The Perils of High-Powered Incentives: Evidence from Colombia's False Positives[†]

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We investigate the use of high-powered incentives for the Colombian military and show that this practice produced perverse side effects. Innocent civilians were killed and misrepresented as guerillas (a phenomenon known in Colombia as "false positives"). There were significantly more false positives during the period of high-powered incentives in municipalities with weaker judicial institutions and where a higher share of brigades were commanded by colonels, who have stronger career concerns than generals. In municipalities with a higher share of colonels, the high-powered incentives period also coincided with a worsening of local judicial institutions and no discernible improvement in overall security. (*JEL* D72, D74, D82, K41, K42, O17)

Though the classic theory of moral hazard emphasizes the importance of providing sufficient rewards for "success" or "good performance," it has long been recognized that high-powered incentives can distort the type of effort exerted or encourage various unproductive activities to improve indicators of performance (e.g., Holmström and Milgrom 1991, Baker 1992, Dixit 1997). Several empirical studies have documented this distortionary facet of high-powered incentives in teaching, managerial behavior, and bureaucracies (e.g., Baker, Gibbons, and Murphy 1994; Oyer 1998; Levitt and Jacob 2003; Aviv 2014; Miller and Babiarz 2014; Fisman and

Wang 2017).[1] In this paper, we study the implications of providing high-powered incentives for the military and security services under weak institutional controls.

Several governments, including in South Africa, the Philippines, Brazil, El Salvador, Guatemala, Mexico, and Peru, as well as the US military in Vietnam, and more recently in Afghanistan, have turned to high-powered incentives as a counterinsurgency strategy.[2] In all of these settings, a consequence, and often the root cause, of the insurgency was a weak institutional environment, which at the same time created impunity for the military now facing stronger incentives. For example, the South African Truth and Reconciliation Commission revealed that in its confrontation with the African National Congress, the country's main counter-insurgency force, the Civil Cooperation Bureau (CCB), "used cash as an incentive to 'produce'. Thus, like other hit-squad or counterinsurgency units such as Koevoet and C10, CCB members were provided with a positive inducement to undertake actions which could, and often did, result in a gross violation of another individ-ual's rights" (p. 142). In Guatemala's long civil war against left-wing insurgents, "The competition to advance in the hierarchy induced agents and officials to indulge more in repression, which created a perverse system in which distain for life became the most important quality to gain promotion." (Oficina de Derechos Humanos del Arzobispado de Guatemala 1998).[3] In both of these cases, even if incentives were intended to encourage the security forces to combat insurgents, they led to extensive human rights violations and violence against civilians.

In the Philippines, cash incentives are currently used to pressure the police to demonstrate results in anti-drug operations, and Amnesty International (2017, p. 29) quotes a police officer's description of current practices as follows:

> There are different types of benefits [for these operations]. We always get paid by the encounter. That's the word we use, "encounter." The amount ranges from 8,000 pesos (US $161) to 15,000 pesos (US $302). … The ones we really go after are pushers. There are categories [of pushers]— different levels based on their notoriety. Higher levels are paid more. … That amount is per head. So if the operation is against four people, that's 32,000 pesos (US $644). The PNP incentive isn't announced. … We're paid in cash, secretly, by headquarters. The payment is [split by] the unit. … There's no incentive for arresting. We're not paid anything.

The Amnesty International report finds that these incentives have encouraged not just actions against "drug lords and pushers," but also systematic extrajudicial killings of civilians.

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[1] For example, following Sears' pay-for-performance program for its auto mechanics, owners of intact cars were misled by mechanics into authorizing unnecessary repairs. Another well-publicized case is extensive teacher gaming and cheating in response to the high-powered teacher incentives introduced by President George Bush's No Child Left Behind policy (Levitt and Jacob 2003, Aviv 2014).

[2] See Truth and Reconciliation Commission of South Africa (1998) for South Africa, Amnesty International (2017) for the Philippines, Barbassa (2015) for Brazil, Lohmuller (2015) for El Salvador and Mexico, Oficina de Derechos Humanos del Arzobispado de Guatemala (1998) for Guatemala, and Gibson (2000) and Turse (2013) for the United States.

[3] Authors' translation from Spanish. All other Spanish texts quoted below are also our translation.

Developed country governments are not immune from the temptation to use high-powered incentives to motivate their military. During its long war in Vietnam, the US military explicitly targeted "body count" as the central measure of success for its personnel, and body counts soon became a quota to be met for promotion (Gibson 2000, Turse 2013). Gibson (2000, p. 112), for example, describes the situation as: "Producing a high body count was crucial for promotion in the officer corps. Many high-level officers established 'production quotas' for their units, and systems of 'debit' and 'credit' to calculate exactly how effectively subordinate units and middle-management personnel performed." Turse (2013, pp. 44-45) quotes officers and soldiers who served in Vietnam describing this as: "Your success was measured by your body count. It came down through the channels"; "It was all about body count. Our commanders just wanted body count"; "Get the body count. Get the body count. Get the body count. It was prevalent everywhere. I think it was the mind-set of the officer corps from the top down"; and "In our unit, guys who got confirmed kills would get a three-day in-country R and R [rest and recreation]. Those guys got sent to the beach at Vung Tau." He also describes other incentives including "medals, badges, extra food, extra beer, permission to wear nonregulation gear, and light duty at base camp." As a result of these incentives, again in the words of a soldier, "our mission was not to win territory or seize positions, but simply to kill … Victory was a high body count … This led to such practices as counting civilians as Viet Cong. 'If it's dead and Vietnamese, it's VC' was a rule of thumb in the bush." (Stulberg and Salomone 2013, p. 176). Current US military practice is also not immune to focusing on body count as the measure of success or source of incentive; a similar strategy has been used in the recent counterinsurgency efforts in Afghanistan (e.g., Thompson 2009).

A notable example of institutionalized high-powered incentives for the armed forces is Colombia's recent strategy of intensifying the military campaign against left-wing guerillas. Following his election as president in 2002, Álvaro Uribe expanded the size of the military and strengthened their incentives to fight the guerillas. A major consequence of these high-powered incentives was a surge in "false positives"—the murder of civilians falsely portrayed by the army to be guerilla combatants (Figure 1).[4] False positives had long existed in Colombia, but increased massively following President Uribe's counterinsurgency strategy, and started declining only after media revelations of the extent of civilian killings in 2008. According to the UN High Commissioner for Human Rights, during this period as many as 5,000 innocent civilians may have been executed. Figure 2 shows the distribution of false positives across Colombia, revealing that the practice was widespread throughout the country and not just driven by a few rogue military units.

