying factors accounting for these patterns, reassuringly we see no pre-trends in either false positives or true positives in these areas, suggesting that these municipalities were not on different trends before the era of high-powered incentives. Confirming this, the results are also very similar when municipality-specific linear trends are included in the regressions. We further find that after 2008, these differential trends disappear, which is consistent with greater public scrutiny bringing the incentives to generate false positives to an abrupt end (a pattern that is also visible in the time series shown in Figure 1). Finally, the empirical evidence also points to a deterioration in the quality of judicial institutions in areas with a high share of brigades commanded by colonels, and perhaps more paradoxically, a worsening of the security situation in such areas (with increases both in guerilla and paramilitary attacks on civilians). Perhaps most paradoxical is the pattern that these areas do not even show increased attacks on guerillas by government forces, raising the intriguing possibility that military units may have taken advantage of higher-powered incentives

without even confronting the guerillas more frequently.

Our bottom-line conclusion that high-powered incentives without a strong accountability system can backfire coheres with a large principal-agent literature. Holmström and Milgrom (1991) and Baker (1992) were the first to emphasize and model the dark side of high-powered (pay-for-performance) incentives, particularly when the agent can game these incentives. There are indeed many examples of significant distortions from multi-tasking type considerations in the organizational economics literature. Summarizing this evidence, Prendergast concludes: "One of the first rules of pay for performance is that you never offer pay for performance in circumstances where a person both diagnoses and cures the problem" (2011, p. 127). Unfortunately, as we will see, this is more or less exactly what happened in the Colombian case. Unintended consequences of pay for performance have been noted in several distinct settings (e.g., Baker, Gibbons and Murphy, 1994, Oyer, 1998, Miller and Babiarz, 2014).[5] One well-publicized case is education, where widespread teacher gaming and cheating surfaced following the higher-powered teacher incentives introduced by George Bush's *No Child Left Behind* policy (Levitt and Jacob, 2003, of Aviv, 2014). Relative to this literature, not only do we provide evidence for multi-tasking type behavior in a novel and arguably more consequential setting, but we also suggest and provide evidence for the effect of institutional restrictions on this type of behavior.

Though a number of recent papers (e.g., Duflo, Hanna, and Ryan, 2012, Finan, Olken and Pande, 2015, and Dal Bó, Finan and Rossi 2013) find that strengthening incentives for public servants is generally effective in developing countries, this is often in a setting where other accountability or monitoring mechanisms are not entirely absent, as they were for security forces in Colombia. When they were largely absent as in Banerjee, Duflo and Glennerster's (2008) study of nurses in the Indian public health care system, such incentive schemes were ineffective and did backfire. Dixit (1997) explicitly argued that these potential costs of high-powered incentives in an environment of low accountability are the reason why bureaucracies do not provide such incentives (see also Acemoglu, Kremer and Mian, 2008). It is thus not surprising that the implications of high-powered incentives in the context of top-down state building parallel their failed applications in other fields.[6]

Our finding that false positives are associated with expanding activity by non-state armed actors is also related to the literature showing that many counter-insurgency activities such as bombing have counter-productive effects (e.g. Kalyvas, Kocher and Pepinsky, 2011, Lyall, 2014, Dell and Querubín, 2016).

[5]Following Sears' pay-for-performance program for its auto mechanics, owners of intact cars were misled by mechanics into authorizing unnecessary repairs. In 1992, Sears abolished the commission plan in its auto-repair shops. The Sears example also reveals that the extent to which dysfunctional behavior takes place depends on the institutional environment: Sears changed its practices in the face of law suits from enraged customers.

[6]Highlighting another potential downside of building the military in weak institutional environments, Galiani, Rossi and Schargrodsky (2011) find that people drafted at random into the Argentine army are subsequently more likely to become criminals.

As we have already noted, the literature on state-building in political science and sociology emphasizes structural features of societies which create incentives or disincentives to develop state capacity (e.g., Mann, 1986, 1993, Spruyt, 1994, Herbst, 2000, Soifer, 2015). A theme common to most of this work is that state-building is driven by inter-state warfare (Hintze, 1975, Brewer, 1990, Tilly, 1990) or civil war (Slater, 2010), an emphasis which helps to cement the idea that "security first" is the only option to build a state. Some scholars even emphasize that violence is an unavoidable aspect of state-building (see Giustozzi, 2011). A small literature (e.g., Rudolph and Rudolph, 1979, and Berman, 2016) has argued that the focus on the militarized creation of idealized 'Weberian' states does not accurately capture the historical processes of state building, but neither these authors nor any other work that we are aware of note the unintended consequences and potential negative effects of top-down state building on other aspects of state capacity. In this context, our results emphasize not only the cost of this top-down state building project in terms of false positives, but also suggest that it may have failed to increase state capacity because it weakened other aspects of this capacity in the process and perhaps also further undermined the legitimacy of the state and its consensual strength, which may be central to its capacity (Acemoglu, 2005, for theoretical argument, and Isacson, 2012, on the Colombian case).

To our knowledge there has been no empirical study of the false positives in Colombia, though Cárdenas and Villa (2013) develop a principal-agent model where the government, acting as principal, offers bonuses, a probability of auditing and a sanction for cheating to military units in exchange for their reported killings. They interpret President Uribe's flagship Democratic Security policy as one which privileged bonuses and disregarded sanctions or auditing, thus increasing cheating (false positives) by military units. While this interpretation is clearly in line with ours, their paper neither develops the basic comparative static predictions that guide our empirical work, nor presents any empirical evidence, nor links this episode to the general problems of top-down state building projects.

We start in the next section with a brief discussion of the Colombian context and the top-down state building project initiated by President Uribe. Section 3 presents our motivating model. Section 4 describes our data. Section 5 presents our empirical strategy and results, and Section 6 concludes. The Appendix contains further case study evidence on the presence of false positives in Colombia and the nature of incentives facing military personnel, proofs of additional results from the theory section, and further empirical results.

2 The Colombian Context and the Top-Down State Building Project

Colombia has a long history of civil war and non-state armed groups. The conflict with the two largest guerilla groups, the *Fuerzas Armadas Revolucionarias de Colombia* (Revolutionary Armed

Forces of Colombia - FARC) and *Ejército de Liberación Nacional* (National Liberation Arny - ELN) dominated the 2002 presidential electoral campaign won by Álvaro Uribe. Voters were particularly disillusioned with previous failed peace processes. President Uribe's flagship policy, *Política de Seguridad Democrática* or Democratic Security Policy, included a major run-up in military expenditure to fight the guerillas, and also simultaneously sought to control paramilitary groups united under an umbrella organization (called *Autodefensas Unidas de Colombia*, United Self-Defense Forces of Colombia - AUC). The AUC demobilized between 2003 and 2007, following a peace process with the government (though splinter paramilitary groups including former AUC fronts are still active in the country).

The Democratic Security program was accompanied by incentives to increase effort to fight the illegal armed groups. We first offer a summary of the introduction and removal of the set of incentives which, we argue, helped exacerbate the problem of false positives. We then discuss the evidence for false positives.

2.1 High-powered incentives in the Colombian military

Uribe's Democratic Security program coincided with the issue of a specific set of documents and informal regulations introducing incentives in the fight against illegal armed groups. Some of these are now public. The secret *Army Directive 29* of November 17 2005, later leaked by the press and exhibited in Figure 4, is particularly relevant. The directive put in place a reward schedule for killing and capturing members of illegal groups, seizing weapons, and sharing information with the following important features. First, military personnel was not explicitly excluded from the possibility of getting the rewards. Second, while there was a fixed amount to be distributed as rewards for killing or capturing guerilla leaders there was no limit in the available pool for lower ranked guerillas.[7] Third, the operation that led to the reward did not need to be authorized ex ante by a superior officer. And finally, posterior intelligence could be used to justify the killings. In sum, strong incentives were introduced, but there were only weak controls on the implementation of the directive.

Another case in point is the Presidential Decree 1400 of May 5 2006 (exhibited in Figure 5), called BOINA (the Spanish acronym for Bonuses for Operations of National Importance, and literally meaning 'beret'). This decree was explicitly targeted at members of the armed forces and the now-extinct Departamento Administrativo de Seguridad (DAS) — Colombia's former intelligence agency.[8] The decree, revoked in May 14, 2007, rewarded army members or DAS functionaries

[7] For instance, the directive approved only up to fifteen total rewards for illegal armed groups' top leadership. For the lower-ranked commanders and foot soldiers, while the payment per member was lower, there were no limits on the number of monetary rewards that could be awarded.

[8] The agency was closed in the midst of a number of scandals during Uribe's presidency, involving illegal wiretapping of members of the opposition, selling classified information to members or armed groups, drug traffickers and foreign governments, elimination of judicial antecedents of paramilitaries, and even an assault on a senator (see Verdad

with up to 12 times their monthly salary for participating in successful operations of "national importance" against the insurgency. These bonuses, reserved for very high-ranking individuals and signed off by the President, also fit into the general policy of providing high-powered incentives in the fight against the guerilla.

As we document further in the Appendix, while the formal directives were in effect starting at least by 2005, informal incentives were ratcheted up soon after Uribe came to power (in August 2002). These incentives were partly in terms of money or vacations, and partly, in the form of promotions and careers. The report by Human Rights Watch (2015) describes the introduction of incentives after 2002 which "rewarded combat killings with vacation time, promotions, medals, training courses, and congratulations from superiors, among other prizes" (p. 29). They quote as well a leaked 2009 memo from the US Embassy in Bogotá, which suggested that General Mario Montoya "initiated the practice" (p. 68) of false positives when he commanded the 4th brigade in 2002 and 2003. Montoya became the head of the army between February 2006 and November 2008. Indeed, Human Rights Watch begins its report by stating

> "Between 2002 and 2008, army brigades across Colombia regularly executed civilians. Under pressure from superiors to show 'positive' results and boost body counts in their war against guerillas, soldiers and officers abducted victims or lured them to remote locations under false pretenses ... killed them, placed weapons on their lifeless bodies, and then reported them as enemy combatants killed in action." (p. 1).

UN Commissioner Alston also observes that the pressure to "show results" and rewards for doing so is cited by experts, even within the military, as one of the causes of false positives. A soldier explained a killing by his unit would be rewarded with 15 days of vacation. "When important holidays approached, he stated, soldiers would attempt to 'earn' vacation time" (Alston, 2010, p. 11). Another soldier, who witnessed as many as 25 false positive cases occurring in 2007 and 2008, refers to Directive 029 of 2005, and notes that to claim the monetary rewards it promised for killings and war material, army members would kill civilians and "plant" weapons on them (many of which had been seized in operations and kept unregistered for later use in these operations, or bought illegally). According to his testimony, pictures were then taken and annexed to reward certificates that were paid for with confidential expenses. Moreover, he mentions the case of one involved officer (Sergeant Consuegra) who was rewarded with a trip to the United States to take a course on Human Rights, and later returned to continue with these operations.[9] (We present more specific examples of this in the Appendix). Alston further notes

Abierta, 2011).

[9] "Me dijeron que están ofreciendo $50 millones por mí", *El Espectador*, April 9, 2016. Available at: `http://www.elespectador.com/entrevista-de-cecilia-orozco/me-dijeron-estan-ofreciendo-50-millones-mi-articulo-626269` (last accessed May 12, 2016).

> "There were incentives: an informal incentive system for soldiers to kill, and a formal one for civilians who provided information leading to the capture or killing of guerillas. The ... system lacked oversight and accountability" (p. 2).

These provisions and other directives creating high-powered incentives for military personnel were ended after 2008 due to mass media attention following the adbuction and murder by the army of 22 men in Soacha, a suburb of Bogotá. For example, the aforementioned directive 29 was modified by directives 02 of 2008 and 01 of 2009. After 2008, the government also took disciplinary action, ousting high-ranking officials involved in possible false positives. It also created a specialized unit in the Office of the Attorney General (*Fiscalía*) to investigate these crimes.

Based on all of this evidence, we suppose in our empirical work that high powered incentives were in effect from the beginning of 2003 until the end of 2008. In some of our specifications, we parameterize the power of these incentives as increasing gradually between 2003 and the end of 2007. This choice of timing is consistent with the emphasis in the case study literature and the time-series patterns of false positives already depicted in Figure 1, which also superimposes our parameterization of these incentives on top of the time-series variation.

2.2 Career concerns of Colombian colonels

Following President Uribe's election, the Colombian military experienced an unprecedented expansion, nearly tripling from about 160,000 soldiers in 2002 to about 430,000 at the end of the decade. This growth also implied the creation of new military brigades, while the rank composition of the military command could not change as rapidly (it takes time to become a high-ranking official). This phenomenon, which experts in Colombia called *escasez de cuadros* (cadre scarcity), forced the army to appoint colonels to command brigades, a position previously reserved for generals. Colonels leading brigades, unlike generals, were up for promotion and unlike lower ranked officers, were in charge of important military units whose results were tied to their personal success. Therefore, high-powered incentives are more likely to have an effect on their behavior. Though some generals were also influenced by high-powered incentives and may have been motivated, among other things, by a desire to be promoted to a higher-ranked generalship, the stakes for colonels were clearly much higher, mainly because promotion to the rank of general is generally viewed as a very difficult step in the Colombian army.

At the beginning of 2014, 4,262 members of the Armed Forces were under investigation for their responsibility in false positives cases. Almost 10% of them (401) were army officers, ranging from generals to lieutenants.[10] The majority of the officials that are now being investigated were colonels,

[10] "4.262 militares investigados por falsos positivos", *El Espectador*, February 19, 2014. Available at: `http://www.elespectador.com/noticias/judicial/4262-militares-investigados-falsos-positivos-articulo-476121` (last accessed September 20, 2014).

and in fact, many of the generals that are implicated were commanding brigades as colonels at the time of the events.

The case study evidence, which we discuss more in the Appendix, supports the notion that colonels had greater incentives to encourage and reward false positives. For instance, in his testimony, Captain Antonio Rozo Valbuena, former commander of the GAULA special operations unit working in the department of Córdoba, asked the judges to investigate a general who committed scores of false positives in the brigade under his command while he was a colonel. According to Captain Rozo Valbuena, the only objective of the official was to gather enough "statistics" to be able to be promoted to general.[11] While Colombian justice has been slow to prosecute involved officers, the case against General Escalante, the first with an arrest warrant, is also particularly telling. One of his soldiers claims that Torres Escalante, then a colonel commanding Brigade XVI, knew about false positives in his unit and explicitly emphasized killings over capture ("you talk to me about killings", he is quoted as saying). He also directly approved rewards with confidential funds for the killing of civilians.[12] With cases like this, the General Attorney Office sought to prove that "killings were not isolated murders by foot soldiers or low-rank individuals, but responded to a directive from the top ranks privileging deaths as operational results over captures or demobilizations."[13]

But perhaps the most telling case on the potentially different behavior of colonels and generals is that of Brigade XI. Colonel Borja confessed committing 57 false positives as commander of a special joint forces unit of this brigade, in turn commanded by Colonel Peña Forero. Upon arrival, in November of 2007, of General Jorge Arturo Salgado, a process to uncover these crimes was initiated. At the time, the brigade led the rankings based on brigade killings, allegedly promoted by army commander General Montoya. But the large numbers seemed suspect, and General Salgado set out to uncover the "criminal machine" of Colonel Borja and fired him after confirming irregularities in the reports of rebels killed in combat. In Borja's confession, he refers to Colonel Peña's concerns about the standing of his brigade relative to others by number of rebels killed, wishing to top the list. Borja also emphasized that those not making a killing quota were forced out or moved away. The investigation of the General Attorney concluded that between 2007 and 2008 in Brigade XI there were two styles of leadership, that of Colonel William Peña Forero and that of General Jorge Arturo Salgado. Colonel Peña set killing goals and advised his subordinates on how to claim rewards for false positives. General Salgado, instead, uncovered Colonel Peña's crime, and perhaps

[11] "Confesiones siniestras", *Agencia Prensa Rural*, October 10, 2011. Available at: `http://prensarural.org/spip/spip.php?article6588` (last accessed August 15, 2014).

[12] "Un testigo clave contra el general Torres Escalante, investigado por falsos positivos", *El Espectador*, March 30, 2016, Available at `http://www.elespectador.com/noticias/judicial/un-testigo-clave-contra-el-general-torres-escalante-inv-articulo-624660?` (last accessed May 12, 2016).

[13] "Se entregó el general Torres Escalante por 'falsos positivos"', *El Espectador*, March 28, 2016, Available at `http://www.elespectador.com/noticias/judicial/se-entrego-el-general-torres-escalante-falsos-positivos-articulo-624164` (last accessed May 12, 2016).

as a consequence, during the first year of his tenure, reported killings fell from 181 to 60.[14]

2.3 Evidence on false positives

It is important to underscore that the data we use on false positives do indeed correspond to killings of civilians. This issue is discussed in detail in the UN and Human Rights Watch reports, and was also extensively covered in the Colombian press. Figure 6, for example, depicts some of the evidence gathered by judicial authorities allowing them to establish that alleged guerilla killings were, in fact, false positives. The figure contains images published by Semana (the main Colombian political magazine) in 2010.[15] Corpses were, somewhat carelessly, set up simulating combats. The image in the top left reveals that the victim's fingers were artificially placed on the trigger, and subsequent forensic tests revealed that the weapon was not fired. The second picture, to the right, shows three grenades dangerously placed on a victim's pockets, where they could easily explode in the midst of combat. In the bottom left image, the victim is wearing the right boot on the left foot, and vice versa. Finally, in the bottom right image, the magazine is stored inside the boot, which would have been extremely uncomfortable during combat.

3 A model of intentional and unintentional false positives

In this section we present a simple theoretical framework which will guide our empirical work.

3.1 Setup and assumptions

Consider the following simple extension of Holmström and Milgrom's (1991) model of multi-tasking. We take the incentive scheme as given, and focus on the implications for the agent's behavior. The agent can exert good effort a_T, which produces true positives $\exp(q_T)$, where

$$q_T = a_T + \varepsilon_T, \tag{1}$$

and $\varepsilon_T \sim \mathcal{N}(0, \sigma_T^2)$. False positives can be produced intentionally or accidentally, and are given by $\exp(q_F)$, where

$$q_F = \chi(a_T + \varepsilon_T) + (a_F + \varepsilon_F), \tag{2}$$

$\chi > 0$, and $\varepsilon_F \sim \mathcal{N}(0, \sigma_F^2)$ and independent of ε_T.[16] The first term in equation (2) corresponds to

[14] "El general que frenó los falsos positivos en Sucre", *El Espectador*, April 16, 2016, Available at `http://www.elespectador.com/noticias/judicial/el-general-freno-los-falsos-positivos-sucre-articulo-627510` (last accessed May 12, 2016).

[15] Semana, "Los casos olvidados de los falsos positivos", July 17, 2010, `http://www.semana.com/nacion/articulo/los-casos-olvidados-falsos-positivos/119416-3`.

[16] Throughout, since observed true positives, $\exp(q_T)$, and false positives, $\exp(q_F)$, are respectively monotonic in q_T and q_F, with some abuse of terminology, we refer to either set of objects as true or false positives.

unintentional "collateral damage" that arises when, striving to produce true positives, the agent nonetheless generates false positives; it thus naturally scales with good effort, a_T. The second term incorporates bad effort a_F, intentionally producing false positives. For tractability, as with true positives, we assume that the performance measure, q_F, is a linear function of effort with additive normal noise. Notice that as χ tends to zero, all false positives come from bad effort, whereas for large values of χ, false positives largely reflect collateral damage.[17]

The observed performance measure for the agent is assumed to be $\hat{q}_T$, given by

$$\hat{q}_T = q_T + \alpha q_F,$$

where $\alpha \in [0, 1]$ captures the extent to which the agent may successfully misrepresent false positives, and corresponds to an inverse measure of the *quality of local judicial institutions.*[18]

The agent has constant absolute risk aversion preferences over his reward w net of effort costs $\Psi(a_T, a_F)$,

$$u(w - \Psi(a_T, a_F)) = \mathbb{E}[-\exp(-\eta\,(w - \Psi(a_T, a_F)))],$$

where η is the coefficient of absolute risk aversion, and $\Psi(a_T, a_F) = \frac{1}{2}(c_T a_T^2 + c_F a_F^2) + \delta a_T a_F$.[19] When $\delta = \Psi''_{a_T a_F}(a_T, a_F) > 0$, there is effort substitution: more bad effort increases the cost of good effort. Conversely, when $\delta < 0$, the two types of efforts are technological complements, and more effort in one dimension reduces the cost of effort in the other.

The reward to the agent is the sum of a flat component (e.g., base salary) τ and a linear incentive scheme as a function of the performance measure $\hat{q}_T$, so that

$$w = \tau + \pi s \hat{q}_T.$$

Here s corresponds to the power of the incentives facing the agent (as a function of the performance measure $\hat{q}_T$), while π parameterizes how much he cares about this aspect of his rewards, for example,

[17] Just as intentional effort directed at true positives produces false positives, one could allow effort directed at false positives to accidentally generate true positives (killings of real guerilla members) when trying to produce false positives. This does not change the essence of the results that follow. Moreover, it is not as relevant in our empirical application for at least two reasons. First, when killing civilians to present them as guerilla members the army typically targeted individuals known not to be guerilla members (petty criminals, the homeless, the mentally ill and others at the margin of society). Second, even if they killed a guerilla member or collaborator, the fact that they did it via "bad effort" (that is, killing him outside of combat and disguising him as killed in combat) is a false positive — both legally and from the viewpoint of corrupting the system by killing people in search of personal rewards.

[18] A slightly more general assumption would be to have

$$\hat{q}_T = q_T + \alpha_1\,[\chi(a_T + \varepsilon_T)] + \alpha_2(a_F + \varepsilon_F),$$

with α_1 corresponding to the misrepresentation of collateral damage and α_2 to the portrayal of intentional false positives as true killings. We adopt the simpler specification (with $\alpha_1 = \alpha_2 = \alpha$) since we do not have a way of distinguishing these more detailed parameters in the data.

[19] Here the reward w is inclusive of monetary rewards as well as non-pecuniary ones, such as promotion and days off. The assumption that this reward is a linear function of $\hat{q}_T$ is for simplicity and tractability.

capturing his career concerns resulting from good performance (as measured by $\hat{q}_T$).

Then, using the properties of the CARA utility, the agent's utility $u(a_T, a_F)$ is proportional to

$$\tau + \pi s \left(a_T \left(1 + \alpha\chi\right) + \alpha a_F\right) - \frac{1}{2}(c_T a_T^2 + c_F a_F^2) - \delta a_T a_F - \frac{\eta(\pi s)^2}{2}\left(\left(1+\alpha\chi\right)^2 \sigma_T^2 + \alpha^2 \sigma_F^2\right), \quad (3)$$

where the first two terms correspond to the expected rewards, the second two terms to the costs, and the last term to the variance multiplied by the coefficient of absolute risk aversion, η.

We first observe that in the extreme case with $\delta = \sqrt{c_T c_A}$, there is full substitution and the agent specializes in one task (since in this case $\Psi(a_T, a_F) = \frac{1}{2}(\sqrt{c_T}a_T + \sqrt{c_F}a_F)^2$). In the text, we assume that

$$|\delta| < \sqrt{c_T c_F}, \quad (A1)$$

which enables us to focus on the more interesting (and less extreme) cases. The Appendix (Section A.4) discusses the cases of perfect substitutes and perfect complements, establishing that the results are essentially identical to those presented here.