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[4]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. False positives and some of their causes have been extensively discussed by the press and some nongovernmental organizations, such as the Centro de Investigación y Educación Popular (CINEP), whose data we draw on, the Office of the High Commissioner for Human Rights of the United Nations (Alston 2010), and Human Rights Watch (2015). Figure 1 shows both incidents producing false positives and the number of people killed in these events. In this and Figure 3, we plot a two-period moving average of the raw numbers for ease of inspection.
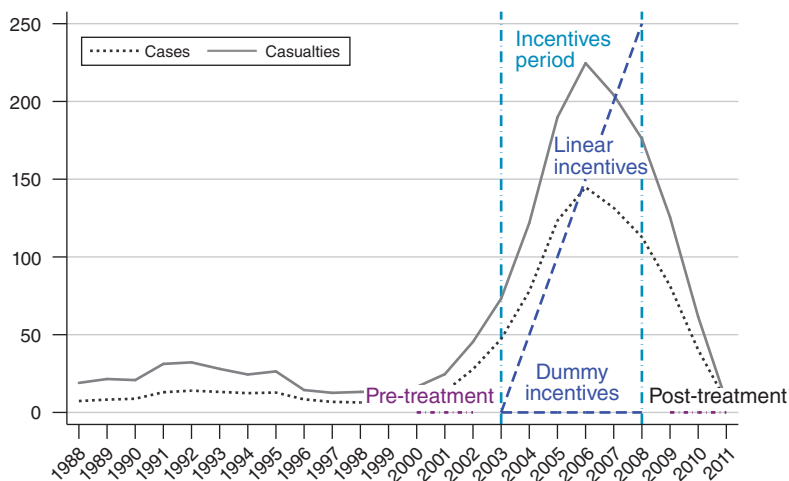
FIGURE 1. FALSE POSITIVES: CASES AND CASUALTIES 1988–2011

Notes: False positives between 1988 and 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 two-year moving average of the raw numbers.

Source: CINEP

The Colombian setting shares with the others mentioned above not only the possibility of extreme costs (in the form of the murder of innocent civilians) from high-powered incentives, but also the weak checks on the unintended consequences of high-powered incentives for the military. Unlawful behavior by soldiers can be prevented either by the military hierarchy or by other branches of the government (such as the judiciary). The weakness of the Colombian state made both types of checks highly imperfect. Officers (in particular, as we argue below, colonels) with powerful career concerns exploited the high-powered incentives for their own gains, while the judiciary was unable to act as a check on the military.

To clarify how judicial institutions can affect the extent of unintended consequences from high-powered incentives for the military, we start with a simple extension of Holmström and Milgrom's (1991) multitasking framework. In our model, agents (army officers) can exert good effort, which produces "true positives" (real nonstate armed actors killed), 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 that guide our empirical work. First, more powerful incentives for military personnel to kill guerillas will not just increase such killings but also fuel false positives. Second, this perverse 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, it 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 impact false positives but
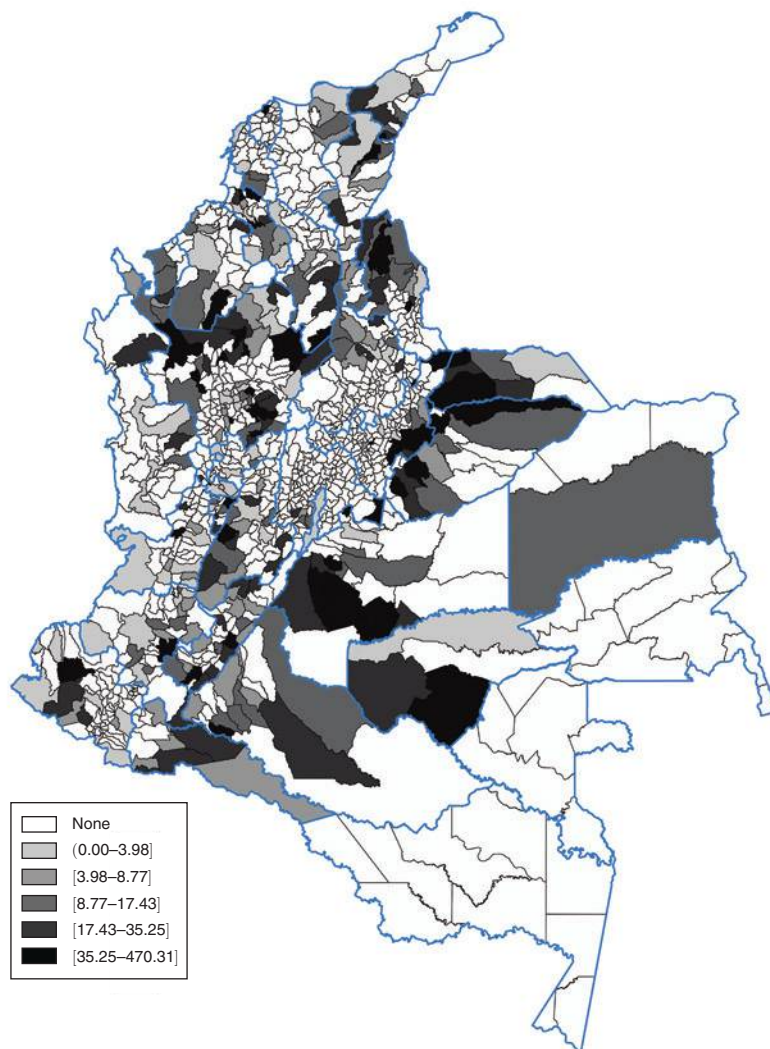
FIGURE 2. FALSE POSITIVES: TOTAL EXECUTIONS PER 100,000 INHABITANTS

Note: False positives per municipality (executions per 100,000 inhabitants) over the entire sample period.

Sources: Own calculations with data from CINEP (false positives) and DANE (population).

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 collateral damage from a successful counterinsurgency strategy, but systematic bad effort by military units directed toward killing civilians and portraying them as guerilla combatants. Finally, when risk aversion and noise in performance measures are limited, the state's agents obtain sufficiently high returns from exerting bad effort so that their overall utility is decreasing in the effectiveness of judicial institutions. Under such circumstances, if they are sufficiently powerful, they may take actions to further weaken the local judiciary. This last prediction underscores

another difference in our conceptual structure from other examples of multitasking: in an environment with already weak institutions, excessively high-powered incentives for the military can further erode the quality of institutions.

To study the unintended consequences from high-powered incentives empirically 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. Though measuring false positives is challenging, we believe that the data available to us are fairly reliable. These data come from the meticulous efforts of a Colombian Jesuit NGO, based on direct reports from the ground, including from the clergy, and detailed analysis of various national and local news sources. These data are unlikely to suffer from the systematic biases of estimates from official sources and victim associations.

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 estimate the impact of the interaction between the share of brigades commanded by colonels and the quality of (initial) local judicial institutions with high-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 high-powered incentives (Figure 1). True positives, in contrast, start increasing sharply several years before the onset of high-powered incentives, in part because of the collapse of the peace process initiated by Uribe's predecessor, Andrés Pastrana; and then decline during the period of high-powered incentives, 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 natural facet 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 pretrends in either false positives or true positives in these areas, suggesting that these municipalities would continue on similar trends in the absence 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 these differential cross-municipality trends disappear after 2008, 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 to no discernible improvement in the security situation (that is, no declines in attacks by either the guerrilla or the paramilitaries).

As already anticipated, our bottom-line conclusion that high-powered incentives without a strong accountability system can backfire coheres with a large
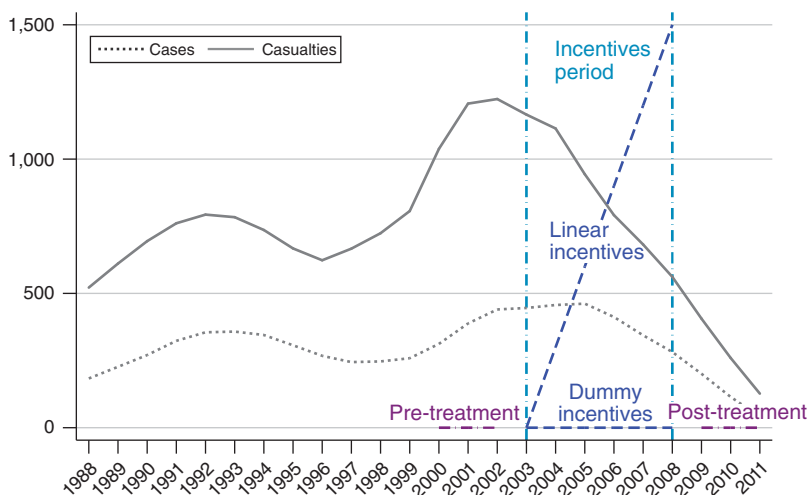
FIGURE 3. TRUE POSITIVES: CASES AND CASUALTIES 1988–2011

Notes: True positives between 1988 and 2011. Cases are 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 two-year moving average of the raw numbers.

Sources: CINEP–Restrepo, Spagat, and Vargas (2004)

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. There are many examples of significant distortions from multitasking-type considerations in the organizational economics literature. Summarizing this evidence, Prendergast (2011, p. 127) 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." Unfortunately, as we will show, this is more or less exactly what happened in the Colombian case. Relative to the aforementioned empirical literature on the unintended consequences of high-powered incentives, we not only provide evidence for multitasking-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; Dal Bó, Finan, and Rossi 2013; Behrman et al. 2015; and Finan, Olken, and Pande 2015) find that strengthening incentives for public servants is generally effective in developing countries, this work typically focuses on environments 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 a study of nurses in the Indian public health care system in Banerjee, Duflo, and Glennerster (2008), 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 utilize them (see also Acemoglu, Kremer, and Mian 2008). It is thus not surprising that the implications

of high-powered incentives for the military parallel their failed applications in other fields.[5]

Our paper is related to a small literature documenting the effectiveness of "winning hearts and minds" in the context of counterinsurgency, including Berman, Shapiro, and Felter (2011); Crost, Felter, and Johnston (2014); and Beath, Fotini, and Enikolopov (2016), and to several recent papers documenting that counterinsurgency strategies centering on military dominance, such as bombing, have had counterproductive effects (e.g., Kocher, Pepinsky, and Kalyvas 2011; Lyall 2014; Dell and Querubín 2018).[6]

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 privileging bonuses at the expense of sanctions or auditing, thus increasing cheating (false positives) by military units. While this interpretation is in line with ours, their paper neither develops the basic comparative static predictions that guide our empirical work nor presents any empirical evidence.

We start in the next section with a brief discussion of the Colombian context. Section II presents our motivating model. Section III describes our data. Section IV presents our empirical strategy and results, and Section V concludes. The online 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.

I. The Colombian Context

Colombia has a long history of civil war and nonstate 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).

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[5] Highlighting another potential downside of military impunity 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.

[6] Ager, Bursztyn, and Voth (2016) also document the consequences of military incentives, in the form of status competition between fighter pilots, with negative unintended consequences including a higher death rate for low-skilled incentivized peers.

The Democratic Security Policy 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 mention the key role played by local judicial institutions. Finally, we discuss the evidence for false positives.

A. *High-Powered Incentives in the Colombian Military*

Uribe's Democratic Security Policy 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, even though in principle rewards were for outside informants, not military personnel, members of the armed forces were not explicitly excluded and received some of these benefits. Second, even though 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 by the president in May 14, 2007, rewarded army members or DAS functionaries 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 on by the president, also fit into the general policy of providing high-powered incentives in the fight against the guerillas.

As we document further in the online 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 the form 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 that "rewarded combat killings with vacation time, promotions, medals, training

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[7]For instance, the directive approved only up to 15 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 Abierta 2011).
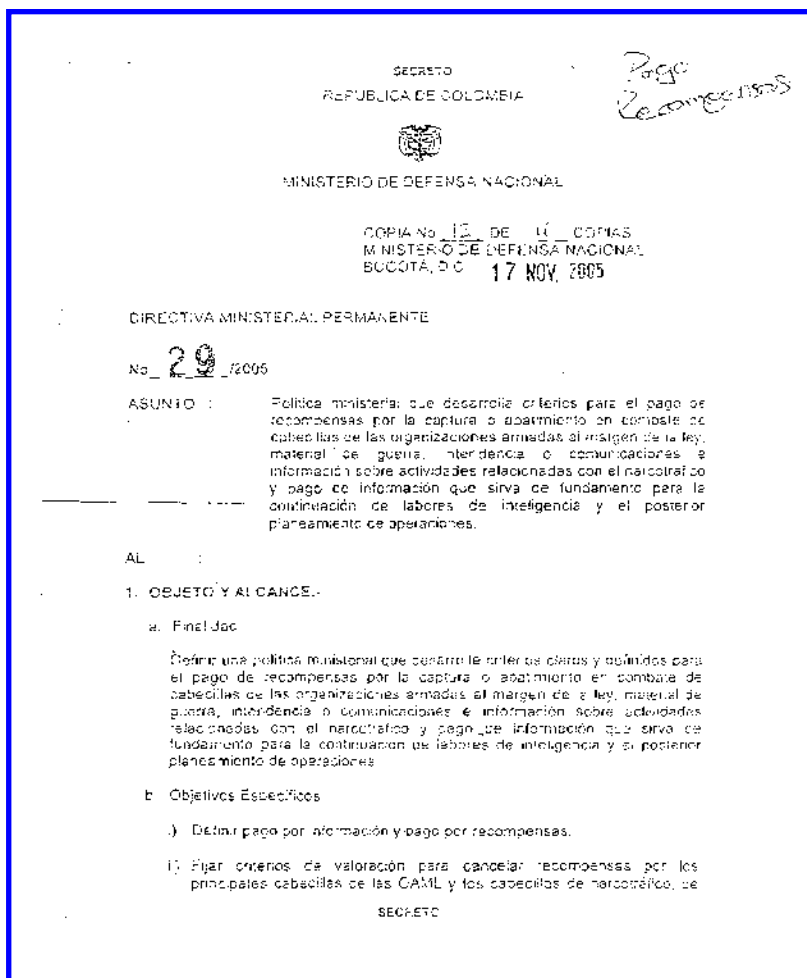
FIGURE 4. "SECRET" ARMY DIRECTIVE 29 OF NOVEMBER 17, 2005, COLOMBIAN MINISTRY OF DEFENSE

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, known as false positives.

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 fourth brigade in 2002 and 2003. Montoya became the head of the army between February 2006 and November 2008. Indeed, Human Rights Watch (2015, p. 1) 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.

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 1°. 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 1°. 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 2°. La Bonificación de que trata este artículo, solo será reconocida y pagada por la participación en la respectiva operación de importancia nacional.