3.2 Solution and implications

The agent maximizes $u(a_T, a_F)$ in (3) by choosing good and bad effort, a_T and a_F. Bearing in mind the possibility of corner solutions, equilibrium effort levels are obtained as

$$\begin{aligned} a_F^* &= 0 \Leftrightarrow \delta \geq \frac{\alpha}{1+\alpha\chi} c_T \equiv \underline{\delta_F}, \\ a_T^* &= 0 \Leftrightarrow \delta \geq \frac{1+\alpha\chi}{\alpha} c_F \equiv \underline{\delta_T}, \end{aligned}$$

where $\underline{\delta_J}$ is the critical value of δ above which the agent exerts no effort of type J.

Because the marginal cost of effort is zero when both types of efforts are equal to zero, the agent will exert at least one kind of effort. To determine which, suppose that $\underline{\delta_T} < \underline{\delta_F}$, or equivalently $\frac{\alpha}{\sqrt{c_F}} > \frac{1+\alpha\chi}{\sqrt{c_T}}$. This implies that $\underline{\delta_T} < \sqrt{c_T c_F} < \underline{\delta_F}$. Then for $\delta \in \left(0, \underline{\delta_T}\right]$, the agent chooses $a_T^* > 0$ and $a_F^* > 0$, while if $\delta \in \left(\underline{\delta_T}, \sqrt{c_T c_F}\right)$, he opts for $a_T^* = 0$ and $a_F^* > 0$. The symmetric argument holds when $\underline{\delta_T} > \underline{\delta_F}$.

Intuitively, these conditions underscore that when δ is sufficiently large, the agent specializes in one kind of effort, and which one this is depends on the relative profitability of bad versus good effort (captured in the comparison $\alpha/\sqrt{c_F} \gtrless (1+\alpha\chi)/\sqrt{c_T}$). When δ is small (and trivially for negative δ), both types of effort are exerted.

Summarizing these possibilities, utility maximization yields the following effort levels:

$$a_F^* = \begin{cases} \pi s \frac{\alpha c_T - \delta(1+\alpha\chi)}{c_T c_F - \delta^2} & \text{if } \delta < \min\left\{\underline{\delta_F}, \underline{\delta_T}\right\} \\ \pi s \frac{\alpha}{c_F} & \text{if } \underline{\delta_T} < \delta < \underline{\delta_F} < \sqrt{c_T c_F}, \\ 0 & \text{if } \underline{\delta_F} < \delta < \sqrt{c_F c_T} < \underline{\delta_T} \end{cases} \tag{4}$$

$$a_T^* = \begin{cases} \pi s \frac{(1+\alpha\chi)c_F - \delta\alpha}{c_T c_F - \delta^2} & \text{if } \delta < \min\left\{\underline{\delta_F}, \underline{\delta_T}\right\} \\ \pi s \frac{1+\alpha\chi}{c_T} & \text{if } \underline{\delta_F} < \delta < \underline{\delta_T} < \sqrt{c_T c_F}, \\ 0 & \text{if } \underline{\delta_T} < \delta < \sqrt{c_F c_T} < \underline{\delta_F} \end{cases} \tag{5}$$

We focus on the implications of the model on these equilibrium efforts and, more importantly, on the quantities that we can measure; true positives given by $\mathbb{E}[\exp(q_T^*)]$, and false positives given by $\mathbb{E}[\exp(q_F^*)]$.[20] More specifically, these quantities can be computed as

$$\mathbb{E}[\exp(q_T^*) = \mathbb{E}[\exp(a_T^* + \varepsilon_T)] = \exp(a_T^*) \exp\left(\frac{\sigma_T^2}{2}\right), \tag{6}$$

and

$$\mathbb{E}[\exp(q_F^*)] = \mathbb{E}[\exp(a_F^* + \varepsilon_F) + \chi(a_T^* + \varepsilon_T)] = \exp(\chi a_T^* + a_F^*) \exp\left(\frac{\chi^2\sigma_T^2 + \sigma_F^2}{2}\right), \tag{7}$$

where the last equalities in both expressions make use of the fact that the error terms are normally distributed.

The next proposition uses these expressions to obtain the comparative statics of true and false positives.

Proposition 1. ***(Equilibrium false and true positives and incentives)***
A marginal increase in incentives s:

1. *weakly increases true and false positives, i.e.,*

$$\frac{\partial \mathbb{E}[\exp(q_T^*)]}{\partial s} \geq 0 \text{ and } \frac{\partial \mathbb{E}[\exp(q_T^*)]}{\partial s} = 0 \text{ if and only if } a_T^* = 0,$$
$$\frac{\partial \mathbb{E}[\exp(q_F^*)]}{\partial s} \geq 0 \text{ and } \frac{\partial \mathbb{E}[\exp(q_F^*)]}{\partial s} = 0 \text{ if and only if } a_F^* = 0 \text{ and } \chi = 0;$$

2. *leads to a weakly larger increase in true and false positives where reported output is a more*

[20] In the Appendix (Section A.5), we show that the comparative statics are identical if we focus on expected values of the performance measures, $\mathbb{E}[q_T^*]$ and $\mathbb{E}[q_F^*]$.

important part of compensation (higher π), i.e.,

$$\frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \pi} \geq 0 \text{ and } \frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \pi} = 0 \text{ if and only if } a_T^* = 0,$$
$$\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \pi} \geq 0 \text{ and } \frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \pi} = 0 \text{ if and only if } a_F^* = 0 \text{ and } \chi = 0;$$

3. *leads to a weakly larger increase in false positives where misrepresentation of false positives is more likely (higher α), i.e.,*

$$\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \alpha} \geq 0 \text{ with } \frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \alpha} = 0 \text{ if and only if } a_F^* = 0 \text{ and } \chi = 0;$$

4. *may lead to a larger or smaller increase in true positives where misrepresentation of false positives is more likely (higher α), i.e.,*

$$\frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial \alpha \partial s} \begin{cases} 0 & \text{if } a_T^* = 0 \\ \lesseqgtr 0 & \text{if } (a_T^*, a_F^*) > 0 \text{ and } \chi \lesseqgtr \frac{\delta}{c_F} \\ > 0 & \text{if } a_F^* = 0 \end{cases}.$$

Proof. All stated results follow from combining equilibrium effort (4) and (5) with (6) and (7). For the direct impact of s and its interaction with π, these are almost immediate by noticing that $\mathbb{E}[\exp(q_F^*)] > 0$ and that the derivatives, $\frac{\partial a_J^*}{\partial s}$, $\frac{\partial a_J^*}{\partial \pi}$, $\frac{\partial^2 a_J^*}{\partial s \partial \pi}$, for $J \in \{F, P\}$, are greater than or equal to zero, with equality when the corresponding effort equals zero.

Only the cross derivative with α requires some elaboration. For true positives, when no good effort or only good effort is exerted, these results are also immediate. Taking the case where both efforts are positive, we can compute $\frac{\partial a_T^*}{\partial s} = \frac{a_T^*}{s}$ and $\frac{\partial^2 a_T^*}{\partial s \partial \alpha} = \frac{1}{s}\frac{\partial a_T^*}{\partial \alpha}$. After substituting and simplifying, we can write:

$$\frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \alpha} = \mathbb{E}[\exp(q_T^*)]\frac{1}{s}\frac{\partial a_T^*}{\partial \alpha}(a_T^* + 1),$$

which leads to the stated condition.

For false positives, we have

$$\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \alpha} = \mathbb{E}[\exp(q_F^*)]\left[\left(\chi\frac{\partial a_T^*}{\partial s} + \frac{\partial a_F^*}{\partial s}\right)\left(\chi\frac{\partial a_T^*}{\partial \alpha} + \frac{\partial a_F^*}{\partial \alpha}\right) + \left(\chi\frac{\partial^2 a_T^*}{\partial s \partial \alpha} + \frac{\partial^2 a_F^*}{\partial s \partial \alpha}\right)\right].$$

If there is no collateral damage ($\chi = 0$) and no bad effort ($a_F^* = 0$), all derivatives and cross derivatives in the expression equal zero and thus $\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial \alpha \partial s} = 0$. If this is not the case, the term with the derivatives with respect to s is always positive because at least one type of effort is strictly positive. The remaining terms with the derivatives and cross-derivatives with respect to α are also trivially positive when just one effort is exerted or if $\delta \leq 0$. In the case of effort substitution, we can complete the square to obtain

$$\left(\chi\frac{\partial^2 a_T^*}{\partial s \partial \alpha} + \frac{\partial^2 a_F^*}{\partial s \partial \alpha}\right) = \frac{1}{s}\left(\chi\frac{\partial a_T^*}{\partial \alpha} + \frac{\partial a_F^*}{\partial \alpha}\right) = \frac{\pi}{c_F c_T - \delta^2}\left[(\chi\sqrt{c_F} - \sqrt{c_T})^2 + 2\chi\left(\sqrt{c_F c_T} - \delta\right)\right] > 0,$$

where we have made use of (A1), or $\delta < \sqrt{c_F c_T}$. □

The first prediction in Proposition 1 is that more high-powered incentives increase both true and false positives. The increase is strict with a few exceptions (which occur when the agent chooses to specialize in just one type of effort, and for false positives when in addition there is no collateral damage). One major implication is that we should expect an increase in both true and false positives, and this effect should be more pronounced when the agent has greater career concerns (as captured by the second part of the proposition). Moreover, provided there is collateral damage, this result applies even when the military are not exerting any bad effort. Crucially, however, the predictions in the cases where there is and is not bad effort diverge, when we look at the comparative statics with respect to the quality of local institutions: part 3 shows that greater α will always increase bad effort and false positives (except in the corner case where there is no bad effort and no collateral damage), while the impact of worse local judicial institutions on true positives is ambiguous. Intuitively, worse local judicial institutions encourage bad effort, and thus false positives, because they make it easier for military personnel to portray such killings as true positives. They also impact good effort, because they permit collateral damage resulting from good effort to be portrayed as true positives. When this collateral damage effect is small (because χ is small) and when there is sufficient substitutability between the two types of efforts, good effort and true positives will decline. These contrasting predictions from (the interaction of) the power of incentives and α on false and true positives is particularly important because it gives us a way to distinguish between a scenario in which false positives are just collateral damage resulting from good effort versus one in which there is a shift towards more bad effort targeted towards killing civilians and disguising them as guerilla combatants, and furthermore because in the data we will indeed find different responses of false and true positives to the quality of local institutions.[21]

We next turn to the implications of high-powered incentives on the quality of local institutions, and show that agents may benefit from weaker local judicial institutions, and as a consequence, may take actions to weaken them given the opportunity.[22]

Proposition 2. ***(Implications for institutions)***
Consider the agent's equilibrium payoff $u(a_T^*, a_F^*)$. *Suppose that* $\delta < \min\left\{\underline{\delta_T}, \underline{\delta_F}\right\}$, *so that an*

[21] In the Appendix (Section A.6), we also show that we cannot distinguish the importance of bad effort relative to good effort by looking at $\exp(q_F)/\exp(q_T)$, which is potentially a nonmonotonic function of the extent of bad effort relative to good effort.

[22] The results in Proposition 1 depend only on marginal incentives and are thus entirely independent of how the intercept of the incentive schedule, τ, is determined. The results in this proposition, on the other hand, depend on expected total payoffs, and thus on τ. Since, to the best of our knowledge, base salaries for officers and soldiers were not modified when high-powered incentives were introduced (and certainly not as a function of whether they were colonels or generals), we assume in the next proposition the most natural benchmark that τ is independent of s and α.

interior solutions exists. Then

$$\begin{aligned}
\frac{\partial u}{\partial \alpha} &= \pi s\left[\chi a_T^* + a_F^* - \eta\pi s\left((1+\alpha\chi)\chi\sigma_T^2 + \alpha\sigma_F^2\right)\right] \lesseqgtr 0 \\
\frac{\partial^2 u}{\partial \alpha \partial s} &= 2\pi\left[\chi a_T^* + a_F^* - \eta\pi s\left((1+\alpha\chi)\chi\sigma_T^2 + \alpha\sigma_F^2\right)\right] \lesseqgtr 0 \\
\frac{\partial^3 u}{\partial \alpha \partial s \partial \pi} &= 4\left[\chi a_T^* + a_F^* - \eta\pi s\left((1+\alpha\chi)\chi\sigma_T^2 + \alpha\sigma_F^2\right)\right] \lesseqgtr 0
\end{aligned}$$

Moreover, each of these expressions is positive if and only if

$$\chi a_T^* + a_F^* > \eta\pi s\left((1+\alpha\chi)\chi\sigma_T^2 + \alpha\sigma_F^2\right)$$

which is satisfied provided that the agent's risk aversion is sufficiently low or the noise for good and bad efforts are sufficiently small.

Proof. By evaluating the agent's payoff at the optimum levels of effort and applying the envelope theorem, we obtain the first expression. The second and third expressions follow from simple differentiation and using (4) and (5) to note that $\pi\frac{\partial a_J^*}{\partial \pi} = a_J^*$ and $s\frac{\partial a_J^*}{\partial s} = a_J^*$ for $J = F, T$. □

Proposition 2 implies that agents may be interested in decreasing the quality of local institutions to raise their payoff (so long as the extra payoff compensates for the cost of the added risk, which happens either when they are not too risk averse or when effort translates to output without much noise). More importantly, in this case, they will also have a more pronounced preference for weaker institutions in the presence of higher-powered incentives when they have stronger career concerns themselves (when π is greater).

4 Data

4.1 Data

Our key dependent variables are the number of false and true positives in a given municipality and period. This is measured biannually, from January to June and July to December. We choose this frequency since promotions and commander appointments are typically done at the beginning of the year or mid-way through the year. In what follows, we refer to each time period as a 'semester'. The basic source is from the Colombian Jesuit NGO "Center for Research and Popular Education" (or CINEP, for its Spanish acronym), which has been collecting high-quality data on violent events in Colombia. Their data include a detailed description of chronologically ordered violent events in Colombia, including date of occurrence, geographical location, the group, or groups, deemed responsible, individuals killed and injured, and the group to which the victims

belong. As primary sources, CINEP relies on press articles from newspapers with both national and regional coverage, and reports gathered directly by several organizations on the ground, especially the clergy. Since the Catholic Church is present even in the most remote areas of the country, CINEP's data on Colombian civil conflict are generally considered very comprehensive and accurate. Using this source and contrasting it with others, Restrepo, Spagat and Vargas (2004) constructed a comprehensive event-based dataset on Colombian conflict that has been widely used. This dataset codes clashes, (one-sided) attacks, and casualties from each of the parties involved in Colombia's internal conflict.

Our true positives measure comes from the updated version of these data, and is defined as killings of rebels (guerillas or paramilitaries) by the government. We use both the number of instances (events) producing such killings as well as the number of rebels killed in the events. As already mentioned above, we define false positives as killings of civilians by the government which were falsely claimed to be rebels killed in combat, and obtain it from CINEP's Data Bank on Human Rights and Political Violence (Banco de Datos de Derechos Humanos y Violencia Política del CINEP). This dataset includes every episode of arbitrary execution and unlawful detention of alleged rebels and specifies: the date and place of recruitment and execution; whether the victim declared to be a member of the guerillas, the paramilitaries or an "unknown" rebel group; whether the perpetrators are from the army, police, or navy; and whether there is an ongoing investigation or sentence in connection with the crime. Again, we use both the number of instances (events) producing such killings and the number of people killed in the events in each municipality and semester.[23]

Our main independent variables are municipal judicial inefficiency and the rank (general or colonel) of brigade commanders in each municipality. To measure judicial inefficiency we use data from the Inspector General (*Procuraduría*), the institution in charge of disciplinary oversight of all public servants. In particular, we have an event-based dataset with all processes arising from complaints against public servants, from 1995 to 2010. With these data, we compute both an initial measure of judicial inefficiency (Jud. Inefficiency$_{m,0}$) and a time-varying measure (Jud. Inefficiency$_{m,t}$):

[23]While any measure of false positives is inevitably imperfect, this dataset appears much better than available alternatives. Official counts based on investigations by judicial and disciplinary authorities may suffer geographic biases as a function of institutional capacity. On the other hand, counts from victims' associations have been criticized as exaggerating the problem. On the whole, this dataset is quite conservative, including 925 cases of false positives, involving 1,513 victims from 1988 to 2011. This contrasts with journalistic accounts quoting approximately 3,000 cases or more (as noted in the introduction, UN commissioner Alston refers to sources citing up to 5,000 cases). Hence, while we cannot completely rule out underreporting of cases, it seems unlikely that we include false positives which are truly killings of guerilla members ('true positives'). This point is particularly important, since it implies that misclassification of true positives as false positives cannot explain the divergent time-series patterns shown in Figures 1 and 3 (and in fact, the magnitude of the decline in true positives is much larger than the increase in false positives).

$$\text{Judicial Inefficiency}_{m,0} = \frac{\sum_{t=1995.1}^{1999.2} \text{Complaints against judicial functionaries}_{m,t}}{\sum_{t=1995.1}^{1999.2} \text{All complaints}_{m,t}},$$

$$\text{Judicial Inefficiency}_{m,t} = \frac{\text{Complaints against judicial functionaries}_{m,t}}{\text{All complaints}_{m,t}}. \; t \in \{2000.1, ..., 2010.2\}$$

Thus, while the initial judicial inefficiency measure looks at the five years preceding our estimation period, the time-varying measure considers the period by period variation in this ratio.

These measures have several advantages. First, they are specifically about a corrupt judicial system, the main dimension of institutional weakness that may affect the ease with which army members may disguise civilian killings as rebels killed in combat (α in our model). Second, some areas in the country may have relatively low reporting rates of all public official abuses because of the weakness of their institutional environment, leading to possible non-classical measurement error. Differences in reporting rates between different municipalities do not influence our measure since by taking the ratio between judicial complaints and other types of complaints, any municipality-specific reporting rate cancels out, leaving only the ratio of judicial abuses to total abuses. Only differences in the reporting rates that vary both by municipality and type of functionary could bias our measure, a possibility that we cannot fully rule out but which should be second-order relative to municipality-wide differences.

Our colonel variable is the share of brigades operating in a given municipality that are led by colonels. We compute a weighted share using the population of all municipalities under a brigade's jurisdiction as weights, to recognize that larger brigades may be more important. In the Appendix, we also report results using the simple unweighted share or a dummy variable indicating whether any brigade operating in the municipality has a colonel commander. We were unable to obtain from the army the historical records of the military structure and the rank of the commanders of different units, but we could reconstruct the historical organizational structure from the Colombian army's webpage. The current structure of the army (jurisdiction and commanders of divisions, battalions and brigades) is available from the army's website. For the past structure, we searched expired versions of the army's website hosted in the Internet Archive's Way Back Machine (`http://archive.org/web/`). These are available since 2000, and to reach further back in time we checked other online sources looking for news that mentioned a particular brigade and its commander, allowing us to identify is rank. We also used news stories from the online archive of *El Tiempo*, Colombia's main national newspaper, enabling us to determine and date the creation of new units and the changes in their command line. This enabled us to identify the rank of brigade commanders on a semester basis.

We also use a range of time-invariant covariates (interacted with time) in our empirical analysis. These are in particular: the logarithm of the population in 2000, average rainfall level, distance to

the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in the year 2000 in math, science and language, poverty index, log of tax income per capita in 2000, a dummy for the presence of the navy, Catholic churches per capita, coca cultivated area per 100 hectares in 1999, and the average protests per capita from 1995 to 1999. In addition, we include a full set of time interactions with initial paramilitary attacks, guerilla attacks, and the unemployment rate in the municipality.[24]

4.2 Descriptive statistics

Table 1 presents descriptive statistics for the main variables in our analysis, both before and after 2003, the beginning of our "incentives" period. Confirming the patterns visible in Figure 1, false positives show a remarkable increase in the period. From just 0.005 cases per semester and municipality before 2003, the average incidence of events involving the killings of civilians to disguise them as combatants rises an order of magnitude, to 0.057, after 2003. We see the same thing in the casualties involved in false positive incidents. There were an average of 0.011 deaths per semester in the years before 2003 (with a maximum of 5 killings), and an average of 0.089 after 2003 (with a maximum of 14 deaths). In contrast, as already shown in Figure 3, episodes producing true positives, while much more frequent, are largely stagnant (0.206 cases per semester and municipality on average before 2003 and 0.207 after 2003), and even declining in terms of the number killed (0.677 average deaths before 2003 and 0.428 after 2003).[25] The judicial inefficiency index similarly shows no marked change on average before and after the incentives period, with judicial complaints representing 6.3% of total before 2003 and 6.6% after 2003. Nevertheless, as our regressions below indicate, there is a relative worsening of the index in colonel-led areas.

Turning to the colonel variables, the average weigthed share of brigades in a municipality having a colonel commander is 10.6% before 2003, rising to 26.8% thereafter. Figures for the unweigthed share are similar, and the dummy variable indicating presence of any colonel-led brigade in the municipality rises from 11% before 2003 to 28.2% afterwards. Finally, Table 1 also describes attacks by each of the main groups in the Colombian conflict: guerillas (with a declining incidence of attacks, from an average of 0.479 to 0.159 per semester before and after 2003), paramilitaries (similarly declining from 0.105 to 0.041), and the government (with a small increase from 0.057 to 0.069).

Table A-2 in the Appendix provides descriptive statistics on our time-invariant variables.

[24]The first two variables are defined as the average yearly attacks of each group between 1991 and 2000 per 100,000 inhabitants. We use the unemployment rate in the municipality in 2005, because this is the earliest year in our sample that is available (this variable is computed only from census data, and the previous census is 1993). Appendix Table A-1 lists all variables in the analysis, describing their definition and sources.

[25]There is an unusually large instance of true positives before 2003 (but shortly after Uribe's inauguration in August): the killing of 260 guerilla members after the armed forces bombarded a FARC camp in Ituango, Antioquia, on September 19 of 2012.

5 Results

Figures 1 and 3, presented in the Introduction, already show a sizable increase in false positives with no corresponding increase in true positives during the period in which the high-powered incentives were in operation. This time-series evidence thus suggests a link between the high-powered incentives of Colombia's top-down state-building project and false positives. But, by its nature, this evidence cannot distinguish between confounding events that may be impacting both true and false positives in the time series. Our main evidence, instead, comes from the cross-sectional implications of high-powered incentives as outlined in our theory section. In the remainder of this section we describe our empirical strategy to investigate these predictions, the main results of this empirical strategy, a range of robustness checks and also results on the impact of high-powered incentives on the quality of institutions.

5.1 Empirical Strategy

The main idea we investigate in our empirical work is the one emphasized by Proposition 1, that following the introduction of high-powered incentives for the military personnel, the increase in false and true positives should be larger in places where brigades are commanded by colonels (who have stronger career concerns and thus should be more responsive to high-powered incentives). We then attempt to distinguish between the scenarios where all these patterns can be explained as a consequence of collateral damage versus those in which there is a significant increase in bad effort towards deliberately killing and then disguising civilians. For this we will exploit the result that while false positives should also respond more to higher-powered incentives in areas with weaker judicial institutions, the same is not necessarily true for true positives. Finally, we will turn to the impact of these high-powered incentives on local judicial institutions.