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

ARTÍCULO 2°. 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.

ARTÍCULO 3°. VALOR DE LA BONIFICACIÓN. La bonificación podrá otorgarse hasta por un valor equivalente a doce (12) asignaciones básicas mensuales que perciba el miembro de la Fuerza Pública o el funcionario del Departamento Administrativo de Seguridad - DAS, a quien se le reconozca el derecho a percibir la misma.

El monto de la bonificación será determinado por el Presidente de la República, en el decreto mediante el cual se otorgue la bonificación.

FIGURE 5. PRESIDENTIAL DECREE 1400 OF MAY 5, 2006 OR BOINA BONUSES FOR OPERATIONS OF NATIONAL IMPORTANCE

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 guerillas.

UN Special Rapporteur Phillip 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 29 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). He also 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 online Appendix. Alston (2010, p. 2) further notes "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."

These provisions and other directives creating high-powered incentives for military personnel were ended after 2008 due to mass media attention following the abduction and murder by the army of 22 men in Soacha, a town in the metropolitan area 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, in our empirical work we suppose 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 2008. 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.

B. *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

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[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).

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.[10]

At the beginning of 2014, 4,262 members of the Armed Forces were under investigation for their responsibility in false positives cases. Almost 10 percent of them (401) were army officers, "mainly colonels, majors, captains and lieutenants" with no mention of generals involved.[11] The Human Rights Watch (2015) report cites just 16 active or retired generals under investigation, and while this may partly reflect more impunity toward higher-ranking officials, it also reflects the differential incentives faced by colonels as opposed to generals.

The case study evidence, which we discuss more in the online 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.[12]

While Colombian justice has been slow to prosecute involved officers, the case against General Torres Escalante, the first to be issued an arrest warrant, is particularly revealing. 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.[13] The Attorney General Office argued in this and other similar cases that "killings were not isolated murders by foot soldiers or low-rank individuals, but

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[10] Both former functionaries of the Ministry of Defense and officials in human rights organizations highlighted that the promotion to general is the single most important step in an officer's career. Legislation reflects this too. The Constitution (article 47) gives Congress the duty to approve promotions for generals, not for earlier ranks. Moreover, to become a general an officer must not only have the required seniority (28 years) and approve the courses for promotion, but in addition at least two postgraduate degrees. Finally, while data on the full roster of the army with ranks is not available, the mere structure of the units reveals that the pyramid gets much thinner for duties fulfilled by generals. For instance, there are almost ten times as many battalions (led by colonels) as there are regular brigades (led mostly by generals). The contrast at lower echelons is not as stark: each battalion has about four companies (led by a mayor or captain) and each company has about four platoons (led by a lieutenant or sublieutenant). In short, the step to become a general is both the main bottleneck and more politically significant moment in an officer's career.

[11] "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).

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

[13] "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).

responded to a directive from the top ranks privileging deaths as operational results over captures or demobilizations."[14]

But perhaps the most telling case on the potentially different behavior of colonels and generals is that of Brigade XI. Colonel Borja confessed to committing 57 false positives as commander of a special joint forces unit of this brigade, which was at the time commanded by Colonel Peña Forero. After General Jorge Arturo Salgado took over the control of this brigade in November 2017, he started an investigation of the very large number of killings that had been reported. He uncovered 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, and that Colonel Peña set killing goals and advised his subordinates on how to claim rewards for false positives. Tellingly, after the arrival of General Salgado, reported killings fell from 181 to 60 per year.[15]

The case study evidence supports the interpretation that the patterns we observe are more likely to be driven from differential incentives than differences in the characteristics of the areas under the command of colonels. Indeed, except generals being in charge of larger cities, there are no systematic differences between characteristics of areas with generals or colonels (and we control for distance to the closest major city), and several brigades switch back and forth from generals to colonels.[16] An important exception is provided by the 18 mobile brigades in our sample, which are always led by colonels and operate in difficult areas. We confirm, however, that our estimates are not driven by mobile brigades.

C. *Local Judicial Institutions*

Our theory emphasizes that effective judicial institutions discourage bad effort and false positives because they make extrajudicial killings harder to falsify. This is consistent with the assessment of the UN Special Rapporteur, Phillip Alston, who wrote 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 percent. 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).

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[14] "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).

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

[16] Some examples include: brigade 6 (general from 2000–2005, colonel after 2005), brigade 8 (colonel from 2000 to 2002, general 2003 from 2006, colonel after 2006), brigade 9 (colonel before 2001, general until 2003, colonel from 2004 to 2007, general in 2008, colonel in 2009, general in 2019), among several other cases.

In Colombia, the local branch of the Office of the Attorney General and its Technical Investigation Unit are in charge of the initial investigation of combat killings. When there is an accusation of foul play, the case and the available evidence are transferred to local judges who conduct hearings and gather additional evidence. If one of these branches of the judiciary is corrupt or just ineffective, they are less likely to investigate extrajudicial killings successfully and in a timely manner, and also they may be unable to prevent threats against, and even murders of, key witnesses. The qualitative evidence we present in the online Appendix shows not only that judicial inefficiencies are pervasive in Colombia but also that officers have sometimes worked to actively corrupt and block the judiciary.

D. *Evidence on False Positives*

It is important to underscore that the data we use on false positives indeed correspond to killings of civilians. This issue is discussed in detail in the United Nations and Human Rights Watch reports, and was also extensively covered in the Colombian press. Evidence gathered by judicial authorities and the media allowed them to establish that alleged guerilla killings were, in fact, false positives. For instance, images published by Semana (the main Colombian political magazine) in 2010[17] show that corpses were, somewhat carelessly, set up, simulating combats: victim's fingers were artificially placed on the trigger, and subsequent forensic tests revealed that the weapon was not fired; grenades were dangerously placed on a victim's pockets, where they could easily explode in the midst of combat; a victim wore the right boot on the left foot, and vice versa; and a gun's magazine was stored inside the victim's boot, which would have been extremely uncomfortable during combat.

II. A Model of Intentional and Unintentional False Positives

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

A. *Setup and Assumptions*

Consider the following simple extension of Holmström and Milgrom's (1991) model of multitasking. 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

$$(1) \qquad\qquad q_T = a_T + \varepsilon_T,$$

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

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

$$(2) \qquad q_F = \chi(a_T + \varepsilon_T) + (a_F + \varepsilon_F),$$

$\chi > 0$, and $\varepsilon_F \sim \mathcal{N}(0, \sigma_F^2)$ and independent of ε_T.[18] The first term in equation (2) corresponds to 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.[19]

The observed performance measure for the agent is

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

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*.[20]

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

$$u(w - \Psi(a_T, a_F)) = E\left[-\exp(-\eta(w - \Psi(a_T, a_F)))\right],$$

where η is the coefficient of absolute risk aversion (CARA), and $\Psi(a_T, a_F) = (1/2)(c_T a_T^2 + c_F a_F^2) + \delta a_T a_F$.[21] 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.

---

[18] 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.

[19] 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.

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

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

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.

[21] Here the reward w is inclusive of monetary rewards as well as nonpecuniary 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.

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, 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

(3)
$$\tau + \pi s \big(a_T(1 + \alpha \chi) + \alpha \, a_F \big) - \tfrac{1}{2}\big(c_T a_T^2 + c_F a_F^2 \big)$$

$$- \delta a_T a_F - \frac{\eta (\pi s)^2}{2}\Big((1 + \alpha \chi)^2 \sigma_T^2 + \alpha^2 \sigma_F^2 \Big),$$

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 $\Psi(a_T, a_F) = (1/2)(\sqrt{c_T}\,a_T + \sqrt{c_F}\,a_F)^2$). In the text, we assume that

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

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

B. *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 satisfy

$$a_F^* = 0 \;\Leftrightarrow\; \delta \geq \frac{\alpha}{1 + \alpha \chi}\, c_T \;\equiv\; \underline{\delta_F},$$

$$a_T^* = 0 \;\Leftrightarrow\; \delta \geq \frac{1 + \alpha \chi}{\alpha}\, c_F \;\equiv\; \underline{\delta_T},$$

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, first

suppose that $\underline{\delta_T} < \underline{\delta_F}$, or equivalently $\alpha/\sqrt{c_F} > (1+\alpha\chi)/\sqrt{c_T}$. This implies that $\underline{\delta_T} < \sqrt{c_T c_F} < \underline{\delta_F}$. Then for $\delta \in (0, \underline{\delta_T}]$, the agent chooses $a_T^* > 0$ and $a_F^* > 0$, while if $\delta \in (\underline{\delta_T}, \sqrt{c_T c_F})$, 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:

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

$$(5) \qquad a_T^* = \begin{cases} \pi s \dfrac{(1+\alpha\chi)c_F - \delta\alpha}{c_T c_F - \delta^2} & \text{if } \delta < \min\{\underline{\delta_F}, \underline{\delta_T}\} \\[2ex] \pi s \dfrac{1+\alpha\chi}{c_T} & \text{if } \underline{\delta_F} < \delta < \underline{\delta_T} < \sqrt{c_T c_F}. \\[2ex] 0 & \text{if } \underline{\delta_T} < \delta < \sqrt{c_F c_T} < \underline{\delta_F} \end{cases}$$

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 $E[\exp(q_T^*)]$, and false positives given by $E[\exp(q_F^*)]$ (the results for $E[q_T^*]$ and $E[q_F^*]$ are identical as we show in the online Appendix, Section A.5). More specifically, these quantities can be computed as

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

and

$$(7) \qquad E[\exp(q_F^*)] = E[\exp(a_F^* + \varepsilon_F) + \chi(a_T^* + \varepsilon_T)]$$

$$= \exp(\chi a_T^* + a_F^*)\exp\left(\frac{\chi^2 \sigma_T^2 + \sigma_F^2}{2}\right),$$

where the last equalities in both expressions use 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:*

(i) *weakly increases true and false positives, i.e.,*

$$\frac{\partial E\big[\exp(q_T^*)\big]}{\partial s} \geq 0 \ and \ \frac{\partial E\big[\exp(q_T^*)\big]}{\partial s} = 0 \quad if \ and \ only \ if \ a_T^* = 0,$$

$$\frac{\partial E\big[\exp(q_F^*)\big]}{\partial s} \geq 0 \ and \ \frac{\partial E\big[\exp(q_F^*)\big]}{\partial s} = 0 \quad if \ and \ only \ if \ a_F^* = 0 \ and \ \chi = 0;$$

(ii) *leads to (weakly) greater increases in true and false positives where reported output is a more important part of compensation (higher π or stronger career concerns), i.e.,*

$$\frac{\partial^2 E\big[\exp(q_T^*)\big]}{\partial s\partial\pi} \geq 0 \ and \ \frac{\partial^2 E\big[\exp(q_T^*)\big]}{\partial s\partial\pi} = 0 \quad if \ and \ only \ if \ a_T^* = 0,$$

$$\frac{\partial^2 E\big[\exp(q_F^*)\big]}{\partial s\partial\pi} \geq 0 \ and \ \frac{\partial^2 E\big[\exp(q_F^*)\big]}{\partial s\partial\pi} = 0 \quad if \ and \ only \ if \ a_F^* = 0 \ and \ \chi = 0;$$

(iii) *leads to (weakly) greater increases in false positives where misrepresentation of false positives is more likely (higher α), i.e.,*

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

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

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

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 $E\big[\exp(q_F^*)\big] > 0$ and that the derivatives, $\partial a_J^*/\partial s$, $\partial a_J^*/\partial\pi$, $\partial^2 a_J^*/\partial s\partial\pi$, 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 $\partial a_T^*/\partial s = a_T^*/s$

and $\partial^2 a_T^* / \partial s \partial \alpha = (1/s)(\partial a_T^* / \partial \alpha)$. After substituting and simplifying, we can write

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

which leads to the stated condition.

For false positives, we have

$$
\frac{\partial^2 E\left[\exp(q_F^*)\right]}{\partial s \partial \alpha}
$$

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

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 $\partial^2 E\left[\exp(q_F^*)\right]/\partial \alpha \partial s = 0$. 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

$$
\left(\chi \frac{\partial^2 a_T^*}{\partial s \partial \alpha} + \frac{\partial^2 a_F^*}{\partial s \partial \alpha}\right) = \frac{1}{s}\left(\chi \frac{\partial a_T^*}{\partial \alpha} + \frac{\partial a_F^*}{\partial \alpha}\right)
$$

$$
= \frac{\pi}{c_F c_T - \delta^2}\left[\left(\chi \sqrt{c_F} - \sqrt{c_T}\right)^2 + 2\chi\left(\sqrt{c_F c_T} - \delta\right)\right] > 0,
$$

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. Nevertheless, when this collateral damage effect is small (because χ is small), and when there is sufficient substitutability between the two types of efforts, in contrast to false positives, 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 they give 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 toward more bad effort targeted toward 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.[22]

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.[23]

PROPOSITION 2: (Implications for institutions). *Consider the agent's equilibrium payoff* $u(a_T^*, a_F^*)$. *Suppose that* $\delta < \min\{\underline{\delta_T}, \underline{\delta_F}\}$, *so that an interior solution exists. Then*

$$\frac{\partial u}{\partial \alpha} = \pi s\Big[\chi\, a_T^* + a_F^* - \eta\pi s\big((1+\alpha\chi)\chi\,\sigma_T^2 + \alpha\,\sigma_F^2\big)\Big] \lessgtr 0,$$

$$\frac{\partial^2 u}{\partial \alpha \partial s} = 2\pi\Big[\chi\, a_T^* + a_F^* - \eta\pi s\big((1+\alpha\chi)\chi\,\sigma_T^2 + \alpha\,\sigma_F^2\big)\Big] \lessgtr 0,$$

$$\frac{\partial^3 u}{\partial \alpha \partial s \partial \pi} = 4\Big[\chi\, a_T^* + a_F^* - \eta\pi s\big((1+\alpha\chi)\chi\,\sigma_T^2 + \alpha\,\sigma_F^2\big)\Big] \lessgtr 0.$$

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

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

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

---

[22] In the online 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.

[23] 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.

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 $\pi(\partial a_J^*/\partial \pi) = a_J^*$ and $s(\partial a_J^*/\partial s) = a_J^*$ 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).