All of our results are obtained from regressions of the following form, where m denotes municipality and t semester:

$$\begin{aligned} y_{m,t} &= \sum_{j=1}^{2} \nu_j^{\text{Pre,Col}} \left(\text{Pre}_j \times \text{Colonel}_{m,t}\right) + \beta^{\text{Col}} \left(\text{Incentives}_t \times \text{Colonel}_{m,t}\right) + \sum_{j=1}^{2} \nu_j^{\text{Post,Col}} \left(\text{Post}_j \times \text{Colonel}_{m,t}\right) \\ &+ \sum_{j=1}^{2} \nu_j^{\text{Pre,Jud}} \left(\text{Pre}_j \times \text{J. Ineff}_{m,0}\right) + \beta^{\text{Jud}} \left(\text{Incentives}_t \times \text{J. Ineff}_{m,0}\right) + \sum_{j=1}^{2} \nu_j^{\text{Post,Jud}} \left(\text{Post}_j \times \text{J. Ineff}_{m,0}\right) \\ &+ \varphi \text{Colonel}_{m,t} + \delta_m + \gamma_t + \sum_{x \in \mathbf{X_m}} \Phi_x (x \times \gamma_t) + \varepsilon_{m,t}. \end{aligned} \tag{8}$$

In (8), $y_{m,t}$ is our outcome variable of interest — in our main results, either true or false positives, but also later the quality of judicial institutions in the municipality. In our main specifications, these variables are parameterized as $\ln(1+x)$, since there are many municipality-semester observations

in which false positives or true positives are equal to zero.[26] $\text{Colonel}_{m,t}$ is the share of brigades with jurisdiction over m that are commanded by colonels, while $\text{J. Ineff}_{m,0}$ is our measure of judicial inefficiency in the municipality. As already discussed above and suggested by Figure 1, we will use two specifications for Incentives_t: either an indicator variable for the period in which incentives were in place (from the first semester of 2003 to the second semester of 2008) or a linear trend for this period. This latter parameterization attempts to capture both the intensification of higher-powered incentives and the potentially cumulative effects of these policies while they were in effect. Throughout, we also always include two semesters of additional interactions before the period in which the incentives are in effect (first and second semesters in 2002, denoted by Pre_1 and Pre_2 in the expression), which will act as a simple test for whether there are pre-trends in municipalities where the brigades are commanded by colonels and where judicial institutions are weak (we also investigate this issue by including municipality-specific time trends in our robustness checks). In some specifications we also include two semesters of additional interactions after the period of incentives (denoted by Post_1 and Post_2) to see whether once the government reverses its policy of higher-powered incentives with little oversight, false positives show a sharp decline and what the impact of this is on true positives.

In addition, we include a full set of municipality fixed effects δ_m, thus focusing on within-municipality variation, before and after the ratcheting up of incentives, and a full set of time fixed effects γ_t capture any national-level trend in false or true positives. Notice also the penultimate term $\sum_{x \in \mathbf{X_m}} \Phi_x(x \times \gamma_t)$ in (8), which stands for a full set of time (semester) interactions with a rich set of time-invariant municipality characteristics, which were described in Section 4. These interactions also control for any potential differential trends that might exist by municipalities that differ in terms of their economic, social, geographic or historical features.[27]

The key coefficients are β^{Col} and β^{Jud}, and measure the differential response of false or true positives to a greater share of colonels and weaker institutions in a municipality during the high-powered incentive period. In addition, the ν^{Pre} coefficients also matter greatly as they indicate whether there is prima facie evidence that municipalities with a higher share of colonels and with weaker judicial institutions appear to be on differential trends.

[26] In the Appendix, we use the inverse hyperbolic sine parameterization as well, which is more flexible and yields very similar results. The inverse hyperbolic sine transformation is defined as $\ln(x+\sqrt{1+x^2})$ and except for very small x, coefficients in this specification can be interpreted as percentage impacts (notice that its derivative is $1/\sqrt{1+x^2}$, which if x is not too small approximates $1/x$, the derivative of $\ln(x)$). The use of these specifications is motivated by the fact that we are unable to estimate nonlinear count models because of the size of the dataset and also to a lesser extent because of the number of right-hand side variables included.

[27] We also checked whether there is a strong systematic correlation between the assignment of colonels to a specific area and that area's time-invariant and time-varying characteristics. A regression similar to (8), but with the share of colonels on the left-hand side and just semester and municipality dummies on the right-hand side has an R^2 of 0.479. When the time-invariant characteristics interacted with time are added, the R^2 increases by an additional 0.085, to 0.564, which is quite modest. When the judicial inefficiency variables are added, there is a further increase of 0.001, which is again very modest, and suggests that there is no strong correlation between the assignment of colonels and municipality characteristics, especially judicial institutions.

Finally, throughout all standard errors are corrected for spatial and first-order time correlation following Conley (1999, 2008).[28]

5.2 Main results

Table 2 shows our baseline results for false positives. We initially estimate equation (8) for false positives. Odd-numbered columns look at the cases of false positives, while even-numbered columns are for casualties from these incidents. The first two columns are for the specification where incentives are parameterized as a dummy variable, while the next two are for the case in which they are parameterized as a linear trend during the period of incentives. The last four columns also include the interactions with the two semesters following the period of high-powered incentives.

Overall, the picture is very clear. In all specifications, the interactions with share of colonels and judicial inefficiency in the municipalities are positive and significant — at 5% or less with the dummy specification and at 1% or less with the linear trend specification of incentives. The positive coefficients indicate that during the period of higher-powered incentives, false positives increased significantly more in municipalities where there were more colonels in charge and institutions were weaker. Also notably, there is no evidence of pre-trends. Finally, columns 5-8 show that there is no evidence that these differential effects survived to the years after the end of the higher-powered incentives.

Table 3 turns to true positives. Here too the pattern is fairly clear. Though in the time series, true positives did not show an increase during the period of higher-powered incentives, they appear to increase more during this period in municipalities with a greater share of colonels in charge relative to other municipalities. More notably, for the purposes of distinguishing the pure collateral damage story from the switch to bad effort scenario, there is no evidence of a greater increase in true positives during this period in municipalities with weak judicial institutions — the interaction between judicial inefficiency and the incentive variable is significant at 10% in columns 3, 6 and 7, but has the opposite signed to that predicted by a pure collateral story. In addition, there is again no indication of pre-trends in this table either. Finally, the general picture from columns 5 to 8 is once again one in which these effects die out once the high-powered incentives on the army are removed.

The magnitudes of the coefficient estimates in Tables 2 and 3 are difficult to interpret since the left-hand side variable is parameterized as $\ln(1+x)$ and the coefficients of interest are interaction terms. Table 4 gauges their magnitudes by computing the counterfactual changes in false and true positives when all brigades are commanded by colonels or when all municipalities are brought to the level of lowest judicial inefficiency. The numbers are very consistent across panels and specifications. The counterfactual exercise of removing the colonels reduces false positive cases and casualties by

[28] Specifically, we allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor.

about 6.5% (estimates ranging from 6.2 to 6.92%), while getting rid of judicial inefficiency has a slightly larger impact (ranging from 6.49 to 8.79%). We further find that both of these exercises have much smaller effects on true positives, ranging from a decline of 0.74 to 1.16% for getting rid of colonels and an increase from 0.64 to 1% for getting rid of judicial inefficiency. Thus, our estimates suggest that, conditional on having the high-powered incentives in place, introducing the appropriate checks and removing the agents with the worst incentives for abuses due to their career concerns would have had little cost in terms of combating the guerillas, but would have saved a significant number of innocent civilian lives.[29] We should emphasize that these counterfactuals do not inform us about the implications of not having the high-powered incentives in place, which are absorbed by the time effects. Indeed, Figure 1 suggests that these may have been quantitatively much more important than removing the worse career concerns and having better checks in an environment of otherwise very high-powered incentives.

5.3 Robustness

We next report several robustness exercises, which show that the patterns in Tables 2 and 3 are generally robust and bolster our confidence in the general picture presented so far.

Table 5 starts by including municipality-specific linear trends. As in all the robustness tables in the text, we no longer report specifications with the post-interactions and instead combine false and true positives in a single table. Though much more demanding, these specifications including municipality-specific trends show very similar results to our baseline. The coefficient estimates for the interactions with judicial inefficiency in the regressions for false positives are very comparable to those in Tables 2. Those for the interactions with share of colonels are about 10% smaller in some specifications, but still statistically significant with the single exception of the dummy specification for incentives and false positive cases on the left-hand side. We do see two pre-trends coefficients that are significant at 10% in this case, which is no more than what we might have expected ex ante since there are total of 32 such coefficients. The interactions for share of colonels in the regressions for true positives are generally similar to those in Table 3, and the interactions for judicial inefficiency in these regressions continue to be insignificant in most specifications.

Table 6 probes the robustness of our results in a different dimension — by dropping outliers. Specifically, we drop all municipality-semester observations that are below the 5th or above the 95th percentile in the distribution of residuals in our baseline regressions. The qualitative nature of the results changes very little, though the parameter estimates are somewhat lower for the key interactions for false positives (especially with judicial inefficiency) and are higher for the interactions with the share of colonels for true positives. There continues to be no evidence of a differential increase in true positives in areas with weaker judiciary, and no indication of systematic

[29] We use the coefficient estimates, regardless of whether they are statistically significant. The main message is similar when we do not use insignificant coefficients.

pre-trends.

Table 7 takes yet another approach and includes a fourth-order polynomial in true positives on the right-hand side (parameterized again as $\ln(1+x)$) when estimating the impact of higher-powered incentives on false positives. Though true positives, which are also endogenous to incentives, are a 'bad control' (Angrist and Pischke, 2008), this specification is nonetheless a useful, even if demanding, check as it verifies whether there is an increase in false positives over and beyond that which would be predicted by a simple collateral damage story linking false positives to a known function of true positives. The results are very similar, both qualitatively and quantitatively, to those presented in Table 2, and provide another piece of evidence against the hypothesis that false positives are just a consequence of collateral damage from effort directed towards killing the guerilla.

In addition to these robustness checks reported in the text, in the Appendix we show that the results are very similar when we do not include any covariates, other than municipality and time fixed effects, in our baseline specifications (which verifies that the patterns we are documenting are not driven by any of the controls we are including on the right-hand side); when we use the unweighted share of colonels or a dummy for any brigade commanded by a colonel in the area instead of the share of colonels; when we use the inverse hyperbolic sine transformation for the left-hand side variable, which flexibly covers the linear and the logarithmic cases; and when we control for a quartic in population on the right-hand side.[30] In addition, we investigated whether false positives are driven solely by mobile brigades, which increased in number over the period and were always led by colonels, by separately including the share of colonel-led regular and the share of mobile brigades. The results of this exercise show that both types of colonel-led brigades are associated with more true and false positives in the incentives period. Finally, we also investigated whether there are significant spillovers from the career concern-induced incentives in neighboring municipalities. This is a possibility which could potentially bias our estimates because guerillas may relocate from one area to another in response to differential incentives of brigades to attack them. To do this, we constructed neighbors' incentives by taking the arithmetic average of the share of colonels in all neighboring (adjacent) municipalities,[31] and found no significant effects from incentives or judicial institutions of neighboring areas on false or true positives in a given municipality.

[30] In the case of the inverse hyperbolic sine transformation and the changes in the measure of colonels, because coefficients are not comparable to our baseline estimates, we also computed the implied magnitudes as in Table 4 and verified that they are very similar.

[31] Acemoglu, Garcia-Jimeno and Robinson (2015) incorporate both distance and changes in elevation of the terrain between municipalities to weight municipalities' neighbors. For simplicity, regressions reported below take the unweighted average of neighbors' characteristics instead.

5.4 The impact of high-powered incentives on institutions and security

In this subsection, we turn to the impact of high-powered incentives on the quality of institutions. As argued in Proposition 2, powerful agents may have heightened incentives to weaken local institutions when they start facing higher-powered incentives (and are shifting towards bad effort).

In Table 8, we start with the effect of higher-powered incentives coming from the more pronounced career concerns of colonels on the quality of local judicial institutions. We thus estimate (8) with the time-varying judicial inefficiency variable on the left-hand side, and of course without any interactions involving the judicial inefficiency variable on the right-hand side. These specifications show that judicial inefficiency increases differentially in municipalities with a higher share of colonels during the period of higher-powered incentives. However, there is a significant and large negative differential effect in the first semester of 2002 which is concerning. In Table 9, we investigate the source of this pre-trend, and show that it is caused by outliers. When we take out outliers in the same manner as in Table 9, the impact of high-powered incentives on the quality of local judicial institutions remains similar as in Table 8, but the pre-trend in the first semester of 2002 disappears (though there is now a positive significant impact in the second semester of 2002 in columns 5 and 6). Overall, though the pre-trends in some of the specifications make us a little cautious in overinterpreting these results, they do appear to indicate worsening judicial institutions in places where career concerns of commanders were conducive to generating extrajudicial killings.

In Tables 10 and 11, we turn to the effect of higher-powered incentives on attacks by the guerillas and paramilitaries. Since high-powered incentives were ostensibly directed at increasing the state's military control and monopoly of violence, we should find these attacks declining in places where the military have stronger incentives. But the pattern is quite different. For guerilla attacks, in most specifications, we see a positive effect from the interactions with the share of colonels, and no effect from interactions with local judicial institutions. For paramilitary attacks, we see even more clear-cut evidence of a positive impact from a higher share of colonels in the municipality during the period of high-powered incentives, and no significant impact from interactions with judicial inefficiency. Moreover, the effect from interactions with the share of colonels continues even after the period of high-powered incentives comes to an end. These results suggest that the top-down state building projects via high-powered incentives were at best ineffective, and at worst counterproductive, in expanding the control of the state over nonstate armed actors. Though this is a little speculative, the most likely explanation for this paradoxical result is that higher-powered incentives of the military may have worsened rather than improved the security situation. The time pattern further suggests that, in areas where the military had incentives for extrajudicial killings, the security situation may have worsened so much, and trust of the population in the military may have collapsed so completely, that the power of the guerilla and paramilitary forces may have increased and continued to do so even after the period of high-powered incentives came to an end.

Finally, we also look at the government attacks on non-state actors. At the very least, higher-

powered incentives should have increased government attacks. However, the results in Table 12 are once again paradoxical and show no effect on government attacks of having a higher share of colonels (with the stronger career concerns and responsiveness to incentives) and worse local judicial institutions (with less ability to check the higher-powered incentives of the military). The results must be interpreted with caution, particularly because there is a negative pre-trend in the interaction of colonels with the first semester of 2002. Nevertheless, this result also adds to the impression that the higher-powered incentives for soldiers — at least when focusing on variation across municipalities in the intensity of these incentives — did not achieve its main aim of significantly improving the security situation in Colombia.

In sum, these results paint a picture of high-powered incentives given to the military being fairly ineffective at improving the state's monopoly of violence. Not only do we see a sizable increase in false positives, documented in the previous subsections, but there is evidence that the areas where these incentives were strongest experienced a deterioration in their judiciary, and even in their security situation.

6 Conclusions

Creating a secure environment for the population and building state capacity are some of the most pressing problems facing poor and even middle-income countries all around the world. The conventional wisdom views the first step of this process to be the establishment of the state's monopoly of violence over its territory. Other aspects of state capacity, including bureaucratic, fiscal and administrative capacity and rule of law, are viewed as dimensions that can be developed later (sometimes much later). Many attempts to build this monopoly of violence are top-down, starting with a strong leader or an elite-driven state building project imposed on society without its consent. However, such top-down attempts to build the state may create unintended negative consequences because, given their focus, they are often unidimensional, paying attention only to military control. This unwavering focus on military control, particularly in an environment where other dimensions of state capacity are underdeveloped and do not provide effective mechanisms of accountability, can create incentives for those controlling the state or its agents to take actions that undermine not only other dimensions of state capacity but possibly the trust of the population in the institutions of the state.

President Alvaro Uribe's policy of strengthening the military and its incentives to combat the guerilla after he came to office in 2002, provides an example of such top-down state building efforts. In this paper, we have shown that these efforts appear to have indeed created very significant unintended consequences and may have even failed in many of their aims, while also weakening the judicial dimension of state capacity. After presenting a simple multi-tasking model adapted to this environment, we presented evidence consistent with the implications of this model. The evidence

suggests that the high-powered incentives, which rewarded soldiers for killing non-state armed actors, particularly guerillas, in combat led to a large upsurge in illegal murders of civilians, who were then disguised to look like guerillas. Crucially, this happened more in municipalities where military units were headed by colonels, who have stronger career concerns because of their promotion incentives, and where local judicial institutions were less efficient and thus presumably less capable of investigating reports of killings of innocent civilians. We also found that the efficiency of judicial institutions further deteriorated in places where brigades were led by colonels, presumably because this made it easier to execute civilians and get away with it. Even more counter-productively, we found that in these same places there was little evidence of increased army attacks on guerillas, but both guerilla and paramilitary attacks increased permanently.

Taken together, our results suggest that not only did the introduction of a top-down, unidimensional state-building project in Colombia bring about significant loss of innocent human life, but it was counter-productive even in terms of the explicit goals it was trying to achieve. The main lesson we draw from them is that even if one's goal is to attain a legitimate monopoly of violence, it is critical to try to build state institutions in multiple dimensions simultaneously and that high-powered incentives in the absence of accountability can be highly perverse.

Though the situation in Colombia is unique, there are many other examples of top-down, unidimensional state-building efforts, which might be expected to have similar perverse, unintended consequences. Similar counterinsurgency strategies and arbitrary executions by the military and security services with little judicial oversight are quite common in the rest of Latin America. In Peru and Guatemala, for example, post-conflict truth and reconciliation commissions have documented widespread killings of civilians. In Peru, the commission documents the "cold-blooded" killings of individuals outside combat, which were used "repeatedly by members of the Army, the Navy, and the Police as part of the counterinsurgent strategy from 1983 to 1996" (Comisión de la Verdad y la Reconciliación, 2003, p. 134). The impetus for these murders came, according to the commission's report, from a top-down, security-first logic: "by privileging a military approach, one of the main objectives of the counterinsurgent strategy was the elimination of members, sympathizers or collaborators of armed insurrection, even more than the objective of capturing them to be judged by the competent judicial authorities" (p. 146). The report also emphasizes the importance of the lack of judicial control in these outcomes (p. 176). Interestingly, the Guatemalan commission also reaches a similar conclusion to our study on the adverse effects of such a strategy on the quality of judicial institutions, stating:

> "Militarization became a pillar of impunity. Moreover, in a broad sense, it weakened the country's institutions, reducing their possibilities for functioning effectively and contributing to their loss of legitimacy" (Comisión para el Esclarecimiento Histórico, 1999, p.28).

It goes on to conclude: "The justice system, nonexistent in large areas of the country before the armed confrontation, was further weakened when the judicial branch submitted to the requirements of the dominant national security model." (p. 36)

These issues are relevant beyond Latin America as well. Attempts to re-create the monopoly of violence that the state had lost by relying on a top-down strategy in Somalia, Afghanistan and Iraq all appear to have backfired in recent decades. From the vantage point of our general approach, this might have been, in part, because they sought to create strong incentives for the security forces to combat rebels or insurgents, without much of an effort to build support from the local population. Even states that already have a firm monopoly of violence may be impacted by such considerations as suggested by the recent paper of Fisman and Wang (2015), which studies the implications of the introduction of death ceilings in China to incentivize bureaucrats to reduce accidental deaths in their provinces. The study finds large reported reductions in deaths, but concludes that they were most likely due to manipulation by bureaucrats to appear to achieve their targets in the face of high-powered incentives.

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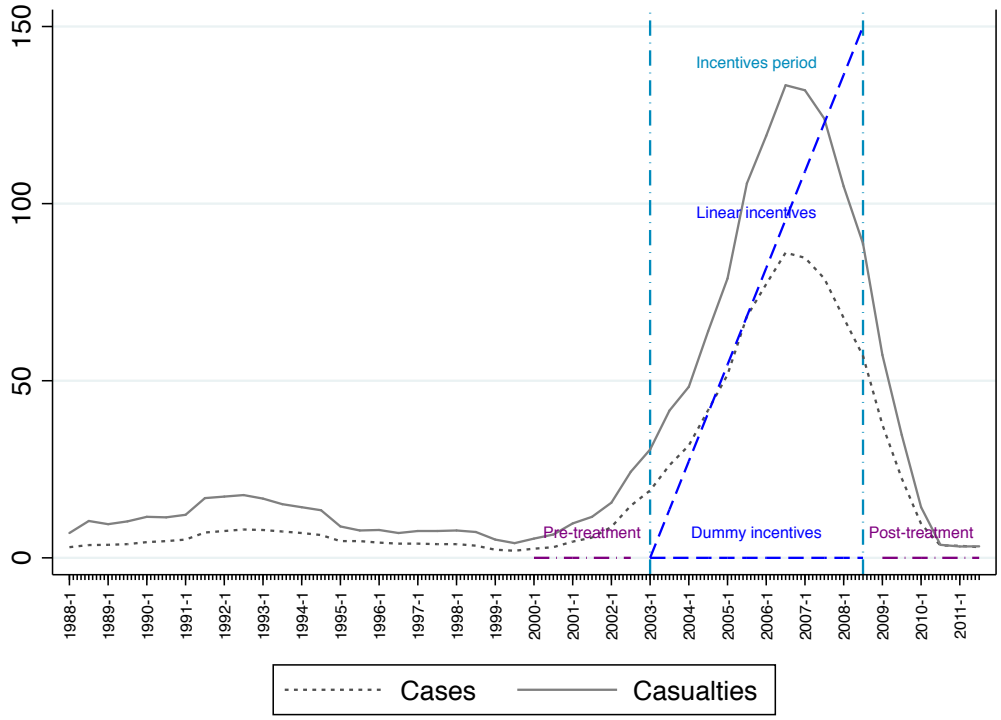
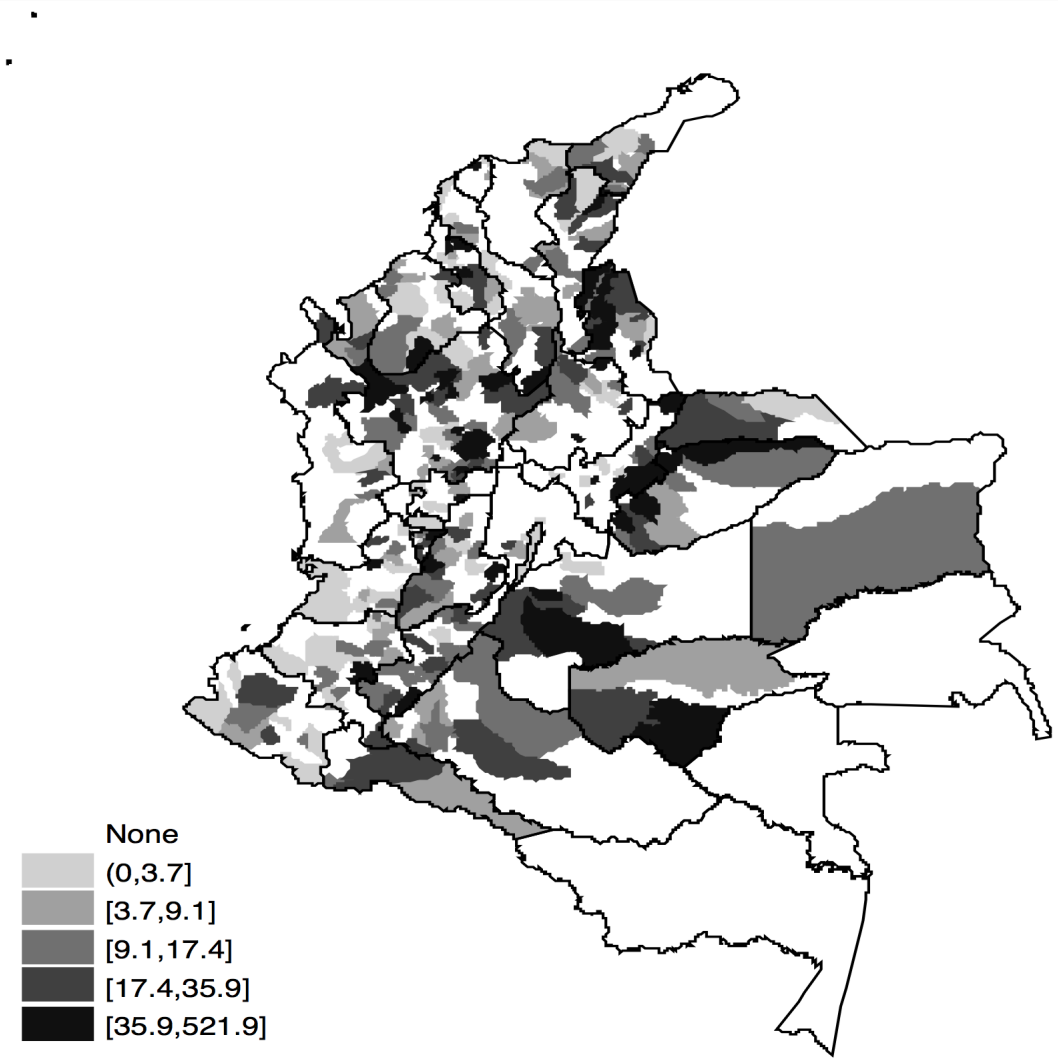
Figure 1: False Positives by Semester
Cases and casualties 1988 - 2011



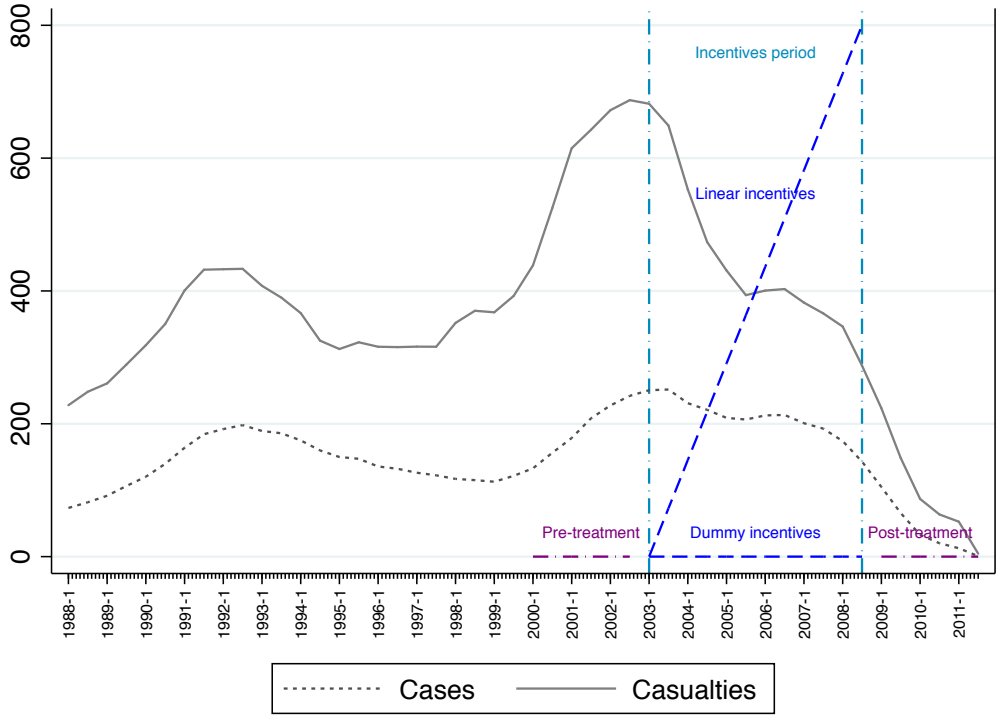
Notes: False positives between the first semester of 1988 and the second semester of 2011. Cases is the total number of events producing false positives, while casualties are the total number of people that were killed in these events. In both cases we depict the 3-semester moving average of the raw numbers.
Source: CINEP.