These predictions motivate our empirical strategy. Nevertheless, two alternative interpretations may lead to similar patterns. First, colonels may not be as good as generals in monitoring their troops, who may then engage in extrajudicial killings without their commander's approval. Second, generals may be better at concealing evidence of extrajudicial killings and avoiding prosecution, or may even scapegoat colonels. The qualitative evidence we present in online Appendix A.1 does not support these interpretations. There is no evidence that commanders were trying but unable to control their troops (rather, they were often the ones encouraging their troops to engage in extrajudicial killings). Since we are using data from the CINEP based on detailed local information, not official statistics, the concerns about colonels being scapegoated are less relevant.

III. Data and Descriptive Statistics

A. *Data*

Our key dependent variables are the number of false and true positives in a given municipality and year. 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.[24]

As already mentioned above, we define false positives as killings of civilians by the government that 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*). 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 was declared to be a member of the guerillas, the paramilitaries, or an "unknown" rebel group; whether the perpetrators were 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 year.[25]

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 (*Judicial Inefficiency$_{m,0}$*) and a time-varying measure (*Judicial Inefficiency$_{m,t}$*):

$$Judicial\ Inefficiency_{m,0} = \frac{\sum_{t=1995}^{1999} Complaints\ against\ judicial\ functionaries_{m,t}}{\sum_{t=1995}^{1999} All\ complaints_{m,t}},$$

$$Judicial\ Inefficiency_{m,t} = \frac{Complaints\ against\ judicial\ functionaries_{m,t}}{All\ complaints_{m,t}},$$

$$t \in \{2000, \dots, 2010\}.$$

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[24] The time series patterns of our true positives data, depicted in Figure 3, do not coincide with official statistics from the Ministry of Defense, which instead point to a more persistent increase in the numbers of guerillas killed by the army during the period from 2002 to 2007. There are several reasons, however, for not trusting the official statistics (see, e.g., Grupo de Memoria Histórica 2013, chap. 1). In addition to the presence of false positives, Otero Prada (2008) points out that the official numbers, combined with the numbers of demobilized and captured guerilla, produce hugely unrealistic totals relative to the estimated sizes of guerilla fighters. For example, just from 2002 to 2007 they imply that more than 50,000 guerilla members were killed or captured, or demobilized, leading to a much larger number than the estimate of 15,000 guerilla members around this time. These exaggerated numbers likely reflect a desire to "convey a sense of success in the fight against insurgency" (Otero Prada 2008, 21).

[25] 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 (compared to more than 4,000 possible victims mentioned in newspapers based on judicial investigations). This conservative coding implies that misclassification of true positives as false positives is very unlikely to 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).

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 nonclassical 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 that 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 online 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 Wayback 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 its 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 yearly basis.[26]

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

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[26] Because promotions and commander appointments are typically done at the beginning of the year or midway through the year, when the rank of a brigade's commander varies within a year we take the average rank.

TABLE 1—DESCRIPTIVE STATISTICS: TIME-VARYING VARIABLES

	Before 2003		2003 to 2008		After 2008	
Variable	Mean	Standard deviation	Mean	Standard deviation	Mean	Standard deviation
False positives:						
Cases	0.010	0.144	0.132	0.623	0.007	0.085
Casualties	0.022	0.288	0.207	1.018	0.008	0.123
True positives:						
Cases	0.410	1.024	0.440	1.243	0.058	0.331
Casualties	1.267	6.030	0.868	2.758	0.185	1.771
Judicial inefficiency	0.071	0.145	0.069	0.126	0.070	0.128
Colonel in charge (weighted share)	0.106	0.308	0.224	0.404	0.443	0.490
Colonel in charge (unweighted share)	0.108	0.309	0.248	0.426	0.457	0.493
Colonel in charge (dummy)	0.110	0.313	0.256	0.437	0.467	0.499
Guerilla attacks	0.957	2.209	0.325	1.062	0.220	0.746
Paramilitary attacks	0.211	0.715	0.085	0.503	0.313	2.920
Government attacks	0.113	0.441	0.150	0.743	0.114	0.451

Notes: Data from 2000 to 2010. False positives cases are the number of instances where civilians are killed to be presented as rebels in a given municipality and year, 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.

with initial paramilitary attacks, guerilla attacks, and the unemployment rate in the municipality.[27]

B. *Descriptive Statistics*

Table 1 presents descriptive statistics for the main variables in our analysis, before, during, and after our "incentives" period. Confirming the patterns visible in Figure 1, false positives show a remarkable increase. From just 0.01 cases per year and municipality before 2003, the average incidence of events involving the killings of civilians who were then subsequently purported to be combatants rises an order of magnitude, to 0.132, during the incentive period (2003–2008), and falls again to 0.007 once incentives were removed. We see the same trend in the casualties involved in false positive incidents. There were on average 0.022 deaths per year before 2003 (with a maximum of 7 killings), and 0.207 deaths per year from 2003 to 2008 (with a maximum of 20 deaths). In contrast, as already shown in Figure 1, episodes producing true positives, while much more frequent, are roughly constant over the incentive period (0.41 cases per year and municipality on average

[27] 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). Online Appendix Table A-1 lists all variables in the analysis, describing their definition and sources. An additional online Appendix provides further details on our reconstruction of army ranks and brigades jurisdictions.

before 2003, 0.44 from 2003 to 2008) and even declining in terms of the number killed (1.267 average deaths before 2003 and 0.868 from 2003 to 2008). However, both true positives cases (0.058) and casualties (0.185) fall when incentives were removed (after 2008).[28] The judicial inefficiency index similarly shows no marked change, on average, before and after the incentives period, with judicial complaints representing 7.1 percent of the total before 2003, 6.9 percent during the incentive period, and 7 percent after 2008. Nevertheless, as our regressions below indicate, there is a relative worsening of this index in colonel-led areas.

Turning to the colonel variables, the average weighted share of brigades in a municipality having a colonel as commander is 10.6 percent before 2003, rising to 22.4 percent during the incentive period (and continuing to grow to 44.3 percent after 2008). Figures for the unweighted share are similar, and the dummy variable indicating the presence of any colonel-led brigade in the municipality rises from 11 percent before 2003 to 25.3 percent during the incentive period. 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 0.957 to 0.325 to 0.220 per year before, during, and after the incentive period), paramilitaries (similarly declining from 0.211 to 0.085 and then rising to 0.313), and the government (with a small increase from 0.113 to 0.150 and then falling slightly to 0.114).

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

IV. Results

Figures 1 and 3 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. However, this time-series evidence cannot be directly used to answer our main questions or mapped to our theoretical framework because of confounding events impacting false or true positives, such as the guerilla withdrawing to remoter areas following the collapse of President Pastrana's peace process. Our main evidence, instead, comes from the longitudinal 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.

A. *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 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

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[28] 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, 2002.

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 aimed at deliberately killing and then disguising civilians. For this we will exploit the result that while false positives should also respond more to high-powered incentives in areas with weaker judicial institutions, the same is not true for true positives.[29]

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 year:[30]

$$
\begin{aligned}
(8) \qquad y_{m,t} = {} & \nu^{Pre,Col}\big(Pre \times Colonel_{m,t}\big) + \beta^{Col}\big(Incentives_t \times Colonel_{m,t}\big) \\
& + \nu^{Post,Col}\big(Post \times Colonel_{m,t}\big) + \nu^{Pre,Jud}\big(Pre \times J.\ Ineff_{m,0}\big) \\
& + \beta^{Jud}\big(Incentives_t \times J.\ Ineff_{m,0}\big) + \nu^{Post,Jud}\big(Post \times J.\ Ineff_{m,0}\big) \\
& + \varphi\, Colonel_{m,t} + \delta_m + \gamma_t + \sum_{x \in \mathbf{X}_m} \Phi_x\big(x \times \gamma_t\big) + \varepsilon_{m,t}.
\end{aligned}
$$

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-year observations in which false positives or true positives are equal to zero.[31] The term $Colonel_{m,t}$ is the share of brigades with jurisdiction over m that are commanded by colonels, while $J.\ Ineff_{m,0}$ 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 2003 to 2008) or a linear trend for this period. This latter parameterization attempts to capture both the intensification of high-powered incentives and the potentially cumulative effects of these policies while they were in effect. Throughout, we also always include an interaction with the year before the period in which the incentives are in effect (2002, denoted by "Pre" in the expression), which will act as a simple test for whether there

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[29] Because, as highlighted by our theoretical framework, judicial inefficiency is endogenous to incentives and career concerns of colonels, we use baseline levels of judicial inefficiency before the period of high-powered incentives (hence the subscript 0 in (8)).

[30] We only know the location of false positives, not directly which brigade may have committed the killing. Moreover, we only have 13 officers (out of 158) who commanded a brigade both as colonel and general, limiting the sample size for longitudinal analysis. As a result, it is impossible for us to control for commander fixed effects.

[31] In the online 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 $\ln\big(x + \sqrt{1 + x^2}\big)$ 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 $\ln(x)$). These specifications are motivated by the fact that we are unable to estimate nonlinear count models because of the size of the dataset and the large number of right-hand side variables included.

are pretrends in municipalities where the brigades are commanded by colonels and where judicial institutions are weak. We also investigate the role of preexisting trends by including municipality-specific time trends in our robustness checks. In some specifications we also include an additional interaction with the year after the period of incentives (denoted by "Post") to see if once the government reverses its policy of high-powered incentives with little oversight, false positives show a sharp decline, and what the impact of this change in incentives 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 (year) interactions with a rich set of time-invariant municipality characteristics, which were described in Section III. 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.

We also investigate, in online Appendix Table A-3, the relationship between the assignment of colonels and the 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 year and municipality dummies on the right-hand side, has an R^2 of 0.488. When the time-invariant characteristics interacted with the pre, incentives, and post time periods are added, the R^2 increases by an additional 0.0136, to 0.5016, which is quite modest. When the judicial inefficiency variables are added, there is a further increase of just 0.0008, which is again very modest. This evidence thus suggests that there is no strong correlation between the assignment of colonels and municipality characteristics, especially judicial institutions. Other important area characteristics, such as levels of conflict (paramilitary and guerilla attacks), are not significant in these regressions, which is reassuring against concerns that more aggressive brigades or more inexperienced commanders were systematically assigned to places with greater conflict.

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. We find no systematic preexisting trends, suggesting that judicial inefficiency and the assignment of colonels to different jurisdictions was not systematically related to the difficulty of fighting the guerilas.

Finally, throughout, all standard errors are corrected for spatial and first-order temporal autocorrelation, following Conley (1999, 2008).[32]

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[32] Specifically, we allow spatial correlation to extend to up to 279 km from a municipality's centroid to ensure that each municipality has at least one neighbor. The average distance between pairs of regular brigades in our sample is 279 km, so this strategy allows for significant correlation between true and false positives across neighboring brigades. We also report very similar results using clustering at the municipality level in online Appendix Tables A–4, A–15, and A–16.

TABLE 2—FALSE POSITIVES, COLONELS, AND JUDICIAL INEFFICIENCY, 2000–2010: BASELINE RESULTS

	Incentives dummy		Incentives linear		Incentives dummy		Incentives linear	
	Cases (1)	Casualties (2)	Cases (3)	Casualties (4)	Cases (5)	Casualties (6)	Cases (7)	Casualties (8)
Dependent variable is $\log(1 + \textit{false positives})$								
Judicial inefficiency								
… × 2002	−0.006	0.032	−0.014	0.028	−0.003	0.033	−0.014	0.027
	(0.056)	(0.073)	(0.057)	(0.074)	(0.057)	(0.075)	(0.058)	(0.075)
… × incentives	0.159	0.215	0.042	0.060	0.162	0.216	0.042	0.060
(2003–2008)	(0.039)	(0.050)	(0.011)	(0.015)	(0.040)	(0.052)	(0.011)	(0.015)
… × 2009					0.011	0.002	0.001	−0.003
					(0.048)	(0.053)	(0.049)	(0.054)
Colonel in charge (share)								
… × 2002	−0.007	−0.022	−0.004	−0.019	−0.007	−0.021	−0.002	−0.016
	(0.015)	(0.023)	(0.015)	(0.022)	(0.015)	(0.023)	(0.015)	(0.023)
… × incentives	0.032	0.046	0.008	0.011	0.032	0.047	0.009	0.012
(2003–2008)	(0.016)	(0.021)	(0.003)	(0.005)	(0.016)	(0.022)	(0.004)	(0.005)
… × 2009					0.000	0.003	0.005	0.007
					(0.012)	(0.015)	(0.013)	(0.016)
Controls × time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	9,823	9,823	9,823	9,823	9,823	9,823	9,823	9,823
Municipalities	893	893	893	893	893	893	893	893
R^2	0.094	0.091	0.095	0.092	0.094	0.091	0.095	0.092

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (columns 1, 2, 5, and 6) or a linear trend (columns 3, 4, 7, and 8), both for the period from 2003 to 2008. Time dummies are interacted with the following set of time-invariant predetermined municipal controls: quartic polynomial for 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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.

B. *Main Results*

Table 2 shows our baseline results when estimating 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 2009, the year 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 percent or less with the colonels and at 1 percent or less judicial inefficiency. The positive coefficients indicate that during the period of high-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 pretrends. Finally, columns 5–8 show that there is no evidence that

these differential effects survived to the years after the end of the high-powered incentives. This last result is reassuring against the concern that our results may be confounded by the mechanical effects of colonel-led brigades that became more numerous after 2003 and may tend to generate more false positives even without additional incentives (for example, because colonels are less experienced, less good at keeping discipline, or less able to conceal extrajudicial killings). In particular, it shows that once incentives ceased, false positives sharply declined despite the continued presence of colonel-led brigades.[33]

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 high-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 consequential for the purposes of distinguishing the pure collateral damage story from the switch to bad effort scenario is that there is no evidence of an increase in true positives during this period in municipalities with weak judicial institutions—the interaction between judicial inefficiency and the incentive variable is not significant and has the opposite sign to that predicted by a pure collateral story. In addition, there is again no indication of pretrends 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.[34]

The magnitudes of the coefficient estimates in Tables 2 and 3 are not transparent 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 generals 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 about 6.5 percent (estimates ranging from 6.01 to 7.07 percent), while getting rid of judicial inefficiency has a slightly larger impact (ranging from 9.48 to 10.40 percent). We further find that both of these exercises have much smaller effects on true positives, ranging from a decline of 1.89 to 1.97 percent for getting rid of colonels and an increase from 0.84 to 2.07 percent for getting rid of judicial

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[33] Our case study evidence, presented in online Appendix A.3, also does not support these concerns. Moreover commander tenure in our sample is 1.56 years on average (and the largest tenure is five years), which further alleviates concerns that our results are confounded by significant experience effects.