Figure 2: False positives
Total executions per 100.000 inhabitants



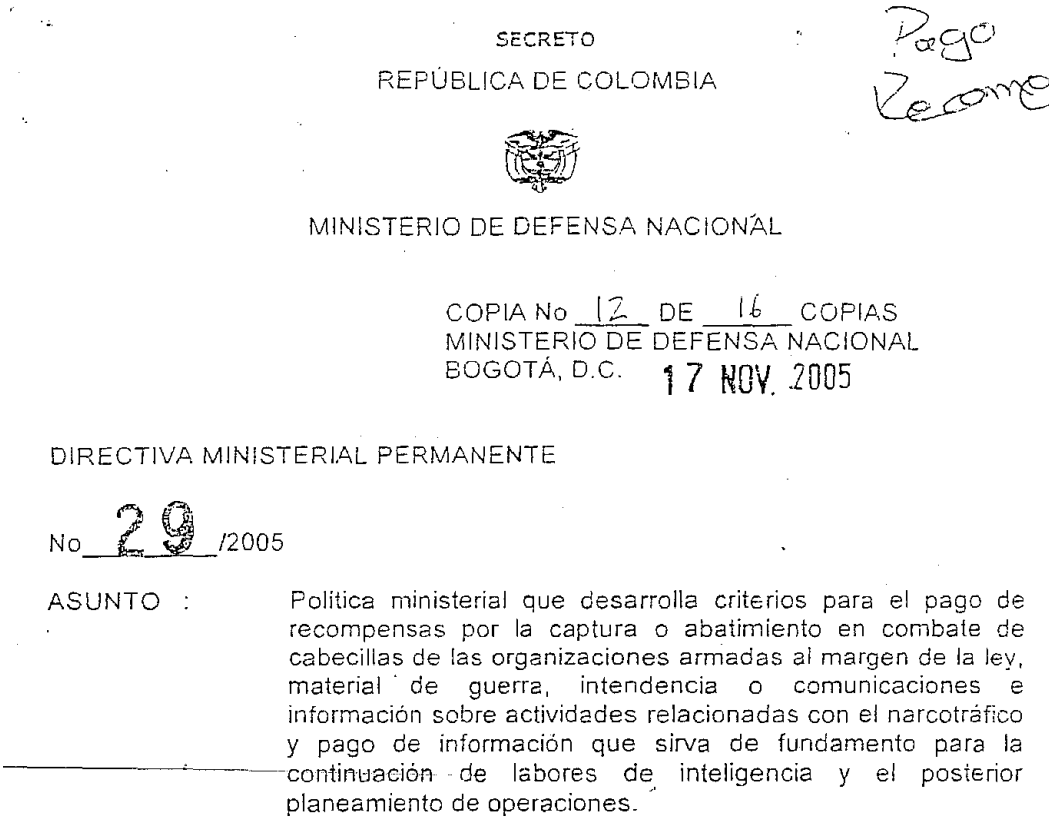
Notes: False positives per municipality over the entire sample period. Own calculations with data from CINEP (false positives) and DANE (population).

Figure 3: True Positives by Semester
Cases and casualties 1988 - 2011



Notes: True positives between the first semester of 1988 and the second semester of 2011. Cases is the total number of events producing true positives, while casualties are the total number of people that were killed in these events. In both cases we depict the 3-semester moving average of the raw numbers.
Source: CINEP - Restrepo, Spagat and Vargas (2004).

Figure 4: 'Secret' Army Directive 29 of November 17 2005
Colombian Ministry of Defense



SECRETO

REPÚBLICA DE COLOMBIA

Pago
Recompe

MINISTERIO DE DEFENSA NACIONAL

COPIA No 12 DE 16 COPIAS
MINISTERIO DE DEFENSA NACIONAL
BOGOTÁ, D.C. 17 NOV. 2005

DIRECTIVA MINISTERIAL PERMANENTE

No 29 /2005

ASUNTO : Politica ministerial que desarrolla criterios para el pago de recompensas por la captura o abatimiento en combate de cabecillas de las organizaciones armadas al margen de la ley, material de guerra, intendencia o comunicaciones e información sobre actividades relacionadas con el narcotráfico y pago de información que sirva de fundamento para la continuación de labores de inteligencia y el posterior planeamiento de operaciones.

Notes: Exhibit of the army directive leaked by the press, detailing a reward schedule for killings and capturing members of illegal groups, seizing weapons, and sharing information. National media and political commentators criticized many of the features of this directive, as likely triggers of the increase in killings of civilians later presented as rebels, know as false positives.

Figure 5: Presidential Decree 1400 of May 5 2006 or BOINA Bonuses for Operations of National Importance

REPÚBLICA DE COLOMBIA

DEPARTAMENTO ADMINISTRATIVO DE LA FUNCIÓN PUBLICA

DECRETO NUMERÓ **1400** DE 2006

5 MAY 2006

Por el cual se crea la Bonificación por Operaciones de Importancia Nacional - BOINA

EL PRESIDENTE DE LA REPÚBLICA DE COLOMBIA,

En desarrollo de las normas generales señaladas en la Ley 4ª de 1992,

DECRETA:

ARTÍCULO 1º. BONIFICACIÓN POR OPERACIONES DE IMPORTANCIA NACIONAL -BOINA. Créase la Bonificación por Operaciones de Importancia Nacional - BOINA, para los Miembros de la Fuerza Pública y funcionarios del Departamento Administrativo de Segundad - DAS, que participen en una operación de importancia nacional, la cual se otorgará por cada ocasión,

PARÁGRAFO 1º. Esta bonificación podrá ser otorgada a una misma persona tantas veces cuantas se haga acreedora a ella, por participación en operaciones de importancia nacional.

PARÁGRAFO 2º. La Bonificación de que trata este artículo, solo será reconocida y pagada por la participación en la respectiva operación de importancia nacional.

PARÁGRAFO 3º. Esta bonificación no constituye factor para liquidar elementos salariales o prestacionales, ni se tendrá en cuenta para determinar remuneraciones de otros servidores públicos.

ARTÍCULO 2º. OPERACIONES DE IMPORTANCIA NACIONAL. Para efectos del presente decreto, se consideran de importancia nacional aquellas operaciones en las cuales se logre la captura de los cabecillas de los niveles I y II que se encuentran determinados en la Directiva expedida por el Ministro de Defensa Nacional.

Notes: Exhibit of Presidential Decree 1400 of May 5 2006, rewarding army members or DAS functionaries –Colombia's former intelligence agency– with up to 12 times their monthly salary for participating in successful operations of "national importance" against the insurgency. It is known that these bonuses, authorized by the President, were not directly responsible for false positives. But they did fit into the general policy of providing high-powered incentives in the fight against the guerilla.

Figure 6: False positives
Evidence of Scene Manipulation

Notes: The figure shows four pictures from alleged guerillas killed in combat. The picture in the top left reveals that the victims fingers were artificially placed on the trigger. Moreover, forensic tests revealed that the weapon was not fired. The second picture, to the right, shows three grenades dangerously placed on a victim's pockets, where they could easily explode in the midst of combat. In the bottom left picture the victim is wearing the right boot on the left foot, and vice versa. Finally, in the bottom right picture the cartridge is stored inside the boot, an uncomfortable place for combat.

Source: Semana, "Los casos olvidados de los falsos positivos", July 17, 2010, `http://www.semana.com/nacion/articulo/los-casos-olvidados-falsos-positivos/119416-3`.

Table 1: Descriptive Statistics: Time-varying variables

Variable	Mean	Std. Dev.	Min	Max	Mean	Std. Dev.	Min	Max
	Before first semester of 2003				After first semester of 2003			
False positives:								
Cases	0.005	0.090	0	3	0.057	0.353	0	14
Casualties	0.011	0.195	0	5	0.089	0.588	0	14
True positives:								
Cases	0.206	0.618	0	8	0.207	0.704	0	17
Casualties	0.677	5.076	0	260	0.428	1.775	0	45
Judicial Inefficiency	0.063	0.161	0	1	0.066	0.156	0	1
Colonel in charge (weighted share)	0.106	0.308	0	1	0.268	0.440	0	1
Colonel in charge (unweighted share)	0.108	0.309	0	1	0.274	0.442	0	1
Colonel in charge (dummy)	0.110	0.313	0	1	0.282	0.450	0	1
Guerilla attacks	0.479	1.248	0	16	0.159	0.613	0	10
Paramilitary attacks	0.105	0.442	0	6	0.041	0.333	0	15
Government attacks	0.057	0.284	0	6	0.069	0.478	0	30

Notes: Data from the first semester of 2000 to the second semester of 2010. False positives cases are number of instances were civilians are killed to be presented as rebels in a given municipality and semester, while casualties are the total number killed in these events. True positives cases are events producing true killings of rebels and casualties the total number of rebels killed. Judicial inefficiency is the ratio of complaints against judicial functionaries relative to total complaints against all public officials. Colonel in charge (unweighted share) is the fraction of brigades with jurisdiction over the municipality that are led by colonels, the weighted share takes the population of municipalities under each brigade's jurisdiction as weights, and dummy is a dichotomous variable indicating whether any brigade present in the municipality is led by a colonel. Guerilla, paramilitary, and government attacks are the number of one-sided attacks by each of these groups.

Table 2: False positives, colonels and judicial inefficiency, 2000-2010
Baseline results

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false positives)								
Judicial Inefficiency								
... x 2002:1	−0.0049	0.0200	−0.0128	0.0104	−0.0037	0.0204	−0.0138	0.0086
	(0.0309)	(0.0439)	(0.0315)	(0.0447)	(0.0314)	(0.0446)	(0.0323)	(0.0457)
... x 2002:2	−0.0114	−0.0003	−0.0193	−0.0099	−0.0102	0.0001	−0.0203	−0.0118
	(0.0544)	(0.0776)	(0.0547)	(0.0781)	(0.0547)	(0.0780)	(0.0551)	(0.0786)
... x Incentives (2003:1-2008:2)	0.0829***	0.1118***	0.0100***	0.0138***	0.0841***	0.1122***	0.0099***	0.0136***
	(0.0229)	(0.0307)	(0.0032)	(0.0046)	(0.0236)	(0.0317)	(0.0033)	(0.0048)
... x 2009:1					−0.0045	0.0022	−0.0143	−0.0094
					(0.0260)	(0.0307)	(0.0270)	(0.0322)
... x 2009:2					0.0142	0.0011	0.0044	−0.0105
					(0.0302)	(0.0295)	(0.0311)	(0.0311)
Colonel in charge (share)								
... x 2002:1	−0.0028	−0.0074	−0.0017	−0.0071	−0.0031	−0.0073	−0.0011	−0.0063
	(0.0066)	(0.0091)	(0.0063)	(0.0087)	(0.0067)	(0.0092)	(0.0064)	(0.0088)
... x 2002:2	−0.0109	−0.0229	−0.0098	−0.0226	−0.0112	−0.0228	−0.0092	−0.0218
	(0.0083)	(0.0146)	(0.0081)	(0.0143)	(0.0084)	(0.0147)	(0.0082)	(0.0144)
... x Incentives (2003:1-2008:2)	0.0171**	0.0272***	0.0023***	0.0033***	0.0167**	0.0273***	0.0024***	0.0034***
	(0.0071)	(0.0094)	(0.0008)	(0.0010)	(0.0073)	(0.0098)	(0.0008)	(0.0011)
... x 2009:1					−0.0026	0.0001	0.0007	0.0029
					(0.0067)	(0.0081)	(0.0070)	(0.0086)
... x 2009:2					0.0008	0.0004	0.0039	0.0031
					(0.0070)	(0.0077)	(0.0073)	(0.0082)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.065	0.063	0.065	0.064	0.065	0.063	0.065	0.064

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 3: True positives, colonels and judicial inefficiency, 2000-2010
Baseline results

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+true positives)								
Judicial Inefficiency								
... x 2002:1	0.1285	0.3681	0.1298	0.3947	0.1096	0.3380	0.1172	0.3802
	(0.1307)	(0.3341)	(0.1295)	(0.3320)	(0.1329)	(0.3375)	(0.1307)	(0.3336)
... x 2002:2	−0.0061	−0.2569	−0.0049	−0.2302	−0.0250	−0.2870	−0.0175	−0.2448
	(0.1098)	(0.1804)	(0.1085)	(0.1769)	(0.1124)	(0.1868)	(0.1099)	(0.1799)
... x Incentives (2003:1-2008:2)	−0.0626	−0.1293	−0.0086*	−0.0121	−0.0815	−0.1594*	−0.0100*	−0.0137
	(0.0471)	(0.0793)	(0.0051)	(0.0083)	(0.0530)	(0.0931)	(0.0054)	(0.0091)
... x 2009:1					−0.0298	−0.0556	−0.0218	−0.0128
					(0.0911)	(0.1468)	(0.0880)	(0.1380)
... x 2009:2					−0.1220	−0.1865	−0.1141	−0.1438
					(0.0790)	(0.1433)	(0.0754)	(0.1343)
Colonel in charge (share)								
... x 2002:1	0.0099	−0.0146	0.0117	−0.0100	0.0102	−0.0128	0.0134	−0.0059
	(0.0316)	(0.0480)	(0.0314)	(0.0478)	(0.0316)	(0.0483)	(0.0315)	(0.0480)
... x 2002:2	−0.0362	−0.0932	−0.0344	−0.0887	−0.0359	−0.0914	−0.0327	−0.0846
	(0.0366)	(0.0590)	(0.0365)	(0.0589)	(0.0365)	(0.0590)	(0.0364)	(0.0589)
... x Incentives (2003:1-2008:2)	0.0248**	0.0294*	0.0034***	0.0045**	0.0252*	0.0314	0.0036***	0.0049**
	(0.0119)	(0.0176)	(0.0012)	(0.0018)	(0.0130)	(0.0194)	(0.0013)	(0.0020)
... x 2009:1					0.0038	0.0176	0.0089	0.0272
					(0.0155)	(0.0250)	(0.0154)	(0.0246)
... x 2009:2					−0.0025	−0.0081	0.0025	0.0012
					(0.0166)	(0.0304)	(0.0166)	(0.0303)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.062	0.060	0.062	0.060	0.062	0.060	0.062	0.060

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 4: False and true positives, colonels and judicial inefficiency, 2000-2010
Size of the effects

	Without Post Trend				With Post Trend			
	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Observed (false)	853	1403	853	1403	853	1403	853	1403
Observed (true)	6542	15851	6542	15851	6542	15851	6542	15851
Panel A. Dependent variable is log (1+false positives)								
Judicial Inefficiency to minimum								
Change	−74	−104	−64	−92	−75	−105	−63	−91
Percent Change	−8.68	−7.41	−7.5	−6.56	−8.79	−7.48	−7.39	−6.49
Colonels to Generals								
Change	−58	−87	−59	−89	−59	−87	−57	−87
Percent Change	−6.8	−6.2	−6.92	−6.34	−6.92	−6.2	−6.68	−6.2
Panel B. Dependent variable is log (1+true positives)								
Judicial Inefficiency to minimum								
Change	64	159	61	101	84	197	71	114
Percent Change	0.98	1	0.93	0.64	1.28	1.24	1.09	0.72
Colonels to Generals								
Change	−75	−128	−76	−130	−74	−123	−71	−117
Percent Change	−1.15	−0.81	−1.16	−0.82	−1.13	−0.78	−1.09	−0.74

Notes: Using the corresponding regressions in Tables 2 and 3, we compute the predicted change in false and true positives, respectively, of either setting judicial inefficiency to its minimum (zero) or setting all brigades to be led by generals (fixing the colonel share at zero). The first line in each case shows the predicted change in the number of false or true positives, and the second the percent change relative to observed false or true positives. All point estimates are used in the simulation regardless of significance.

Table 5: False positives, true positives, colonels and judicial inefficiency, 2000-2010
Municipality-specific trends

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false or true positives)								
	False Positives				True Positives			
Judicial Inefficiency								
... x 2002:1	0.0022	0.0325	−0.0170	0.0072	0.1118	0.3418	0.1242	0.3833
	(0.0299)	(0.0414)	(0.0304)	(0.0420)	(0.1377)	(0.3561)	(0.1359)	(0.3526)
... x 2002:2	−0.0057	0.0099	−0.0228	−0.0128	−0.0200	−0.2789	−0.0091	−0.2392
	(0.0546)	(0.0784)	(0.0549)	(0.0788)	(0.1196)	(0.1909)	(0.1176)	(0.1850)
... x Incentives (2003:1-2008:2)	0.0797***	0.1068***	0.0102***	0.0138***	−0.0572	−0.1224*	−0.0077	−0.0107
	(0.0227)	(0.0301)	(0.0031)	(0.0045)	(0.0449)	(0.0725)	(0.0047)	(0.0071)
Colonel in charge (share)								
... x 2002:1	0.0001	−0.0033	−0.0005	−0.0050	0.0359	0.0217	0.0329	0.0200
	(0.0052)	(0.0073)	(0.0047)	(0.0066)	(0.0335)	(0.0510)	(0.0332)	(0.0505)
... x 2002:2	−0.0091	−0.0204*	−0.0093	−0.0216*	−0.0134	−0.0618	−0.0159	−0.0629
	(0.0069)	(0.0123)	(0.0065)	(0.0119)	(0.0318)	(0.0521)	(0.0315)	(0.0517)
... x Incentives (2003:1-2008:2)	0.0116	0.0193**	0.0019**	0.0027**	0.0243*	0.0249	0.0030**	0.0036*
	(0.0074)	(0.0097)	(0.0008)	(0.0011)	(0.0128)	(0.0187)	(0.0013)	(0.0019)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.097	0.096	0.097	0.096	0.122	0.126	0.122	0.126

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Municipality specific trends are also included. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 6: False and true positives, colonels and judicial inefficiency, 2000-2010
Removing outliers

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false or true positives)								
	False Positives				True Positives			
Judicial Inefficiency								
... x 2002:1	0.0008	−0.0010	0.0079	−0.0060	0.2062	1.0813	0.1867	1.1861
	(0.0060)	(0.0139)	(0.0071)	(0.0164)	(0.1347)	(0.7728)	(0.1372)	(0.7675)
... x 2002:2	0.0035	−0.0002	0.0104	−0.0057	−0.0006	−0.2220	−0.0152	−0.2161
	(0.0064)	(0.0138)	(0.0077)	(0.0166)	(0.1089)	(0.1763)	(0.1092)	(0.1765)
... x Incentives (2003:1-2008:2)	0.0389***	0.0728***	0.0083***	0.0115***	−0.0439	−0.1161	−0.0074	−0.0139
	(0.0120)	(0.0185)	(0.0026)	(0.0034)	(0.0476)	(0.0755)	(0.0062)	(0.0096)
Colonel in charge (share)								
... x 2002:1	0.0019	0.0018	0.0017	0.0013	−0.0070	−0.2902*	−0.0000	−0.2345
	(0.0030)	(0.0028)	(0.0029)	(0.0028)	(0.0304)	(0.1707)	(0.0303)	(0.1571)
... x 2002:2	0.0020	0.0020	0.0018	0.0014	−0.0387	−0.0888	−0.0310	−0.0796
	(0.0030)	(0.0028)	(0.0030)	(0.0028)	(0.0366)	(0.0583)	(0.0363)	(0.0581)
... x Incentives (2003:1-2008:2)	0.0131***	0.0144***	0.0017***	0.0018***	0.0254**	0.0372**	0.0045***	0.0072***
	(0.0045)	(0.0046)	(0.0006)	(0.0007)	(0.0118)	(0.0169)	(0.0013)	(0.0020)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	15995	15995	15995	15995	17734	17653	17715	17648
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.364	0.253	0.363	0.214	0.107	0.104	0.103	0.101

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. Outliers are defined as those observations below the 5th and above the 95th percentile in the distribution of estimation residuals from our baseline regressions. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 7: False positives, colonels and judicial inefficiency, 2000-2010
Controlling for collateral damage

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false positives)								
Judicial Inefficiency								
... x 2002:1	−0.0164	0.0212	−0.0256	0.0105	−0.0143	0.0224	−0.0262	0.0090
	(0.0311)	(0.0440)	(0.0318)	(0.0449)	(0.0316)	(0.0446)	(0.0326)	(0.0458)
... x 2002:2	−0.0154	0.0057	−0.0246	−0.0051	−0.0132	0.0069	−0.0252	−0.0067
	(0.0508)	(0.0762)	(0.0512)	(0.0766)	(0.0511)	(0.0767)	(0.0517)	(0.0772)
... x Incentives (2003:1-2008:2)	0.0885***	0.1170***	0.0105***	0.0142***	0.0907***	0.1183***	0.0104***	0.0141***
	(0.0231)	(0.0306)	(0.0032)	(0.0046)	(0.0238)	(0.0317)	(0.0033)	(0.0047)
... x 2009:1					−0.0030	0.0039	−0.0147	−0.0093
					(0.0248)	(0.0292)	(0.0260)	(0.0308)
... x 2009:2					0.0204	0.0062	0.0087	−0.0070
					(0.0297)	(0.0283)	(0.0307)	(0.0298)
Colonel in charge (share)								
... x 2002:1	−0.0024	−0.0061	−0.0015	−0.0061	−0.0028	−0.0061	−0.0010	−0.0054
	(0.0068)	(0.0092)	(0.0066)	(0.0088)	(0.0069)	(0.0093)	(0.0067)	(0.0089)
... x 2002:2	−0.0085	−0.0204	−0.0076	−0.0204	−0.0089	−0.0204	−0.0072	−0.0197
	(0.0091)	(0.0152)	(0.0089)	(0.0149)	(0.0092)	(0.0152)	(0.0090)	(0.0150)
... x Incentives (2003:1-2008:2)	0.0157**	0.0261***	0.0021***	0.0032***	0.0152**	0.0261***	0.0021***	0.0033***
	(0.0070)	(0.0093)	(0.0008)	(0.0010)	(0.0072)	(0.0096)	(0.0008)	(0.0011)
... x 2009:1					−0.0030	−0.0006	−0.0000	0.0018
					(0.0066)	(0.0080)	(0.0068)	(0.0085)
... x 2009:2					0.0004	0.0007	0.0032	0.0030
					(0.0068)	(0.0076)	(0.0071)	(0.0081)
True Positives Polynomial								
True Positives	−0.0517	0.0161	−0.0493	0.0173	−0.0518	0.0162	−0.0493	0.0173
	(0.1122)	(0.0395)	(0.1123)	(0.0395)	(0.1122)	(0.0395)	(0.1123)	(0.0395)
(True Positives)^2	0.2038	0.0246	0.1981	0.0230	0.2040	0.0246	0.1983	0.0230
	(0.2698)	(0.0579)	(0.2700)	(0.0578)	(0.2698)	(0.0579)	(0.2700)	(0.0578)
(True Positives)^3	−0.1427	−0.0064	−0.1388	−0.0058	−0.1428	−0.0064	−0.1390	−0.0058
	(0.1902)	(0.0242)	(0.1903)	(0.0242)	(0.1902)	(0.0242)	(0.1903)	(0.0242)
(True Positives)^4	0.0370	0.0000	0.0362	−0.0000	0.0370	0.0000	0.0362	−0.0000
	(0.0391)	(0.0028)	(0.0391)	(0.0028)	(0.0391)	(0.0028)	(0.0391)	(0.0028)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.077	0.071	0.078	0.071	0.077	0.071	0.078	0.071

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 8: Judicial inefficiency and colonels, 2000-2010

	Baseline		Post Trends		Municipal Trends	
	Dummy	Linear	Dummy	Linear	Dummy	Linear
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is judicial inefficiency						
Colonel in charge (share)						
... x 2002:1	−0.0330**	−0.0369**	−0.0365**	−0.0410***	−0.0402***	−0.0444***
	(0.0141)	(0.0144)	(0.0143)	(0.0147)	(0.0148)	(0.0153)
... x 2002:2	−0.0008	−0.0047	−0.0043	−0.0088	−0.0068	−0.0110
	(0.0148)	(0.0150)	(0.0149)	(0.0153)	(0.0163)	(0.0168)
... x Incentives (2003:1-2008:2)	0.0164**	0.0011*	0.0125*	0.0007	0.0178**	0.0013**
	(0.0067)	(0.0006)	(0.0071)	(0.0007)	(0.0071)	(0.0006)
... x 2009:1			−0.0146	−0.0188		
			(0.0139)	(0.0141)		
... x 2009:2			−0.0069	−0.0111		
			(0.0100)	(0.0102)		
Controls x time effects	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893
R-Squared	0.026	0.026	0.026	0.026	0.084	0.083

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Columns 5 and 6 include in addition municipality-specific trends. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. Columns 5 and 6 include in addition municipality-specific trends. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 9: Judicial inefficiency and colonels, 2000-2010
Removing outliers

	Baseline		Post Trends		Municipal Trends	
	Dummy	Linear	Dummy	Linear	Dummy	Linear
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is judicial inefficiency						
Colonel in charge (share)						
... x 2002:1	−0.0085	−0.0128	−0.0098	−0.0135	−0.0059	−0.0098
	(0.0096)	(0.0094)	(0.0095)	(0.0095)	(0.0098)	(0.0102)
... x 2002:2	0.0159	0.0117	0.0147	0.0105	0.0193***	0.0154***
	(0.0124)	(0.0124)	(0.0125)	(0.0122)	(0.0027)	(0.0038)
... x Incentives (2003:1-2008:2)	0.0119**	0.0007	0.0109**	0.0006	0.0131**	0.0009*
	(0.0054)	(0.0005)	(0.0054)	(0.0005)	(0.0054)	(0.0005)
... x 2009:1			−0.1502	−0.1372		
			(103.2118)	(100.8766)		
... x 2009:2			−0.0214	−0.0242*		
			(0.0136)	(0.0124)		
Controls x time effects	✓	✓	✓	✓	✓	✓
Observations	16924	16924	16924	16924	17681	17681
Municipalities	893	893	893	893	893	893
R-Squared	0.055	0.054	0.055	0.055	0.133	0.133

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Columns 5 and 6 include in addition municipality-specific trends. Outliers are defined as those observations below the 5th and above the 95th percentile in the distribution of estimation residuals from our baseline regressions. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 10: Guerilla attacks and colonels, 2000-2010

	Baseline		Post Trends		Municipal Trends	
	Dummy	Linear	Dummy	Linear	Dummy	Linear
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is dummy variable for guerilla attacks						
Judicial Inefficiency						
... x 2002:1	0.1305	0.0933	0.1199	0.0775	0.1351	0.0993
	(0.1516)	(0.1505)	(0.1540)	(0.1519)	(0.1317)	(0.1295)
... x 2002:2	0.0408	0.0036	0.0302	−0.0122	0.0445	0.0084
	(0.1206)	(0.1192)	(0.1235)	(0.1208)	(0.1194)	(0.1172)
... x Incentives (2003:1-2008:2)	0.0671	0.0008	0.0565	−0.0009	0.0654	0.0003
	(0.0521)	(0.0050)	(0.0587)	(0.0054)	(0.0482)	(0.0046)
... x 2009:1			−0.0190	−0.0610		
			(0.1164)	(0.1132)		
... x 2009:2			−0.0642	−0.1062		
			(0.0948)	(0.0910)		
Colonel in charge (share)						
... x 2002:1	−0.0295	−0.0282	−0.0221	−0.0213	0.0091	0.0069
	(0.0587)	(0.0582)	(0.0597)	(0.0588)	(0.0547)	(0.0542)
... x 2002:2	0.0299	0.0312	0.0373	0.0382	0.0625	0.0606
	(0.0432)	(0.0424)	(0.0445)	(0.0431)	(0.0427)	(0.0420)
... x Incentives (2003:1-2008:2)	0.0237*	0.0032**	0.0317*	0.0040***	0.0172	0.0021*
	(0.0134)	(0.0012)	(0.0170)	(0.0015)	(0.0124)	(0.0011)
... x 2009:1			0.0235	0.0264		
			(0.0221)	(0.0205)		
... x 2009:2			0.0204	0.0231		
			(0.0212)	(0.0199)		
Controls x time effects	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893
R-Squared	0.073	0.073	0.073	0.073	0.159	0.159

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Columns 5 and 6 include in addition municipality-specific trends. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 11: Paramilitary attacks and colonels, 2000-2010

	Baseline		Post Trends		Municipal Trends	
	Dummy	Linear	Dummy	Linear	Dummy	Linear
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is dummy variable for paramilitary attacks						
Judicial Inefficiency						
... x 2002:1	−0.0750	−0.0601	−0.0818	−0.0616	−0.0853	−0.0679
	(0.1060)	(0.1039)	(0.1096)	(0.1053)	(0.1143)	(0.1109)
... x 2002:2	−0.0748	−0.0599	−0.0816	−0.0614	−0.0835	−0.0662
	(0.0731)	(0.0701)	(0.0786)	(0.0725)	(0.0762)	(0.0713)
... x Incentives (2003:1-2008:2)	−0.0363	−0.0015	−0.0430	−0.0016	−0.0346	−0.0006
	(0.0407)	(0.0036)	(0.0498)	(0.0041)	(0.0377)	(0.0033)
... x 2009:1			−0.0595	−0.0392		
			(0.0607)	(0.0526)		
... x 2009:2			0.0100	0.0303		
			(0.0776)	(0.0713)		
Colonel in charge (share)						
... x 2002:1	−0.0132	−0.0156	0.0012	−0.0051	0.0163	0.0123
	(0.0233)	(0.0229)	(0.0237)	(0.0231)	(0.0222)	(0.0217)
... x 2002:2	0.0194	0.0169	0.0337*	0.0274	0.0445*	0.0405*
	(0.0198)	(0.0194)	(0.0204)	(0.0195)	(0.0237)	(0.0232)
... x Incentives (2003:1-2008:2)	0.0191**	0.0018***	0.0347***	0.0030***	0.0169**	0.0013*
	(0.0077)	(0.0007)	(0.0096)	(0.0008)	(0.0076)	(0.0007)
... x 2009:1			0.0396***	0.0348***		
			(0.0114)	(0.0108)		
... x 2009:2			0.0465***	0.0416***		
			(0.0152)	(0.0148)		
Controls x time effects	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893
R-Squared	0.077	0.077	0.078	0.078	0.159	0.158

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Columns 5 and 6 include in addition municipality-specific trends. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 12: Government attacks and colonels, 2000-2010

	Baseline		Post Trends		Municipal Trends	
	Dummy	Linear	Dummy	Linear	Dummy	Linear
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is dummy variable for government attacks						
Judicial Inefficiency						
... x 2002:1	0.0445	0.0335	0.0278	0.0198	0.0433	0.0341
	(0.0943)	(0.0936)	(0.0964)	(0.0948)	(0.0964)	(0.0950)
... x 2002:2	−0.0034	−0.0144	−0.0201	−0.0281	−0.0044	−0.0139
	(0.0714)	(0.0704)	(0.0742)	(0.0720)	(0.0746)	(0.0729)
... x Incentives (2003:1-2008:2)	0.0122	−0.0008	−0.0045	−0.0023	0.0122	−0.0009
	(0.0348)	(0.0031)	(0.0401)	(0.0035)	(0.0341)	(0.0031)
... x 2009:1			−0.0258	−0.0339		
			(0.0568)	(0.0540)		
... x 2009:2			−0.1091*	−0.1173**		
			(0.0605)	(0.0579)		
Colonel in charge (share)						
... x 2002:1	−0.0258*	−0.0279*	−0.0284*	−0.0308**	−0.0180	−0.0191
	(0.0153)	(0.0152)	(0.0156)	(0.0154)	(0.0157)	(0.0156)
... x 2002:2	−0.0150	−0.0172	−0.0177	−0.0201	−0.0086	−0.0098
	(0.0186)	(0.0184)	(0.0188)	(0.0186)	(0.0180)	(0.0179)
... x Incentives (2003:1-2008:2)	0.0006	−0.0004	−0.0023	−0.0007	−0.0021	−0.0008
	(0.0065)	(0.0006)	(0.0076)	(0.0007)	(0.0066)	(0.0006)
... x 2009:1			−0.0073	−0.0101		
			(0.0088)	(0.0087)		
... x 2009:2			−0.0093	−0.0121		
			(0.0125)	(0.0124)		
Controls x time effects	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893
R-Squared	0.062	0.062	0.062	0.062	0.118	0.118

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Columns 5 and 6 include in addition municipality-specific trends. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

A Appendix

A.1 Context and case-study evidence

In Section 2 of the paper we gave a brief sketch of the Colombian context and some of the key pieces of evidence which undergird our approach. Here we present some additional case study evidence which supports our arguments and interpretations of the evidence.

A.2 The introduction and removal of high-powered incentives

After the Soacha scandal mentioned in the text, *Semana* and other media outlets published a number of documents recently declassified by US intelligence, all of which highlighted army incentives creating the conditions for the emergence of human rights abuses. Some of these dated back to the 1990s.[32] Despite these early concerns, the later increase in false positives in the 2000s was unprecedented. The ensuing national scandal also led to an investigation from a United Nations Special Rapporteur, Phillip Alston, to an internal investigation by the armed forces, and to the ousting of a number of army members, including high-ranking officials.

As noted in Philip Alston's final report on the issue, while the existence of different sorts of incentives is clear, it is to some extent unclear how rewards for killings worked since this was informal in many ways. While critics argue that members of the armed forces receive money, holidays, medals, and promotions for killing guerillas, the government has pointed out that rewards (like those established in Directive 29) cannot be paid to public servants like soldiers. Nonetheless, as the case-study evidence below reveals and as was recognized by Alston and judicial investigations, this theoretical principle was not always true in practice.

First, based on his investigations Alston noted that even if not receiving money "members of the military have also been provided various incentives to kill, including vacation time, medals, and promotions" (Alston, 2010, p. 11). Human Rights Watch point out that army members colluded with potential recruiters for false positives to share the monetary rewards. Moreover, other sources of payment in the form of "gastos reservados" (confidential expenses) and commanders' discretionary funds were used as rewards. Referring to this sources, Alston notes that the Government "conceded that there is more discretion for officers in distributing confidential expenses, and that 'there could be problems there"' (Alston, 2010, p. 10). These observations explain his conclusion already reproduced in the main text that "There were incentives: an informal incentive system for soldiers to kill, and a formal one for civilians who provided information leading to the capture or killing of guerillas. The latter system lacked oversight and accountability" (Alston, 2010, p. 2).

There is some controversy over whether formal directives like 29 of 2005 were directly responsible for false positives or not. But there is little doubt among experts and members of the Office of the General Attorney consulted that incentives were delivered and played a role. These directives leaked by the press can therefore be taken more as a signal of a general effort by the army to provide direct incentives for killing guerillas than as an exact description of how incentives worked

[32]For instance, in a 1994 report, US Ambassador Myles Frechett says that Colombia's Defense Minister Fernando Botero's statements, referring to the growing awareness within the military on the importance of human rights protection and the blocking of promotions to officers suspected of having been involved in abuses, were 'wishful thinking'. Instead, the ambassador claims that a 'body count' mentality is widespread among the Colombian military, *and a necessary condition for promotion.* Another document quotes a Colombian colonel commenting in 1997 that there was a "body count syndrome" in the army, responsible for "fueling human rights abuses by well-meaning soldiers that just try to get their quota to impress superiors".

in practice. This policy, as Alston reports, was reflected not just in the formal policies adopted, but in informal and unregulated incentives. The pressure to "show results" and rewards of doing so is cited by experts, even within the military, as one of the causes of false positives. A soldier explained a killing by his unit would be rewarded with 15 days vacation. "When important holidays approached, he stated, soldiers would attempt to 'earn' vacation time" (Alston, 2010, p. 11).

In the end, a full incentives scheme was in place, that included the expectation of money, vacation and promotions for army members and commanders capable of producing more killings of rebels. In line with a long tradition in Colombia of a body-count mentality, these incentives exacerbated the idea that only army commanders "successful" in the fight against insurgency using this metric were likely to rise up in the military ladder.

Perhaps the most clear indication of the importance of incentives in explaining false positives is the governments' reaction to the media uncovering of the Soacha killings. In a special September 2008 report following the scandal, the government discussed the achievements of President Uribe's flagship Democratic Security Policy, but acknowledged concerns around the persistent complaints of false positive cases. Moreover, when discussing its efforts to avoid false positives, it acknowledges that some measures had already been taken to adjust, precisely, the incentive policy. In particular, it mentions Directive 10 of 2007[33] which "reiterates the obligation of authorities to enforce the law and avoid homicides of protected persons" and created a committee to investigate complaints. In November 2007, this directive was complemented with a second one emphasizing that army commanders should ensure deaths in combat to be first investigated by the judicial police. Yet a third directive, 300-28 of November 2007, was aimed at prioritizing rewards for demobilizations and rescuing hostages rather than killings. In May 2008, Directive 142 changed the criteria for awarding medals (the *medalla al valor* and *medalla de orden público*). According to the report, starting with this directive demobilizations and capturing members of criminal and illegal armed groups are valued "as much or more" than killings (Government of Colombia, 2008)[34].

The 2005 directive 29 was later modified by directives 02 of 2008 and 01 of 2009. All of these are confidential, and only the first one was widely circulated in the press. Nevertheless, reports based on government information such as commissioner Alston's final report indicate that later directives toughened controls and sought to make it harder to use monetary incentives for false positives. In particular, they explicitly excluded the payment of rewards to army members and required that operations have the support of *prior* intelligence and included more controls on supporting documents. Other measures, while not influencing rewards directly, did affect the perceived consequences of producing false positives. Indeed, the government took disciplinary actions, ousting high-ranking officials involved in possible false positives. It also created a specialized unit in the Office of the Attorney General (*Fiscalía*) to investigate the crimes.

A.3 Case studies

We now turn to a more careful description of some of the false positives cases on which there is information. The information comes from two main sources. First, for closed criminal cases on which we are able to access information on trial hearings and sentences (for open cases this information is reserved). Second, from secondary sources, mainly the press and NGO's investigating

[33]Issued on june 6 of 2007, and made available at `http://web.presidencia.gov.co/especial/ddhh_2009/Directivas_ddhh.pdf`.

[34]We have not had direct access, however, to this secret directive.

specific cases, we also have some information even for cases on which there is no definite sentence yet.

Our emphasis is on the role of incentives when explaining false positives, and in particular how these interacted with two main factors: first, a weak judicial system which made army members believe they could "get away" with the killings of civilians; second, the stronger incentives faced by colonels to produce false positives in search for promotion. We also discuss the incentive of the military to further erode the quality of judicial institutions, to facilitate committing these crimes. We further emphasize that the case study evidence is not consistent with false positives being simply the result of collateral damage, an unfortunate by-product of genuine combat activities.

A.3.1 The role of incentives

The case study evidence that we report in this section shows that the incentive package given to military personnel who excelled in the production of quantifiable counterinsurgency results included rewards, permits of absence or vacation time, honors and compliments from superiors, and promotions. In addition to the 'carrots', there were also sticks in the form of high pressure from superior officials to produce 'results' (in the form of killings) and penalties for soldiers who failed to do so. Indeed, several army officials who have been interviewed by the press after the scandal broke out have stated that the psychological torture of having to deliver operational results every day was unbearable.[35]

One example of such pressure for results and punishment of failures can be seen in army official Edgar Iván Flórez Maestre's statement during his hearing before the General Prosecutor. According to Flórez, 14^{th} Brigade's commander Colonel Wilson Cedeño used to tell his troops: "Each company commander is responsible of one combat death per month, and the Second Section is responsible for three deaths per month. At this time war is measured with liters of blood. The commander that cannot show results in terms of deaths every month will face a sanction that will appear in his folder." Flórez also stated that colonel Juan Carlos Barrera Jurado, former commander of the 14^{th} Brigade, once told all battalion commanders under his orders that the battalions that did not have any killings in combat in the next 90 days would have their commanders fired for negligence and operational lack of capacity. According to Flórez, the pressure was so intense that soldiers would start counting the days that they had not faced combat. The excess pressure finally resulted in misbehavior. According to Flórez a fellow soldier once told him how frustrated he was that the only people that were getting permits of leave and honors were the ones that were producing killings, and so that he was planning a "job" for which he had already obtained a gun (to put on the victim's hands to make him appear as a combatant).[36]

Major Juan Carlos Rodríguez Agudelo tells a similar story in an interview with newspaper *El Tiempo*. According to Rodríguez, while back in 1995 an honor medal was given for two combat kills, by 2004 the threshold had gone up to 10 kills. In staff operational meetings, commanders who had produced many killings were praised and those who could not show enough bodies were ridiculed by superiors. Rodríguez argues that wearing one of these honor medals had such high status within the army that the pursuit of glory pushed him to make mistakes and he ended up

[35] "Cada día se van unos 17 hombres del Ejército", *El Tiempo*, July 2, 2006. Available at: `http://www.eltiempo.com/archivo/documento/MAM-2087862` (last accessed August 14, 2014).

[36] Source: Hearing of Edgar Iván Flórez Maestre before the Unit of Human Rights of the National Direction of Special Investigations of the General Prosecutor Office, Medellín, December 15, 2009.

killing civilians.[37]

Human Rights Watch (2015) presents a great deal of supportive evidence. For example, they cite retired Lieutenant Colonel Robinson González del Río as giving testimony that General Mario Montoya, the army's top commander between February 2006 and November 2008 "Pressured subordinate commanders to increase body counts, punishing them for failing to do so" (p. 4). Other testimony by army personnel suggested that Montoya "organized competitions between military units over the number of reported combat kills" (p. 27). González del Río told prosecutors that "you were evaluated based ... on combat kills" (p. 27).

In addition to pressure and threats, 'positive' incentives played a major role. This is acknowledged by official Edgar Iván Flórez Maestre, in the same hearing mentioned above, when stating that one of the incentives offered to commanders of all battalions was vacation for the entire month of December for the platoon that could show the highest number of killings in a given year. In addition, the soldier that perpetrated the highest number of killings would be sent to Sinai, or a course out of the country.[38]

In 2007, Sargent Alexander Rodríguez Sánchez reported to the authorities (the offices of the Attorney General and the General Prosecutor, and even the Army Command) that his unit, mobile Brigade 15, was engaging in unlawful assassinations of civilians in the department of Norte de Santander. Sargent Rodríguez reported that fellow soldiers that killed civilians and portrayed them as guerillas killed in combat were granted a five-day vacation period per casualty produced. His testimony accuses the unit commander, Colonel Santiago Herrera Fajardo, of having pressured battalion commanders to generate results. According to Rodríguez this was because the Army commander in chief, General Mario Montoya, was himself putting pressure on Herrera and other brigade commanders.[39] The accusation was investigated by a military committee headed by Montoya. The result was that Rodriguez was fired from the army while Colonel Herrera was promoted because of the operational results of his unit.[40]

This is not the only testimony that relates commander in chief Montoya with putting pressure from above to unit commanders to produce results, in particular killings, as we noted above. In an interview with TV magazine *Noticias RCN,* Colonel Robinson González del Rio states that he once heard General Montoya saying "I want rivers of blood, I want results", and that he made famous a "top 10" ranking of units according to the results produced as measured by killings.[41]

[37] "Oficial del Ejército admite cómo participó en 'falsos positivos'", *El Tiempo*, June 3, 2012. Available at: `http://www.eltiempo.com/archivo/documento/CMS-11918454` (last accessed August 14, 2014).

[38] Colombia is part of The Multinational Force and Observers (MFO), an international peacekeeping force that operates in the Sinai peninsula overseeing the 1979 peace treaty between Egypt and Israel.

[39] The lack of results of Brigade 15 that generated this pressure may have been a key factor triggering the Soacha killings. A witness in the Soacha case investigations, Sargent Muñoz, declared that after several reprimands from higher level officials, the brigade commander, Colonel Gabriel Rincón Amado agreed to "buy" civilians (from intermediaries who would recruit them for fake jobs) and present them as enemies killed in combat. This is how civilians Jhonnatan Orlando Soto (17) and Julio César Meza (24) disappeared from Soacha after accepting job offers from the recruiter. They were killed two days afterwards. The recruiters were payed 2.2 million pesos (just over US$ 1,000). After that, according to Sargent Muñoz, Colonel Rincón wanted to repeat the operation with more people ("Así se tejió la trampa de los falsos positivos", *El Tiempo*, May 24, 2009. Available at: `http://www.eltiempo.com/archivo/documento/MAM-3456789` (last accessed August 15, 2014)).

[40] Sources: Beriain, David "A su muchacho lo matamos nosotros, señora" *Agencia de Prensa Rural*, May 24. Available at: `http://prensarural.org/spip/spip.php?article1124` (last accessed August 14, 2014); and "Primer militar que denunció 'falsos positivos' en Norte de Santander está preso", *El Tiempo*, May 10, 2009. Available at:`http://www.eltiempo.com/archivo/documento/CMS-5177467` (last accessed August 14, 2014).

[41] "General Montoya responde a denuncias del coronel Del Río", *Semana.com*, June 9, 2014. Available at: `http://`

This is consistent with the concerns expressed by the US Embassy in Bogotá, in a cable filtered by Wikileaks, according to which General Montoya picked General Oscar Enrique González Peña as his successor as commander in chief of the military forces, praising him as "the best commander in the country" during his tenure in charge of the 4^{th} Brigade, because his unit reported the most killings of all: 857.[42] Finally, in the cited interview with *El Tiempo*, Major Juan Carlos Rodríguez Agudelo said that the instructions from above, all the way to General Montoya, first produced "bottles of blood" and that ended in "tanker trucks" of blood. Indeed, according to Major Rodríguez, soldiers that did not have any deaths in their history were "out of the system", and captures just did not count.[43]

This suggests that while the cited formal documents emphasized both captures of insurgents and their death as acceptable outcomes, the informal incentives privileged killings over captures. In another telling example of his interview with *El Tiempo*, Major Rodríguez says that a common situation was one in which a soldier would call a superior to report, say, two killings and three captures, and the superior would reply by saying that he was now calling the local Attorney representative for him to remove all the five corpses, making clear that he expected the soldier to kill the three insurgents who had been captured.

Another incentive used to persuade army members to engage in this practice was the direct payment of rewards. Even if the Directive 029 and other of the cited documents do not mention military personnel as potential recipients of the money that the government had budgeted for intelligence rewards, army units designed mechanisms to allow for funds to be transferred to their soldiers if they were successful at producing results. According to Colonel Luis Fernando Borja, in addition to vacation and honors, soldiers could obtain cash. To this end, units would "create" fake informants that upon receiving the rewards would pass it to the command to distribute it discretionally among soldiers. Colonel Borja confessed he himself managed these funds in his unit.[44] Alfamir Castillo, mother of a false positive victim, Darvey Mosquera, told news web magazine *La Silla Vacía* that she had proof that each of the soldiers of Counter-guerilla battalion Mártires de Puerres, involved in the killing of her son and that of his friend, Alex Hernando Ramírez, received about 3 million pesos in addition to a one-month permit of absence. The two victims were portrayed as insurgents killed in combat. The Human Rights Watch report cites several other instances of this.

`www.semana.com/nacion/articulo/general-montoya-responde-denuncias-del-coronel-del-rio/391036-3` (last accessed August 14, 2014).

[42]The Wikileak cable can be downloaded from: `http://wikileaks.ch/cable/2008/11/08BOGOTA4028.html` (last accessed August 14, 2014).

[43]There are testimonies that involve the then Minister of Defense and current President, Juan Manuel Santos, in exerting pressure to commanders to produce results and threatening punishment to under-performers. However, in sharp contrast with the case of General Montoya, Santos does not appear to be asking for killings, just vaguely for 'results'. In his hearing before the Attorney General, Colonel Luis Fernando Borja Aristizabal, former commander of the Joint Task Force of Sucre, states that when Santos visited the area to preside over a Security Council, he addressed Borja and warned him that he needed to achieve measurable quantitative results, or else he would be fired. Borja, who confessed to having perpetrated 57 false positives and is now facing a 42-year sentence, told the attorney that he felt threatened ("El Coronel que confesó 57 falsos positivos", *KienyKe*, 30 August, 2011. Available at: `http://www.kienyke.com/historias/el-coronel-que-confeso-57-falsos-positivos/` (last accessed August 14, 2014)).

[44]"Soy culpable", *Semana*, July 16, 2011. Available at: `http://www.semana.com/nacion/articulo/soy-culpable/243091-3` (last accessed August 15, 2014.

A.3.2 Oversight by local judicial institutions

Recall that one of our predictions is that, for a given reward structure, cheating is more likely if it is easier to falsify. This highlights the importance of an independent an efficient legal system. As noted by UN Special Rapporteur, Phillip Alston, in his 2010 report:

> "Lack of sufficient accountability has been a key factor in the continuation of *falsos positivos*. Estimates of the current rate of impunity for alleged killings by the security forces are as high as 98.5%. Soldiers simply knew that they could get away with murder. This resulted from problems (...) at each stage of the investigation and disciplinary or criminal justice system" (p. 12).

In Colombia, the local branch of the Office of the Attorney General and its Technical Investigation Unit (CTI) are in charge of the initial inspection and removal of the corpse after a killing in combat, and local attorneys are the key investigators of alleged false positives. When an accusation is issued, the case and the available evidence are transferred to the local judges, who conduct hearings and gather additional evidence. If one of these branches of the judiciary is corrupt or just inefficient, the incentives to commit abuses in their jurisdiction is higher. These can take the form of delays in investigations, harassment, threatening and even killing key witnesses, etc.

There is also evidence that the weakness of the judicial system facilitated these crimes. Moreover, the anecdotal evidence that we review in this subsection also offers several examples of the resulting incentive, for army members, to further corrupt the judicial system in order to get away with the murder of civilians.

As a telling example, according to magazine *Semana*, Colonel Publio Hernán Mejía, former commander of "La Popa" battalion and now jailed for his links with paramilitaries and for committing extrajudicial executions of civilians, had little trouble in producing in false positives because the local representative of the Attorney General helped him with the setups necessary to 'legalize' the victims as insurgents.[45] Similarly, in his testimony, Captain Antonio Rozo Valbuena, former commander of the GAULA special operations unit working in the department of Córdoba, stated that local representatives of the Attorney General Office helped the unit 'legalize' the execution of civilians.[46] More generally, according to the International Federation of Human Rights, in various instances there was evidence of proximity and collaboration between local attorneys and the military unit that operates in the area, with a few cases in which the attorneys even worked inside military garrisons.[47]

There is also indirect evidence of collaboration between local attorneys and the army when it comes to cases of false positives. According to the UN's Office of the High Commissioner for Human Rights, in several instances of illegal executions of civilians, the local office of the attorney refrained from claiming the competence of removing the corpses and initiating a judicial investigation. In these circumstances, cases are referred to the military criminal justice system, largely accused

[45] "De héroe a villano", *Semana*, January 27, 2007. Available at: `http://www.semana.com/nacion/articulo/de-heroe-villano/83183-3` (last accessed August 15, 2014).

[46] "Confesiones siniestras", *Agencia Prensa Rural*, October 10, 2011. Available at: `http://prensarural.org/spip/spip.php?article6588` (last accessed August 15, 2014).

[47] "Colombia. La guerra se mide en litros de sangre" - 2012 Report of the International Federation of Human Rights and the Colombia-Europe-USA Coordination. Available at: `http://www.fidh.org/IMG/pdf/colombie589e.pdf` (last accessed August 15, 2014).

of underplaying the importance of false positives.[48] Human Rights Watch document several cases where functionaries of the military judicial system helped cover up false positives even giving advice to soldiers as to how to make them look more 'realistic' (pp. 77-81).

As a consequence, the UN report conclusion that witnesses were not only afraid of the perpetrators, but also of the local attorneys and prosecutors, since they were believed to cooperate with the perpetrators. This was especially so in the most rural and remote areas. For instance, a human rights activist working in the Casanare region, told *La Silla Vacía* that it was the case that when people approached the authorities to report a disappearance of a family member, the next victims were themselves. Thus "a culture of silence was created".[49]

As already mentioned, after the initial inspection and removal of the corpse by local attorneys, the investigations of alleged false positives are conducted by the judges who have jurisdiction in the area where the crime is perpetrated. However, this practice is sometimes detrimental for the success of investigations. Indeed, in cases in which the victim is abducted or disappears in one place but is executed in another, there is often a dispute of legal responsibility between the judges of the two jurisdictions. Because such disputes take long to resolve (up to a year in some cases), it is not uncommon that in the meantime parts of the evidence get lost and witnesses cannot be found or their memories have conveniently changed. This was frequently the case in the investigation of false positives.[50]

This dual responsibility among judges of different jurisdictions also created incentives for the implicated parties to push for the investigations to end up in one place rather than the other. For instance the defense lawyers of the military members involved in the Soacha scandal formally asked for the cases to be transferred from the regular criminal system to the military criminal justice system. When the petition was denied, the attorneys requested for the cases to be transferred from Soacha, where the victims were recruited, to Norte de Santander, where the victims were killed.[51] This is quite a telling example, as the false positives in Soacha, which created the major media scandal around the phenomena, were perhaps particularly salient precisely because they occurred near the capital city of Bogotá, where state institutions including the judiciary are presumably stronger. Consistent with such situation, these victims were not actually executed near the recruitment cite as in most cases, but were taken far away before being killed.

Other evidence directly suggests that committing false positives likely led to a deterioration of institutional quality. Human Rights Watch (2015, p. 75) report a revealing transcript obtained by the newsmagazine Semana of a phone conversation in 2012 between the then-head of the army General Leonardo Barrero and Lieutenant Colonel González del Río who at the time was being investigated under arrest for his involvement in false positives. Barrero tells González del Río to "create a mafia" to discredit prosecutors. In order to perpetuate false positives there is also evidence that the army paid criminal organizations to find victims. We noted this in the Soacha

[48] 2005 Report of the High Commissioner for Human Rights on the human rights situation in Colombia, UN Document No E/CN 4/2006/009.

[49] "La batalla cotidiana en Casanare por la verdad de los falsos positivos", *La Silla Vacía*, November 5, 2011. Available at: `http://lasillavacia.com/historia-invitado/24106/kristina-johansen/la-batalla-cotidiana-en-casanare-por-la-verdad-de-los-fals` (last accessed August 15, 2014).

[50] "Colombia. La guerra se mide en litros de sangre" - 2012 Report of the International Federation of Human Rights and the Colombia-Europe-USA Coordination. Available at: `http://www.fidh.org/IMG/pdf/colombie589e.pdf` (last accessed August 15, 2014).

[51] "Los secretos de los expedientes de falsos positivos", *El Tiempo*, October 18, 2009. Available at: `http://www.eltiempo.com/archivo/documento/MAM-3674086` (last accessed September 20, 2014).

case and González del Rio also testified that when he had commanded the Gaula Antioquia unit of the 4th Brigade General González Peña, commanded of the joint Caribbean Command, "suggested he work with organized crime groups to commit false positives" (Human Rights Watch, 2015, p. 68). It is likely that things such as attacks on witnesses and threats also help to undermine local institutions (Human Rights Watch, 2015, p. 74).

A.3.3 Promotion incentives for Colonels

In our main theoretical and empirical analysis, we posited that the body count incentives that generated the surge in false positives after 2002 were stronger for colonels than for generals. Our empirical strategy builds on the idea that colonels leading brigades are, unlike generals, up for promotion. Therefore, high-powered incentives are more likely to have an effect on their behavior.

Recall for instance the example of Sargent Alexander Rodríguez, who testified to having witnessed the assassination of several civilians by his unit. However, after blaming the unit's colonel for orchestrating the killings, he had his testimony reviewed by a military board headed by General Mario Montoya. The outcome of the investigation was that Sargent Rodríguez was fired from the force, and the involved colonel was promoted.[52] In addition, Alfamir Castillo, mother of false positive victim Darvey Mosquera, told news web magazine *La Silla Vacía* that even if several soldiers were already serving jail time for the killing of her son, the officials that ordered the killing and organized for him to be portrayed as a guerilla were still free. The officials are Brigadier Generals Emiro José Barrios and Jorge Enrique Navarrete, both of whom were colonels at the time of the events.[53,54]

There are several accusations of colonels, acting as unit commanders, for orchestrating and perpetrating false positives. All of them share common features, for instance that colonels' main motive was to be promoted. For instance, according to magazine *Semana*, 27 soldiers, an entire platoon, were expelled from the army in 2008 for refusing to fire at two alleged guerilla members, but who were dressed as civilians and not engaging in combat. The platoon had seen two individuals in a guerilla camp site and, as one of them said, "it would have been easy to shoot and kill them, but they were unarmed and dressed as civilians". Moreover, they couldn't approach the camp for fear of land mines. When moving, however, the guerilla members in the area noticed the military presence and engaged in combat. The guerilla members escaped, but the soldiers captured one underage female guerilla member and confiscated provisions and computers. Upon returning to their battalion with what they considered a positive outcome, the reaction of the platoon commander (a Lieutenant Colonel) is described by one of the soldiers as follows:

[52] Beriain, David "A su muchacho lo matamos nosotros, señora" *Agencia de Prensa Rural*, May 24. Available at: `http://prensarural.org/spip/spip.php?article1124` (last accessed August 14, 2014).

[53] "Entiendo a los soldados a pesar de que mataron a mi hijo. Cumplían órdenes", *LaSillaVacia.com*, September 19, 2013. Available at: `http://lasillavacia.com/historia/entiendo-los-soldados-pesar-de-que-mataron-mi-hijo-cumplian-ordenes-alfamir-castillo-45670` (last accessed September 20, 2014).

[54] This particular case is peculiar because it became known that the insurgent guerillas killed in combat were civilians because of two key witnesses. One was José Didier Marín, one of the three civilians targeted by the army, who happened to escape before they shot him. The other one was Ernesto Quintana, a soldier of the involved unit (Counter-guerilla Battalion Mártires de Puerres), who recognized his cousin was one of the victims when the perpetrators triumphantly showed the photo album of the alleged combat. When he asked his superiors why his civilian was cousin among the "insurgents" killed, they offered vacations and money for him to remain silent. When he left the battalion to take the promised vacation, he was warned that Major Linares had given orders to kill him. Both witnesses are now part of the government's witness protection program.

> "When my colonel came in he started insulting us and scolding us and told us that we were good for nothing, that we did not understand that the live guerilla insurgent was useless for him, and that what mattered were killings because he was going to be promoted to colonel and he was "measured" that way. He told us he was going to have us all expelled."[55]

A.3.4 The Issue of Collateral Damage

It is important to note that the case study literature also decisively suggests that false positives cannot be interpreted as simple collateral damage which occurs as an unfortunate by product of taking the fight to the guerillas. This is because the evidence is overwhelming that this was a planned criminal operation by the army. Human Rights Watch interviews with military officers confirmed that units had "systems in place for committing false positives" (p. 6) and officers would "meet with their battalion commander on a weekly basis to plan false positives" (p. 6) moreover "the crimes required significant organizing, planning, and logistical coordination by military officers and soldiers" (p. 25).

Evidence against the collateral damage hypothesis also comes from the copious judicial and media evidence. Human Rights Watch (p. 25) quotes a 2012 report from the International Criminal Court that sums up a large body of this evidence by saying that false positives "were directed against particular categories of civilians, who resided in remote areas and were considered to belong to a marginalized sector of the population". Human Rights Watch goes on to note that victims "included farmers, children, unemployed people, homeless people, people ... dependent on drugs, people with mental disabilities, community leaders, people with criminal records, petty criminals. Demobilized guerillas and paramilitaries, and in some rare cases, supposed guerilla collaborators or guerillas who had been detained or surrendered" (p. 25). Thus committing false positives was a purposeful activity which was not a natural side effect of engaging in actual combat with guerillas. This feature also suggests that it probably substituted for such actual combat and what we have termed true positives.

A.4 Theoretical results for the perfect substitutes and perfect complements cases

First consider the perfect complements case, where

$$\Psi(a_T, a_F) = \frac{1}{2} \min \left\{ c_T a_T^2, c_F a_F^2 \right\}.$$

Then, after setting $\frac{a_T}{a_F} = \sqrt{\frac{c_F}{c_T}}$ to minimize costs and substituting in the objective function, we obtain the following optimal efforts:

$$a_F^* = \pi s \left[\frac{1 + \alpha\chi}{\sqrt{c_F c_T}} + \frac{\alpha}{c_F} \right] \tag{A.1}$$

$$a_T^* = \pi s \left[\frac{1 + \alpha\chi}{c_T} + \frac{\alpha}{\sqrt{c_F c_T}} \right] \tag{A.2}$$

[55] "La historia inédita de los falsos positivos", *Semana*, July 6, 2013. Available at: `http://www.semana.com/nacion/articulo/la-historia-inedita-falsos-positivos/349851-3` (last accessed September 20, 2014).

Proposition A.1. ***(False positives with perfect technological complements)*** *Suppose that false and true positives are perfect technological complements, with* $\Psi(a_T, a_F) = \frac{1}{2}\min\left\{c_T a_T^2, c_F a_F^2\right\}$. *Then, a marginal increase in incentives s:*

1. *Increases true and false positives.*
2. *Leads to a larger increase in true and false positives where reported output is a more important part of compensation (higher* π*).*
3. *Leads to a larger increase in true and false positives where misrepresentation of false positives is more likely (higher* α*)*

Proof. All results follow directly from expressions (A.1) and (A.2) together with expressions (6) and (7) in the main text for $E[\exp(q_T^*)]$ and $E[\exp(q_F^*)]$. □

Now consider the perfect substitutes case, where $\delta = \sqrt{c_T c_F}$. Then, we can write the first order conditions for maximization of the agent's payoff in complementary slackness form as follows:

$$\begin{aligned} \pi s\alpha - \sqrt{c_F}\left(\sqrt{c_F}a_F + \sqrt{c_T}a_T\right) &\leq 0, \left[\pi s\alpha - \sqrt{c_F}\left(\sqrt{c_F}a_F + \sqrt{c_T}a_T\right)\right] a_F = 0 \\ \pi s(1+\alpha\chi) - \sqrt{c_T}\left(\sqrt{c_F}a_F + \sqrt{c_T}a_T\right) &\leq 0, \left[\pi s(1+\alpha\chi) - \sqrt{c_T}\left(\sqrt{c_F}a_F + \sqrt{c_T}a_T\right)\right] a_T = 0 \\ a_F &\geq 0, a_T \geq 0 \end{aligned} \tag{A.3}$$

Therefore, we cannot have both $a_F > 0$ and $a_T > 0$ except in a borderline case. More specifically:

$$(a_T^*, a_F^*) = \begin{cases} (0, \frac{\pi s\alpha}{c_F}) & \text{if } \frac{\alpha}{\sqrt{c_F}} > \frac{1+\chi\alpha}{\sqrt{c_T}} \\ (\frac{\pi s(1+\chi\alpha)}{c_T}, 0) & \text{if } \frac{\alpha}{\sqrt{c_F}} < \frac{1+\chi\alpha}{\sqrt{c_T}} \\ (a_T, a_F) \geq \mathbf{0} : \sqrt{c_F}a_F + \sqrt{c_T}a_T = \pi s B & \text{if } \frac{\alpha}{\sqrt{c_F}} = \frac{1+\chi\alpha}{\sqrt{c_T}} \equiv B \end{cases} \tag{A.4}$$

This is enough to establish our main results.

Proposition A.2. ***(False positives with perfect technological substitutes)*** *Suppose that false and true positives are perfect technological substitutes, with* $\delta = \sqrt{c_t c_F}$.

Then, agents specialize in one kind of effort ($a_T^* > 0$ *and* $a_F^* = 0$ *or* $a_T^* = 0$ *and* $a_F^* > 0$*) except if* $\frac{\alpha}{\sqrt{c_F}} = \frac{1+\chi\alpha}{\sqrt{c_T}} \equiv B$, *when any pair* $(a_T, a_F) \geq \mathbf{0}$ *such that* $\sqrt{c_F}a_F + \sqrt{c_T}a_T = \pi s B$ *is optimal.*

A marginal increase in incentives s:

1. *(Weakly) increases observed false and true positives,*

$$\frac{\partial \mathbb{E}[\exp(q_F^*)]}{\partial s} \geq 0, \quad \frac{\partial \mathbb{E}\exp(q_T^*)]}{\partial s} \geq 0.$$

Moreover $\frac{\partial \mathbb{E}[\exp(q_T^*)]}{\partial s} = 0 \Leftrightarrow a_T^* = 0$ *(the agent specializes in bad effort), and* $\frac{\partial \mathbb{E}[\exp(q_F^*)]}{\partial s} = 0 \Leftrightarrow a_F^* = \chi = 0$ *(the agent specializes in good effort and false positives are purely intentional).*

2. *Leads to a (weakly) larger increase in false and true positives where reported output is a more important part of compensation (higher π),*

$$\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \pi} \geq 0, \quad \frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \pi} \geq 0.$$

Moreover $\frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \pi} = 0 \Leftrightarrow a_T^* = 0$, *and* $\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \pi} = 0 \Leftrightarrow a_F^* = \chi = 0$.

3. *Leads to a (weakly) larger increase in false and true positives where misrepresentation of false positives is more likely (higher α),*

$$\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \alpha} \geq 0, \quad \frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \alpha} \geq 0.$$

Moreover $\frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \alpha} = 0 \Leftrightarrow a_T^* = 0$, *and* $\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \alpha} = 0 \Leftrightarrow a_F^* = \chi = 0$.

Proof. All implications follow directly from (A.4) combined with expressions (6) and (7) in the main text for $E[\exp(q_T)]$ and $E[\exp(q_F)]$. □

Contrasting Proposition A.2 with Proposition 1 in the main text, the only difference is in prediction 3. In particular, we no longer obtain the result emphasized throughout the discussion that true positives may respond *less* in areas where α is larger so long as false positives are largely intentional (small χ). However, this contrast emerges not from a deep fundamental difference in the predictions, but from the fact that the propositions have been established for marginal changes in incentives and surrounding conditions. But in the perfect substitutes case, agents select a corner solution except in a borderline case. Thus, it is more important to focus on the implications that the changes in underlying parameters have on the agent's choice about which effort to choose, good or bad.

The next corollary establishes a result with implications along the lines of prediction 4 in Proposition 1 in the main text. In particular, it shows that, as long as false positives are largely intentional, it is more likely that the agent specializes in bad effort in places with weak institutions. This implies that, when comparing places with stronger and weaker institutions, the impact of incentives on true positives is smaller in places with weaker institutions (namely, no impact) than in those with stronger institutions (where there should be an increase).

Corollary A.3. ***(Weak institutions and specialization in bad effort)***
Suppose that false and true positives are perfect technological substitutes, with $\delta = \sqrt{c_T c_F}$.

Then, weaker institutions are more likely to lead to specialization in bad effort ($a_T^* = 0$ *and* $a_F^* > 0$) *if and only if:*

$$\chi > \sqrt{\frac{c_T}{c_F}}$$

Proof. The result follows directly from inspecting the effect of an increase in α in either term of the inequality $\frac{\alpha}{\sqrt{c_F}} \lessgtr \frac{1+\chi\alpha}{\sqrt{c_T}}$, the key condition in (A.4). □

A.5 Implications for $\mathbb{E}(q)$

Consider focusing on:

$$\mathbb{E}[q_T^*] = \mathbb{E}[a_T^* + \varepsilon_T] = a_T^*, \tag{A.5}$$
$$\mathbb{E}[q_F^*] = \mathbb{E}\left[\chi(a_T^* + \varepsilon_T) + (a_F^* + \varepsilon_F)\right] = \chi a_T^* + a_F^*. \tag{A.6}$$

To see that comparative statics are identical as when focusing on $\mathbb{E}[\exp(q_J^*)]$ it suffices to notice that for $J \in \{F, P\}$

$$\frac{\partial a_J^*}{\partial s} = \frac{a_J^*}{s}, \ \frac{\partial^2 a_J^*}{\partial s \partial \pi} = \frac{1}{s}\frac{\partial a_J^*}{\partial \pi}, \text{ and } \frac{\partial^2 a_J^*}{\partial s \partial \alpha} = \frac{1}{s}\frac{\partial a_J^*}{\partial \alpha}.$$

Therefore, after deriving, substituting these properties and rearranging, the following equivalences hold for true positives:

$$\begin{aligned}
\frac{\partial \mathbb{E}[\exp(q_T^*)]}{\partial s} &= \mathbb{E}[\exp(q_T^*)]\frac{\partial a_T^*}{\partial s}, \\
\frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \pi} &= \mathbb{E}[\exp(q_T^*)](1 + a_T^*)\frac{\partial^2 a_T^*}{\partial s \partial \pi}, \\
\frac{\partial^2 \mathbb{E}[\exp(q_T^*)]}{\partial s \partial \alpha} &= \mathbb{E}[\exp(q_T^*)](1 + a_T^*)\frac{\partial^2 a_T^*}{\partial s \partial \alpha}.
\end{aligned}$$

Similarly, for false positives:

$$\begin{aligned}
\frac{\partial \mathbb{E}[\exp(q_F^*)]}{\partial s} &= \mathbb{E}[\exp(q_F^*)]\left(\chi\frac{\partial a_T^*}{\partial s} + \frac{\partial a_F^*}{\partial s}\right), \\
\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \pi} &= \mathbb{E}[\exp(q_F^*)]\left(1 + \chi a_T^* + a_F^*\right)\left(\chi\frac{\partial^2 a_T^*}{\partial s \partial \pi} + \frac{\partial^2 a_F^*}{\partial s \partial \pi}\right), \\
\frac{\partial^2 \mathbb{E}[\exp(q_F^*)]}{\partial s \partial \alpha} &= \mathbb{E}[\exp(q_F^*)]\left(1 + \chi a_T^* + a_F^*\right)\left(\chi\frac{\partial^2 a_T^*}{\partial s \partial \alpha} + \frac{\partial^2 a_F^*}{\partial s \partial \alpha}\right).
\end{aligned}$$

Since $\mathbb{E}[\exp(q_J^*)]$, $1 + a_T^*$, and $1 + \chi a_T^* + a_F^*$ are strictly positive, this establishes that the sign of the partials and cross partials for $\mathbb{E}[\exp(q_J^*)]$ is determined by the sign of the partials and cross partials for $\mathbb{E}[q_J^*]$ — since in each case, the partials and cross partial of $\mathbb{E}[q_J^*]$ are given by the terms in parentheses, which give the responses of a_T^* and $\chi a_T^* + a_F^*$ to the parameter changes.

A.6 Ratio of outputs and bad effort

As noted in the text, examining the response of the ratio of true to false positives to incentives does not help determine the role of bad relative to good effort. To see this, define this ratio as

$$r(s) = \frac{\exp\left(q_F\left(s\right)\right)}{\exp\left(q_T\left(s\right)\right)} = \exp\left[\chi(a_T^*(s) + \varepsilon_T) + (a_F^*(s) + \varepsilon_F) - (a_T^*(s) + \varepsilon_T)\right].$$

Taking the derivative with respect to incentives s and using $\partial a_T^*(s)/\partial s = a_T^*(s)/s$, we find

$$\frac{\partial r(s)}{\partial s} = \frac{r(s)}{s}\left(\left(\chi - 1\right)\left(a_T^*\left(s\right)\right) + a_F^*\left(s\right)\right).$$

Thus, even with agents exerting bad effort, the ratio of false to true positives may increase or decrease depending on the (unknown) relative magnitudes of good effort, bad effort, and the fraction of collateral damage (which moreover has been assumed constant for tractability, but could vary with the degree of effort further complicating the relationship between the importance of both types of effort and the ratio of observed false to true positives).

A.7 Additional tables

Table A-1: Variables and sources

Variable	Description	Sources
	Dependent variables	
	Positives	
False positives	Arbitrary executions of civilians presented as members of illegal armed groups. Measured bianually (from January to June and July to December) from 2000 to 2010. We use both the number of instances (events) producing such killings (*false positives cases*) as well as the number of people killed in the events in each municipality and semester (*false positives casualties*).	CINEP's Data Bank on Human Rights and Political Violence.
True positives	Killings of rebels, guerillas or paramilitaries, by the government armed forces. Measured bianually (from January to June and July to December) from 2000 to 2010. We use both the number of instances (events) producing such killings (*true positives cases*) as well as the number of people killed in the events in each municipality and semester (*true positives casualties*).	CERAC-Universidad del Rosario with information from CINEP.
	Judicial Institutions	
Judicial Inefficiency Index	Ratio of complaints against functionaries in the judicial branch to total complaints. Measured bianually (from January to June and July to December) from 2000 to 2010.	Inspector General (*Procuraduría*).
	Security	
Gueriilla, government, or paramilitary attacks	Dummy variable that equals 1 if the corresponding group perpetrated any attack on a given municipality or semester, from 2000 to 2010.	CERAC-Universidad del Rosario with information from CINEP.
	Explanatory variables	
Initial Judicial Inefficiency	Ratio of complaints against functionaries in the judicial branch to total complaints, from the first semester of 1995 to the second semester of 1999 (before the main sample period)	Inspector General (*Procuraduría*).
Colonels	We use three measures at the semester and municipality level: a dummy, unweighted share, and weighted share. The *dummy* is simply an indicator variable that equals one if any of the brigades operating in a given municipality are led by colonels. The *unweigthed share* computes the share of brigades with influence in the municipality that are led by colonels. The *weighted share* computes the share weighting by brigade population, defined as the total population in municipalities under each brigade's jurisdiction. We also distinguish between the *mobile* brigade share and *regular* colonel-led brigade share in additional Appendix exercises. The *mobile* brigade share is the ratio of mobile to total brigades in the municipality (with mobile brigades always led by colonels), and te regular colonel share is the ratio of colonel-led regular brigades to total brigades.	Colombian Army Webpage and expired versions through Internet Archive's Way Back Machine (`http://archive.org/web/`), Online news search in *El Tiempo*, DANE for municipal population figures.

-Continues in next page-

Table A-1: Variables and sources
-continued from previous page-

Variable	Description	Sources
	Controls	
	Geographic	
Rainfall	Mean annual rainfall level in each municipality in milimeters	All geographic controls from Municipal Panel, CEDE, Universidad de los Andes
Distance to capital	Linear distance to the state's capital in thousands of kilometers	
Soil quality and soil erosion	Soil types are categorized by the Colombian Geographic Institute on a scale of 1-8 based on suitability for agriculture, and on a scale of 1-6 based on soil erosion. The index is weighted average of soil type by municipality.	
Water availability	Weighted average from sub-municipal indicators of availability.	
Altitude	Altitude above sea level, in meters, of the urban center of each municipality	
Municipality area	Total municipal area in hectares (in logs)	
	Basic socioeconomic, in year 2000	
Population, 2000	Total municipal population (in logs)	Colombian Statistical Agency (DANE)
Math, language, and science test scores, 2000	Municipal average scores per area for high-school graduates in the official standardized test	Colombian Institute for Higher Education (ICFES)
Tax Income per cap, 2000	Municipal total amount collected taxes. Millions of pesos per 100.000 inhabitants (in logs +1)	Colombian National Planning Department (DNP)
Poverty index, 2000	Proportion of people in poverty according to the Index of Unmet (or Unsatisfied) Basic Needs. Basic Needs are defined at the household level using indicators for housing overcrowding, dwelling physical characteristics, access to public services, proportion of economically dependent members, and children school attendance.	Colombian Statistical Agency (DANE)
	Additional	
Navy presence	Indicator variable that equals 1 if a Navy unit operates in the municipality.	Colombian Army Official Website
Guerrilla, government and paramilitary attacks (1991-2000)	Average attacks by group, between 1991 and 2000 and per 100.000 inhabitants	CERAC-Universidad del Rosario
Unemployment rate, 2005	Municipal unemployment rate	Colombian Statistical Agency (DANE), Census 2005
Catholic churches per capita	Number of catholic churches per person in each municipality.	Municipal Panel, CEDE, Universidad de los Andes
Coca cultivated area, 1999	Municipal area cultivated with coca, per 100 hectares	Municipal Panel, CEDE, Universidad de los Andes
Average protests per capita, 1995-1999	Sum of all protests per year and per person. Protests are defined as the set of social actions with more than 10 people who intentionally express demands or push for solutions from the state at its different levels, or from private entities or individuals, to address injustices, inequalities or exclusions.	CINEP, *Base de datos de luchas sociales* (Social struggles database)

Table A-2: Descriptive Statistics: Time-invariant variables

Variable	Mean	Std. Dev.	Min	Max	Obs.
Judicial Inefficiency Index					
Initial judicial inefficiency (1995-1999)	0.077	0.080	0.000	0.538	893
Controls (Interacted with time dummies in regressions)					
Geographic					
Mean annual rainfall	1971.843	1064.394	160.000	9200.000	893
Distance to state capital (thousand kms)	0.131	0.107	0.000	0.790	893
Soil quality index	2.683	1.203	0.000	8.000	893
Soil erosion index	1.978	1.019	0.000	5.000	893
Water availability index	3.4e+06	5.3e+05	2.0e+06	5.6e+06	893
Altitude (Km)	1.143	1.178	0.002	25.221	893
Log (Municipal area in km^2)	10.517	1.153	7.313	15.698	893
Basic socioeconomic (in year 2000)					
Log (population)	9.665	1.056	7.144	15.657	893
Math test scores	42.505	1.086	37.083	46.750	893
Language test scores	44.581	1.945	35.750	50.563	893
Science test scores	44.205	1.069	40.886	49.000	893
Log (Tax income per cap)	6.625	2.447	0.000	10.518	893
Poverty index	45.739	21.703	7.220	104.530	893
Additional					
Navy presence	0.029	0.168	0.000	1.000	893
Guerrilla attacks (1991-1999)	1.779	2.804	0.000	39.477	893
Paramilitar attacks (1991-1999)	0.215	0.475	0.000	4.461	893
Unemployment rate 2005	0.049	0.044	0.000	0.430	893
Catholic churches per capita	11.204	11.260	0.000	106.671	893
Coca cultivated area per 100 hectares 1999	0.078	0.656	0.000	16.072	893
Average protests per capita (1995-1999)	0.679	2.118	0.000	31.571	893

Notes: Judicial inefficiency is the ratio of complaints against judicial functionaries relative to total complaints against all public officials, over the period 1995 to 1999.

Table A-3: False positives, colonels and judicial inefficiency, 2000-2010
Using colonel dummy

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1 + false positives)								
Judicial Inefficiency								
... x 2002:1	−0.0049	0.0203	−0.0123	0.0112	−0.0035	0.0208	−0.0131	0.0094
	(0.0311)	(0.0441)	(0.0317)	(0.0449)	(0.0316)	(0.0448)	(0.0325)	(0.0459)
... x 2002:2	−0.0113	0.0001	−0.0187	−0.0089	−0.0100	0.0006	−0.0195	−0.0107
	(0.0541)	(0.0772)	(0.0544)	(0.0776)	(0.0544)	(0.0776)	(0.0549)	(0.0782)
... x Incentives (2003:1-2008:2)	0.0822***	0.1111***	0.0100***	0.0138***	0.0836***	0.1116***	0.0100***	0.0136***
	(0.0230)	(0.0308)	(0.0032)	(0.0046)	(0.0236)	(0.0318)	(0.0033)	(0.0048)
... x 2009:1					−0.0034	0.0031	−0.0125	−0.0077
					(0.0258)	(0.0305)	(0.0268)	(0.0320)
... x 2009:2					0.0141	0.0008	0.0050	−0.0100
					(0.0306)	(0.0298)	(0.0314)	(0.0314)
Colonel in charge (dummy)								
... x 2002:1	−0.0022	−0.0061	−0.0005	−0.0047	−0.0021	−0.0057	0.0011	−0.0029
	(0.0074)	(0.0096)	(0.0072)	(0.0093)	(0.0074)	(0.0097)	(0.0073)	(0.0094)
... x 2002:2	−0.0108	−0.0220	−0.0091	−0.0207	−0.0107	−0.0216	−0.0075	−0.0188
	(0.0089)	(0.0143)	(0.0087)	(0.0140)	(0.0089)	(0.0143)	(0.0088)	(0.0140)
... x Incentives (2003:1-2008:2)	0.0262***	0.0348***	0.0036***	0.0046***	0.0263***	0.0352***	0.0038***	0.0048***
	(0.0071)	(0.0093)	(0.0008)	(0.0010)	(0.0072)	(0.0095)	(0.0008)	(0.0011)
... x 2009:1					0.0003	0.0026	0.0059	0.0085
					(0.0067)	(0.0081)	(0.0070)	(0.0086)
... x 2009:2					0.0002	−0.0003	0.0057	0.0054
					(0.0073)	(0.0079)	(0.0076)	(0.0085)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.066	0.064	0.068	0.065	0.066	0.064	0.068	0.065

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-4: True positives, colonels and judicial inefficiency, 2000-2010
Using colonel dummy

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+true positives)								
Judicial Inefficiency								
... x 2002:1	0.1330	0.3734	0.1339	0.3994	0.1145	0.3442	0.1214	0.3852
	(0.1305)	(0.3333)	(0.1293)	(0.3311)	(0.1327)	(0.3367)	(0.1305)	(0.3327)
... x 2002:2	−0.0031	−0.2529	−0.0023	−0.2269	−0.0217	−0.2821	−0.0148	−0.2411
	(0.1087)	(0.1797)	(0.1073)	(0.1762)	(0.1113)	(0.1861)	(0.1087)	(0.1791)
... x Incentives (2003:1-2008:2)	−0.0621	−0.1282	−0.0085*	−0.0120	−0.0807	−0.1574*	−0.0099*	−0.0135
	(0.0471)	(0.0794)	(0.0051)	(0.0084)	(0.0529)	(0.0930)	(0.0055)	(0.0091)
... x 2009:1					−0.0269	−0.0521	−0.0195	−0.0104
					(0.0921)	(0.1483)	(0.0890)	(0.1395)
... x 2009:2					−0.1219	−0.1808	−0.1145	−0.1392
					(0.0790)	(0.1448)	(0.0754)	(0.1358)
Colonel in charge (dummy)								
... x 2002:1	0.0445	0.0200	0.0444	0.0218	0.0472	0.0274	0.0475	0.0296
	(0.0394)	(0.0520)	(0.0391)	(0.0516)	(0.0395)	(0.0523)	(0.0391)	(0.0518)
... x 2002:2	−0.0208	−0.0811	−0.0209	−0.0793	−0.0181	−0.0737	−0.0178	−0.0715
	(0.0338)	(0.0584)	(0.0336)	(0.0583)	(0.0338)	(0.0586)	(0.0336)	(0.0584)
... x Incentives (2003:1-2008:2)	0.0373***	0.0485***	0.0045***	0.0063***	0.0402***	0.0566***	0.0049***	0.0072***
	(0.0117)	(0.0175)	(0.0012)	(0.0017)	(0.0126)	(0.0192)	(0.0012)	(0.0019)
... x 2009:1					0.0145	0.0346	0.0179	0.0414
					(0.0162)	(0.0258)	(0.0162)	(0.0256)
... x 2009:2					0.0011	0.0098	0.0043	0.0163
					(0.0172)	(0.0287)	(0.0173)	(0.0287)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.062	0.060	0.063	0.061	0.062	0.060	0.063	0.061

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-5: False positives, colonels and judicial inefficiency, 2000-2010
Unweighted colonel share

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false positives)								
Judicial Inefficiency								
... x 2002:1	−0.0048	0.0202	−0.0124	0.0109	−0.0036	0.0206	−0.0133	0.0091
	(0.0310)	(0.0440)	(0.0316)	(0.0448)	(0.0315)	(0.0447)	(0.0324)	(0.0458)
... x 2002:2	−0.0113	−0.0000	−0.0189	−0.0093	−0.0100	0.0004	−0.0198	−0.0111
	(0.0542)	(0.0773)	(0.0545)	(0.0778)	(0.0545)	(0.0778)	(0.0550)	(0.0783)
... x Incentives (2003:1-2008:2)	0.0824***	0.1113***	0.0100***	0.0138***	0.0837***	0.1117***	0.0099***	0.0136***
	(0.0229)	(0.0307)	(0.0032)	(0.0046)	(0.0236)	(0.0318)	(0.0033)	(0.0048)
... x 2009:1					−0.0039	0.0027	−0.0133	−0.0084
					(0.0259)	(0.0306)	(0.0269)	(0.0321)
... x 2009:2					0.0141	0.0009	0.0047	−0.0102
					(0.0304)	(0.0297)	(0.0313)	(0.0313)
Colonel in charge (unweighted share)								
... x 2002:1	−0.0021	−0.0064	−0.0006	−0.0055	−0.0023	−0.0062	0.0005	−0.0042
	(0.0068)	(0.0093)	(0.0066)	(0.0089)	(0.0069)	(0.0094)	(0.0066)	(0.0090)
... x 2002:2	−0.0106	−0.0225	−0.0091	−0.0216	−0.0108	−0.0223	−0.0081	−0.0202
	(0.0085)	(0.0145)	(0.0083)	(0.0142)	(0.0086)	(0.0146)	(0.0084)	(0.0143)
... x Incentives (2003:1-2008:2)	0.0218***	0.0314***	0.0030***	0.0040***	0.0216***	0.0316***	0.0031***	0.0042***
	(0.0070)	(0.0093)	(0.0008)	(0.0010)	(0.0072)	(0.0096)	(0.0008)	(0.0011)
... x 2009:1					−0.0011	0.0014	0.0033	0.0058
					(0.0067)	(0.0081)	(0.0069)	(0.0085)
... x 2009:2					0.0003	−0.0001	0.0046	0.0041
					(0.0073)	(0.0079)	(0.0076)	(0.0085)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.065	0.064	0.066	0.064	0.065	0.064	0.066	0.064

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-6: True positives, colonels and judicial inefficiency, 2000-2010
Unweighted colonel share

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+true positives)								
Judicial Inefficiency								
... x 2002:1	0.1315	0.3716	0.1325	0.3979	0.1128	0.3422	0.1200	0.3837
	(0.1307)	(0.3336)	(0.1294)	(0.3315)	(0.1329)	(0.3370)	(0.1306)	(0.3330)
... x 2002:2	−0.0048	−0.2554	−0.0038	−0.2291	−0.0235	−0.2848	−0.0163	−0.2434
	(0.1093)	(0.1802)	(0.1079)	(0.1767)	(0.1119)	(0.1866)	(0.1093)	(0.1796)
... x Incentives (2003:1-2008:2)	−0.0624	−0.1288	−0.0086*	−0.0121	−0.0810	−0.1582*	−0.0099*	−0.0136
	(0.0471)	(0.0793)	(0.0051)	(0.0083)	(0.0529)	(0.0930)	(0.0055)	(0.0091)
... x 2009:1					−0.0282	−0.0531	−0.0206	−0.0110
					(0.0916)	(0.1476)	(0.0885)	(0.1388)
... x 2009:2					−0.1216	−0.1821	−0.1140	−0.1401
					(0.0791)	(0.1444)	(0.0755)	(0.1355)
Colonel in charge (unweighted share)								
... x 2002:1	0.0320	0.0086	0.0326	0.0115	0.0337	0.0142	0.0351	0.0180
	(0.0347)	(0.0483)	(0.0345)	(0.0479)	(0.0348)	(0.0486)	(0.0346)	(0.0482)
... x 2002:2	−0.0313	−0.0936	−0.0307	−0.0908	−0.0295	−0.0881	−0.0282	−0.0842
	(0.0357)	(0.0611)	(0.0356)	(0.0610)	(0.0356)	(0.0612)	(0.0355)	(0.0611)
... x Incentives (2003:1-2008:2)	0.0320***	0.0403**	0.0040***	0.0055***	0.0339***	0.0463**	0.0043***	0.0062***
	(0.0119)	(0.0176)	(0.0012)	(0.0018)	(0.0128)	(0.0193)	(0.0013)	(0.0019)
... x 2009:1					0.0095	0.0279	0.0133	0.0354
					(0.0158)	(0.0254)	(0.0157)	(0.0251)
... x 2009:2					0.0001	0.0044	0.0037	0.0117
					(0.0168)	(0.0286)	(0.0168)	(0.0285)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.062	0.060	0.062	0.060	0.062	0.060	0.063	0.060

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-7: False positives, colonels and judicial inefficiency, 2000-2010
Baseline results - No Controls

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false positives)								
Judicial Inefficiency								
... x 2002:1	−0.0173	−0.0049	−0.0342	−0.0246	−0.0124	−0.0022	−0.0348	−0.0272
	(0.0307)	(0.0432)	(0.0310)	(0.0435)	(0.0314)	(0.0442)	(0.0318)	(0.0445)
... x 2002:2	0.0053	0.0175	−0.0117	−0.0022	0.0101	0.0202	−0.0122	−0.0048
	(0.0481)	(0.0682)	(0.0482)	(0.0682)	(0.0485)	(0.0688)	(0.0487)	(0.0688)
... x Incentives (2003:1-2008:2)	0.1263***	0.1516***	0.0140***	0.0169***	0.1312***	0.1543***	0.0139***	0.0167***
	(0.0241)	(0.0310)	(0.0033)	(0.0046)	(0.0249)	(0.0324)	(0.0034)	(0.0047)
... x 2009:1					0.0131	0.0126	−0.0090	−0.0121
					(0.0365)	(0.0443)	(0.0369)	(0.0445)
... x 2009:2					0.0263	0.0098	0.0042	−0.0149
					(0.0379)	(0.0414)	(0.0382)	(0.0416)
Colonel in charge (share)								
... x 2002:1	−0.0108	−0.0188*	−0.0090	−0.0171*	−0.0092	−0.0163	−0.0067	−0.0140
	(0.0072)	(0.0101)	(0.0069)	(0.0097)	(0.0075)	(0.0105)	(0.0071)	(0.0099)
... x 2002:2	−0.0136*	−0.0225**	−0.0119*	−0.0208**	−0.0120	−0.0199*	−0.0096	−0.0176*
	(0.0074)	(0.0101)	(0.0071)	(0.0097)	(0.0076)	(0.0105)	(0.0073)	(0.0099)
... x Incentives (2003:1-2008:2)	0.0108*	0.0171**	0.0017**	0.0025***	0.0126*	0.0199**	0.0020**	0.0028***
	(0.0063)	(0.0084)	(0.0007)	(0.0010)	(0.0069)	(0.0092)	(0.0008)	(0.0011)
... x 2009:1					0.0025	0.0060	0.0060	0.0099
					(0.0065)	(0.0082)	(0.0068)	(0.0085)
... x 2009:2					0.0071	0.0091	0.0105	0.0128
					(0.0064)	(0.0077)	(0.0066)	(0.0080)
Observations	24310	24310	24310	24310	24310	24310	24310	24310
Municipalities	1105	1105	1105	1105	1105	1105	1105	1105
R-Squared	0.003	0.002	0.003	0.003	0.003	0.002	0.003	0.003

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-8: True positives, colonels and judicial inefficiency, 2000-2010
Baseline results - No controls

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+true positives)								
Judicial Inefficiency								
... x 2002:1	0.2021	0.4454	0.1716	0.4202	0.1828	0.4093	0.1516	0.3899
	(0.1264)	(0.2843)	(0.1257)	(0.2826)	(0.1277)	(0.2867)	(0.1264)	(0.2836)
... x 2002:2	0.1370	0.1098	0.1065	0.0846	0.1177	0.0737	0.0865	0.0543
	(0.1026)	(0.1715)	(0.1018)	(0.1690)	(0.1042)	(0.1755)	(0.1027)	(0.1708)
... x Incentives (2003:1-2008:2)	0.0117	−0.0129	−0.0057	−0.0081	−0.0075	−0.0490	−0.0079*	−0.0114
	(0.0378)	(0.0647)	(0.0041)	(0.0066)	(0.0417)	(0.0747)	(0.0043)	(0.0071)
... x 2009:1					−0.0641	−0.1295	−0.0951	−0.1486
					(0.0616)	(0.0997)	(0.0594)	(0.0917)
... x 2009:2					−0.0909	−0.1620	−0.1219*	−0.1809
					(0.0737)	(0.1299)	(0.0720)	(0.1240)
Colonel in charge (share)								
... x 2002:1	−0.0150	−0.0496	−0.0104	−0.0406	−0.0154	−0.0494	−0.0085	−0.0364
	(0.0238)	(0.0370)	(0.0234)	(0.0367)	(0.0247)	(0.0385)	(0.0240)	(0.0376)
... x 2002:2	−0.0487**	−0.1035***	−0.0441**	−0.0945***	−0.0491**	−0.1033***	−0.0422*	−0.0903***
	(0.0214)	(0.0324)	(0.0211)	(0.0322)	(0.0226)	(0.0340)	(0.0218)	(0.0331)
... x Incentives (2003:1-2008:2)	0.0032	−0.0028	0.0014	0.0016	0.0027	−0.0026	0.0016	0.0020
	(0.0126)	(0.0182)	(0.0012)	(0.0018)	(0.0153)	(0.0223)	(0.0014)	(0.0021)
... x 2009:1					0.0039	0.0154	0.0118	0.0298
					(0.0169)	(0.0267)	(0.0162)	(0.0257)
... x 2009:2					−0.0068	−0.0152	0.0010	−0.0010
					(0.0183)	(0.0298)	(0.0176)	(0.0290)
Observations	24310	24310	24310	24310	24310	24310	24310	24310
Municipalities	1105	1105	1105	1105	1105	1105	1105	1105
R-Squared	0.001	0.001	0.001	0.001	0.001	0.001	0.001	0.001

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-9: False positives, colonels and judicial inefficiency, 2000-2010
Inverse hyperbolic sine transformation

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is false positives inverse hyperbolic sine transformation								
Judicial Inefficiency								
... x 2002:1	−0.0074	0.0260	−0.0177	0.0134	−0.0058	0.0266	−0.0190	0.0110
	(0.0397)	(0.0567)	(0.0405)	(0.0578)	(0.0403)	(0.0576)	(0.0415)	(0.0590)
... x 2002:2	−0.0149	0.0012	−0.0253	−0.0114	−0.0134	0.0018	−0.0266	−0.0138
	(0.0703)	(0.1005)	(0.0707)	(0.1011)	(0.0706)	(0.1011)	(0.0712)	(0.1018)
... x Incentives (2003:1-2008:2)	0.1077***	0.1446***	0.0130***	0.0178***	0.1092***	0.1453***	0.0128***	0.0175***
	(0.0295)	(0.0396)	(0.0041)	(0.0060)	(0.0304)	(0.0409)	(0.0043)	(0.0062)
... x 2009:1					−0.0058	0.0032	−0.0186	−0.0120
					(0.0333)	(0.0394)	(0.0346)	(0.0413)
... x 2009:2					0.0181	0.0020	0.0052	−0.0132
					(0.0386)	(0.0378)	(0.0397)	(0.0399)
Colonel in charge (share)								
... x 2002:1	−0.0038	−0.0100	−0.0024	−0.0097	−0.0042	−0.0098	−0.0016	−0.0086
	(0.0085)	(0.0117)	(0.0082)	(0.0112)	(0.0086)	(0.0119)	(0.0083)	(0.0114)
... x 2002:2	−0.0142	−0.0297	−0.0128	−0.0295	−0.0146	−0.0296	−0.0120	−0.0284
	(0.0107)	(0.0188)	(0.0104)	(0.0185)	(0.0108)	(0.0190)	(0.0105)	(0.0186)
... x Incentives (2003:1-2008:2)	0.0219**	0.0350***	0.0030***	0.0043***	0.0215**	0.0351***	0.0030***	0.0044***
	(0.0092)	(0.0121)	(0.0010)	(0.0013)	(0.0095)	(0.0126)	(0.0011)	(0.0014)
... x 2009:1					−0.0034	0.0002	0.0008	0.0037
					(0.0086)	(0.0104)	(0.0090)	(0.0111)
... x 2009:2					0.0010	0.0006	0.0050	0.0039
					(0.0089)	(0.0099)	(0.0093)	(0.0106)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.065	0.063	0.065	0.064	0.065	0.063	0.065	0.064

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-10: True positives, colonels and judicial inefficiency, 2000-2010
Inverse hyperbolic sine transformation

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is true positives inverse hyperbolic sine transformation								
Judicial Inefficiency								
... x 2002:1	0.1634	0.4294	0.1652	0.4631	0.1386	0.3936	0.1488	0.4462
	(0.1688)	(0.4022)	(0.1673)	(0.3994)	(0.1717)	(0.4068)	(0.1688)	(0.4015)
... x 2002:2	−0.0062	−0.3170	−0.0043	−0.2834	−0.0309	−0.3529	−0.0207	−0.3003
	(0.1395)	(0.2253)	(0.1378)	(0.2209)	(0.1429)	(0.2335)	(0.1396)	(0.2247)
... x Incentives (2003:1-2008:2)	−0.0813	−0.1617	−0.0111*	−0.0151	−0.1061	−0.1975*	−0.0129*	−0.0169
	(0.0609)	(0.1010)	(0.0066)	(0.0107)	(0.0684)	(0.1183)	(0.0070)	(0.0116)
... x 2009:1					−0.0388	−0.0649	−0.0281	−0.0116
					(0.1178)	(0.1889)	(0.1139)	(0.1780)
... x 2009:2					−0.1600	−0.2235	−0.1494	−0.1703
					(0.1026)	(0.1792)	(0.0981)	(0.1677)
Colonel in charge (share)								
... x 2002:1	0.0122	−0.0137	0.0146	−0.0078	0.0127	−0.0116	0.0168	−0.0026
	(0.0411)	(0.0613)	(0.0409)	(0.0611)	(0.0411)	(0.0617)	(0.0409)	(0.0614)
... x 2002:2	−0.0473	−0.1155	−0.0450	−0.1095	−0.0468	−0.1133	−0.0427	−0.1044
	(0.0476)	(0.0740)	(0.0474)	(0.0739)	(0.0475)	(0.0741)	(0.0474)	(0.0740)
... x Incentives (2003:1-2008:2)	0.0326**	0.0373*	0.0044***	0.0057**	0.0331**	0.0396	0.0047***	0.0063**
	(0.0154)	(0.0226)	(0.0016)	(0.0023)	(0.0167)	(0.0248)	(0.0017)	(0.0025)
... x 2009:1					0.0050	0.0217	0.0116	0.0341
					(0.0200)	(0.0320)	(0.0199)	(0.0315)
... x 2009:2					−0.0032	−0.0106	0.0033	0.0016
					(0.0215)	(0.0382)	(0.0215)	(0.0382)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.062	0.060	0.062	0.060	0.062	0.060	0.062	0.060

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-11: False positives, colonels and judicial inefficiency, 2000-2010
Controlling for quartic population polynomial

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false positives)								
Judicial Inefficiency								
... x 2002:1	−0.0027	0.0230	−0.0114	0.0127	−0.0015	0.0232	−0.0124	0.0105
	(0.0306)	(0.0436)	(0.0312)	(0.0445)	(0.0310)	(0.0443)	(0.0320)	(0.0455)
... x 2002:2	−0.0192	−0.0095	−0.0278	−0.0198	−0.0180	−0.0092	−0.0289	−0.0219
	(0.0462)	(0.0668)	(0.0466)	(0.0674)	(0.0465)	(0.0673)	(0.0471)	(0.0680)
... x Incentives (2003:1-2008:2)	0.0844***	0.1131***	0.0100***	0.0138***	0.0857***	0.1134***	0.0099***	0.0136***
	(0.0228)	(0.0305)	(0.0032)	(0.0047)	(0.0234)	(0.0315)	(0.0033)	(0.0049)
... x 2009:1					−0.0044	0.0016	−0.0151	−0.0108
					(0.0261)	(0.0305)	(0.0272)	(0.0322)
... x 2009:2					0.0146	0.0004	0.0039	−0.0120
					(0.0301)	(0.0293)	(0.0311)	(0.0311)
Colonel in charge (share)								
... x 2002:1	−0.0021	−0.0067	−0.0012	−0.0067	−0.0024	−0.0066	−0.0007	−0.0059
	(0.0067)	(0.0094)	(0.0064)	(0.0090)	(0.0068)	(0.0095)	(0.0066)	(0.0091)
... x 2002:2	−0.0029	−0.0123	−0.0021	−0.0123	−0.0033	−0.0121	−0.0015	−0.0115
	(0.0102)	(0.0177)	(0.0100)	(0.0175)	(0.0102)	(0.0178)	(0.0100)	(0.0175)
... x Incentives (2003:1-2008:2)	0.0170**	0.0268***	0.0022***	0.0032***	0.0166**	0.0269***	0.0023***	0.0033***
	(0.0070)	(0.0093)	(0.0008)	(0.0010)	(0.0073)	(0.0097)	(0.0008)	(0.0011)
... x 2009:1					−0.0024	0.0004	0.0006	0.0028
					(0.0066)	(0.0080)	(0.0068)	(0.0085)
... x 2009:2					0.0006	0.0004	0.0034	0.0026
					(0.0068)	(0.0075)	(0.0071)	(0.0081)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.073	0.073	0.073	0.073	0.073	0.073	0.073	0.073

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: quartic polynomial of the logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-12: True positives, colonels and judicial inefficiency, 2000-2010
Controlling for quartic population polynomial

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+true positives)								
Judicial Inefficiency								
... x 2002:1	0.0934	0.3168	0.0886	0.3330	0.0750	0.2894	0.0750	0.3180
	(0.1268)	(0.3414)	(0.1256)	(0.3393)	(0.1290)	(0.3447)	(0.1268)	(0.3408)
... x 2002:2	0.0079	−0.2206	0.0031	−0.2044	−0.0105	−0.2479	−0.0105	−0.2194
	(0.1020)	(0.1774)	(0.1006)	(0.1738)	(0.1048)	(0.1838)	(0.1021)	(0.1768)
... x Incentives (2003:1-2008:2)	−0.0453	−0.1002	−0.0075	−0.0104	−0.0637	−0.1275	−0.0090*	−0.0120
	(0.0468)	(0.0793)	(0.0051)	(0.0084)	(0.0526)	(0.0931)	(0.0054)	(0.0091)
... x 2009:1					−0.0255	−0.0439	−0.0251	−0.0148
					(0.0917)	(0.1490)	(0.0888)	(0.1403)
... x 2009:2					−0.1221	−0.1751	−0.1217	−0.1460
					(0.0800)	(0.1447)	(0.0767)	(0.1357)
Colonel in charge (share)								
... x 2002:1	0.0417	0.0228	0.0428	0.0263	0.0434	0.0277	0.0453	0.0325
	(0.0365)	(0.0511)	(0.0363)	(0.0508)	(0.0366)	(0.0514)	(0.0363)	(0.0510)
... x 2002:2	−0.0284	−0.0914	−0.0273	−0.0880	−0.0267	−0.0866	−0.0248	−0.0817
	(0.0354)	(0.0615)	(0.0353)	(0.0614)	(0.0354)	(0.0616)	(0.0353)	(0.0615)
... x Incentives (2003:1-2008:2)	0.0297**	0.0366**	0.0038***	0.0052***	0.0314**	0.0419**	0.0041***	0.0059***
	(0.0119)	(0.0175)	(0.0012)	(0.0018)	(0.0128)	(0.0192)	(0.0013)	(0.0019)
... x 2009:1					0.0090	0.0259	0.0133	0.0341
					(0.0158)	(0.0253)	(0.0158)	(0.0250)
... x 2009:2					0.0001	0.0026	0.0042	0.0106
					(0.0166)	(0.0285)	(0.0166)	(0.0285)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.068	0.068	0.069	0.068	0.068	0.068	0.069	0.068

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: quartic polynomial of the logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-13: False positives, colonels and judicial inefficiency, 2000-2010
Regular and mobile brigades

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false positives)								
Judicial Inefficiency								
... x 2002:1	−0.0049	0.0199	−0.0137	0.0092	−0.0037	0.0203	−0.0149	0.0070
	(0.0307)	(0.0435)	(0.0313)	(0.0443)	(0.0312)	(0.0442)	(0.0321)	(0.0453)
... x 2002:2	−0.0112	−0.0000	−0.0200	−0.0107	−0.0099	0.0004	−0.0212	−0.0129
	(0.0545)	(0.0778)	(0.0547)	(0.0782)	(0.0548)	(0.0782)	(0.0552)	(0.0787)
... x Incentives (2003:1-2008:2)	0.0799***	0.1087***	0.0094***	0.0131***	0.0811***	0.1092***	0.0092***	0.0129***
	(0.0230)	(0.0308)	(0.0032)	(0.0046)	(0.0237)	(0.0318)	(0.0033)	(0.0048)
... x 2009:1					−0.0037	0.0030	−0.0149	−0.0102
					(0.0255)	(0.0302)	(0.0265)	(0.0317)
... x 2009:2					0.0132	0.0001	0.0022	−0.0128
					(0.0302)	(0.0297)	(0.0310)	(0.0311)
Colonel in charge (regular)								
... x 2002:1	−0.0034	−0.0080	−0.0025	−0.0078	−0.0036	−0.0078	−0.0016	−0.0068
	(0.0065)	(0.0090)	(0.0062)	(0.0086)	(0.0065)	(0.0091)	(0.0063)	(0.0087)
... x 2002:2	−0.0114	−0.0234	−0.0105	−0.0233	−0.0116	−0.0232	−0.0097	−0.0222
	(0.0081)	(0.0145)	(0.0079)	(0.0142)	(0.0081)	(0.0145)	(0.0079)	(0.0143)
... x Incentives (2003:1-2008:2)	0.0169**	0.0267***	0.0023***	0.0033***	0.0167**	0.0269***	0.0024***	0.0034***
	(0.0069)	(0.0092)	(0.0008)	(0.0010)	(0.0071)	(0.0095)	(0.0008)	(0.0011)
... x 2009:1					−0.0012	0.0015	0.0024	0.0047
					(0.0063)	(0.0078)	(0.0065)	(0.0082)
... x 2009:2					0.0001	−0.0003	0.0036	0.0027
					(0.0071)	(0.0079)	(0.0074)	(0.0084)
Colonel in charge (mobile)								
... x 2002:1	0.0687	0.0722	0.0425	0.0540	0.0602	0.0638	0.0444	0.0611
	(0.0805)	(0.0964)	(0.0766)	(0.0918)	(0.0783)	(0.0940)	(0.0761)	(0.0908)
... x 2002:2	0.0455	0.0367	0.0192	0.0186	0.0369	0.0284	0.0211	0.0257
	(0.0861)	(0.1056)	(0.0823)	(0.1013)	(0.0841)	(0.1034)	(0.0818)	(0.1003)
... x Incentives (2003:1-2008:2)	0.2536***	0.2513***	0.0282***	0.0297***	0.2438***	0.2419***	0.0285***	0.0306***
	(0.0678)	(0.0726)	(0.0079)	(0.0086)	(0.0642)	(0.0692)	(0.0081)	(0.0089)
... x 2009:1					−0.0246	−0.0240	0.0038	0.0210
					(0.0552)	(0.0565)	(0.0558)	(0.0595)
... x 2009:2					−0.0165	−0.0156	0.0097	0.0268
					(0.0272)	(0.0299)	(0.0334)	(0.0398)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.073	0.067	0.075	0.069	0.073	0.067	0.075	0.069

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-14: True positives, colonels and judicial inefficiency, 2000-2010
Regular and mobile brigades

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+true positives)								
Judicial Inefficiency								
... x 2002:1	0.1194	0.3586	0.1212	0.3859	0.0982	0.3238	0.1073	0.3686
	(0.1301)	(0.3354)	(0.1287)	(0.3331)	(0.1323)	(0.3389)	(0.1299)	(0.3345)
... x 2002:2	−0.0088	−0.2582	−0.0070	−0.2309	−0.0300	−0.2931	−0.0209	−0.2482
	(0.1097)	(0.1783)	(0.1082)	(0.1747)	(0.1123)	(0.1848)	(0.1096)	(0.1777)
... x Incentives (2003:1-2008:2)	−0.0669	−0.1357*	−0.0091*	−0.0129	−0.0883*	−0.1708*	−0.0106*	−0.0148
	(0.0471)	(0.0794)	(0.0052)	(0.0084)	(0.0530)	(0.0933)	(0.0055)	(0.0092)
... x 2009:1					−0.0341	−0.0625	−0.0248	−0.0173
					(0.0902)	(0.1455)	(0.0870)	(0.1364)
... x 2009:2					−0.1331	−0.2110	−0.1236	−0.1656
					(0.0822)	(0.1489)	(0.0786)	(0.1397)
Colonel in charge (regular)								
... x 2002:1	0.0106	−0.0132	0.0118	−0.0096	0.0106	−0.0113	0.0130	−0.0056
	(0.0314)	(0.0476)	(0.0312)	(0.0474)	(0.0314)	(0.0479)	(0.0313)	(0.0477)
... x 2002:2	−0.0370	−0.0956	−0.0358	−0.0920	−0.0370	−0.0936	−0.0345	−0.0880
	(0.0369)	(0.0597)	(0.0368)	(0.0596)	(0.0368)	(0.0597)	(0.0368)	(0.0596)
... x Incentives (2003:1-2008:2)	0.0263**	0.0318*	0.0035***	0.0046***	0.0262**	0.0339*	0.0036***	0.0051***
	(0.0119)	(0.0176)	(0.0012)	(0.0018)	(0.0128)	(0.0193)	(0.0013)	(0.0019)
... x 2009:1					0.0054	0.0215	0.0102	0.0307
					(0.0160)	(0.0259)	(0.0159)	(0.0255)
... x 2009:2					−0.0057	−0.0094	−0.0010	−0.0004
					(0.0170)	(0.0305)	(0.0171)	(0.0305)
Colonel in charge (mobile)								
... x 2002:1	0.7553*	0.7652	0.6880*	0.6730	0.8426**	0.9518	0.7324*	0.7818
	(0.3948)	(0.5847)	(0.3925)	(0.5803)	(0.3941)	(0.5793)	(0.3927)	(0.5773)
... x 2002:2	0.1895	0.0268	0.1222	−0.0654	0.2768	0.2134	0.1667	0.0434
	(0.2692)	(0.4072)	(0.2700)	(0.4052)	(0.2717)	(0.4079)	(0.2740)	(0.4090)
... x Incentives (2003:1-2008:2)	0.2922***	0.4196***	0.0245***	0.0362***	0.3901***	0.6286***	0.0302***	0.0500***
	(0.0818)	(0.1271)	(0.0079)	(0.0116)	(0.0889)	(0.1397)	(0.0090)	(0.0136)
... x 2009:1					0.1795*	0.3226**	0.1140	0.2270
					(0.0945)	(0.1417)	(0.0979)	(0.1481)
... x 2009:2					0.2476**	0.6005***	0.1815	0.5037**
					(0.1217)	(0.2075)	(0.1304)	(0.2145)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.064	0.062	0.064	0.062	0.064	0.062	0.064	0.062

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table A-15: False and true positives, colonels and judicial inefficiency: neighboring spillovers, 2000-2010

	Incentives Dummy		Incentives Linear		Incentives Dummy		Incentives Linear	
	Cases	Casualties	Cases	Casualties	Cases	Casualties	Cases	Casualties
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is log (1+false or true positives)								
	False Positives				True Positives			
Judicial Inefficiency								
... x 2002:1	−0.0034	0.0219	−0.0114	0.0122	0.1266	0.3637	0.1275	0.3894
	(0.0296)	(0.0424)	(0.0302)	(0.0432)	(0.1358)	(0.3410)	(0.1346)	(0.3390)
... x 2002:2	−0.0135	−0.0085	−0.0215	−0.0182	−0.0085	−0.2715	−0.0076	−0.2458
	(0.0568)	(0.0854)	(0.0571)	(0.0857)	(0.1103)	(0.1883)	(0.1090)	(0.1850)
... x Incentives (2003:1-2008:2)	0.0829***	0.1119***	0.0100***	0.0138***	−0.0622	−0.1282	−0.0086*	−0.0122
	(0.0230)	(0.0308)	(0.0032)	(0.0046)	(0.0470)	(0.0793)	(0.0051)	(0.0083)
Colonel in charge (share)								
... x 2002:1	0.0212	0.0257	0.0198	0.0232	−0.0537	−0.1587	−0.0439	−0.1398
	(0.0274)	(0.0348)	(0.0273)	(0.0346)	(0.1438)	(0.1582)	(0.1440)	(0.1586)
... x 2002:2	−0.0420	−0.1446	−0.0434	−0.1471	−0.1064	−0.3947	−0.0967	−0.3757
	(0.0678)	(0.1416)	(0.0677)	(0.1414)	(0.0872)	(0.3145)	(0.0874)	(0.3144)
... x Incentives (2003:1-2008:2)	0.0218*	0.0380**	0.0023*	0.0041**	0.0113	0.0066	0.0033	0.0046
	(0.0131)	(0.0179)	(0.0013)	(0.0018)	(0.0275)	(0.0426)	(0.0028)	(0.0043)
Colonel in charge (average share for neighbors)								
... x 2002:1	−0.0308	−0.0425	−0.0276	−0.0391	0.0824	0.1873	0.0720	0.1680
	(0.0342)	(0.0442)	(0.0343)	(0.0441)	(0.1762)	(0.1847)	(0.1764)	(0.1851)
... x 2002:2	0.0401	0.1568	0.0433	0.1603	0.0911	0.3899	0.0807	0.3707
	(0.0824)	(0.1689)	(0.0825)	(0.1689)	(0.1018)	(0.3728)	(0.1019)	(0.3724)
... x Incentives (2003:1-2008:2)	−0.0061	−0.0141	−0.0000	−0.0009	0.0175	0.0297	0.0000	−0.0003
	(0.0147)	(0.0194)	(0.0015)	(0.0020)	(0.0320)	(0.0492)	(0.0033)	(0.0049)
Controls x time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	19646	19646	19646	19646	19646	19646	19646	19646
Municipalities	893	893	893	893	893	893	893	893
R-Squared	0.065	0.063	0.065	0.064	0.062	0.061	0.062	0.061

Notes: Panel estimation from the first half of 2000 to the second half of 2010 with municipality and time (half-year) fixed effects. In "... x Incentives (2003:1-2008:2)", the variable shown is interacted with: a dummy that equals one (odd columns) or a linear trend (even columns), both from the first semester of 2003 to the second semester of 2008. Time dummies are interacted with the following set of time invariant predetermined municipal controls: logarithm of the population in 2000, average rainfall level, distance to the closest major city, quality of soil index, erosion index, water availability index, average elevation, municipality area, students' test results in math, science and language, poverty index, log of tax income per capita, presence of navy, paramilitary and guerilla attacks, unemployment rate, catholic churches per capita, fraction of coca cultivated area, and average protests per capita. Errors in parentheses control for spatial and first-order time correlation following Conley (1999, 2008). We allow spatial correlation to extend to up to 279 km from each municipality's centroid to ensure that each municipality has at least one neighbor. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.