[34] In online Appendix Figure A-1, we also present an event study analysis of the introduction of high-powered incentives. These estimates, exploiting yearly variation, are less precise, but the pattern shown in these figures is consistent with our regression evidence. In particular, for false positives we find that the interactions between judicial inefficiency and year effects turn positive immediately after high-powered incentives are introduced in 2003 and revert back to zero after they are removed. The pattern for the interactions with colonels is less clear and noisier because there is a large negative estimate in 2004. From 2005 onwards, the interactions become positive and again revert to zero after 2009. The negative values early in the incentive period are driven by outliers, and, in fact, in 2003 and 2004 there were very few brigades led by colonels: our colonel share is smaller than 1 percent in 2003 and 2004, and then increases to 30 percent in 2005 and stays above this level until 2010. The limited variation in colonel-led brigades during these two years has modest influence on the overall difference-in-differences estimates, as can be verified in online Appendix Table A-5, which omits years with less than 1 percent colonel share and produces very similar results to our baseline estimates. For true positives, the results are similar to the regression estimates: there is no evidence of significant interactions with judicial inefficiency (consistent with our theoretical interpretation) and positive interactions with colonels after 2003, though these return to zero before 2009. Overall, the event study evidence is consistent with our regression analysis, even if generally less precisely estimated.

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								
… × 2002	0.117	0.088	0.117	0.128	0.092	0.031	0.098	0.097
	(0.137)	(0.228)	(0.133)	(0.217)	(0.146)	(0.246)	(0.137)	(0.226)
… × incentives	−0.061	−0.163	−0.017	−0.027	−0.086	−0.220	−0.022	−0.034
(2003–2008)	(0.088)	(0.144)	(0.019)	(0.030)	(0.101)	(0.171)	(0.021)	(0.033)
… × 2009					−0.102	−0.227	−0.095	−0.162
					(0.135)	(0.238)	(0.126)	(0.218)
Colonel in charge (share)								
… × 2002	−0.001	−0.073	0.001	−0.070	−0.002	−0.070	0.001	−0.067
	(0.047)	(0.067)	(0.047)	(0.067)	(0.047)	(0.069)	(0.047)	(0.068)
… × incentives	0.043	0.056	0.010	0.013	0.042	0.059	0.010	0.014
(2003–2008)	(0.024)	(0.033)	(0.005)	(0.007)	(0.027)	(0.038)	(0.005)	(0.008)
… × 2009					−0.002	0.007	0.000	0.010
					(0.025)	(0.040)	(0.025)	(0.040)
Controls × time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	9,823	9,823	9,823	9,823	9,823	9,823	9,823	9,823
Municipalities	893	893	893	893	893	893	893	893
R^2	0.085	0.082	0.085	0.082	0.085	0.082	0.085	0.082

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (columns 1, 2, 5, and 6) or a linear trend (columns 3, 4, 7, and 8), both for the period from 2003 to 2008. Time dummies are interacted with the following set of time-invariant predetermined municipal controls: quartic polynomial for 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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.

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 strongest career concerns would have had little cost in terms of combating the guerillas, but would have saved a significant number of innocent civilian lives.[35] These counterfactuals do not inform us about the implications of not having the high-powered incentives in place; these effects 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.

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

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 (1)	Casualties (2)	Cases (3)	Casualties (4)	Cases (5)	Casualties (6)	Cases (7)	Casualties (8)
Observed (false)	750	1,181	750	1,181	750	1,181	750	1,181
Observed (true)	3,560	8,373	3,560	8,373	3,560	8,373	3,560	8,373
Panel A. Dependent variable is $\log(1 + false\ positives)$								
Judicial inefficiency to minimum								
Predicted	−76	−112	−72	−113	−78	−112	−72	−113
Percent change	−10.13	−9.48	−9.60	−9.57	−10.40	−9.48	−9.60	−9.57
Colonels to generals								
Predicted	−52	−72	−53	−73	−52	−71	−51	−71
Percent change	−6.93	−6.10	−7.07	−6.18	−6.93	−6.01	−6.80	−6.01
Panel B. Dependent variable is $\log(1 + true\ positives)$								
Judicial inefficiency to minimum								
Predicted	37	128	36	70	52	173	44	88
Percent change	1.04	1.53	1.01	0.84	1.46	2.07	1.24	1.05
Colonels to generals								
Predicted	−69	−165	−68	−163	−69	−161	−68	−158
Percent change	−1.94	−1.97	−1.91	−1.95	−1.94	−1.92	−1.91	−1.89

Notes: Using the corresponding regressions in Tables 2 and 3, we compute the predicted 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 number of false (panel A) or true (panel B) positives, and the second line shows the percent change relative to observed false or true positives. All controls are used in the simulation regardless of significance.

C. *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. These specifications show similar results to our baseline, confirming that our findings are not likely to reflect other correlated trends in places with differential presence of colonels or judicial inefficiency. The coefficient estimates for the interactions with judicial inefficiency in the regressions for false positives are very comparable to those in Table 2 and strongly significant. Those for the interactions with share of colonels are about 30 percent to 50 percent smaller depending of the specification but still statistically significant with the linear specification for incentives. We also see no significant pretrends coefficients, and the interactions for share of colonels in the regressions for true positives are insignificant.

Table 6 probes the robustness of our results in a different dimension—by dropping outliers. Specifically, we drop all municipality-year observations that are below the 2.5th or above the 97.5th percentile in the distribution of residuals in our baseline regressions. The qualitative nature of the results changes very little, and the

TABLE 5—FALSE POSITIVES, TRUE POSITIVES, COLONELS, AND JUDICIAL INEFFICIENCY,
2000–2010: MUNICIPALITY-SPECIFIC TRENDS

	False positives				True positives			
	Incentives dummy		Incentives linear		Incentives dummy		Incentives linear	
	Cases (1)	Casualties (2)	Cases (3)	Casualties (4)	Cases (5)	Casualties (6)	Cases (7)	Casualties (8)
Dependent variable is log(1 + *false or true positives*)								
Judicial inefficiency								
… × 2002	0.008	0.059	−0.020	0.025	0.107	0.060	0.106	0.101
	(0.056)	(0.074)	(0.056)	(0.074)	(0.155)	(0.265)	(0.149)	(0.251)
… × incentives	0.153	0.207	0.042	0.059	−0.056	−0.156	−0.025	−0.036
(2003–2008)	(0.039)	(0.049)	(0.011)	(0.014)	(0.082)	(0.126)	(0.020)	(0.028)
Colonel in charge (share)								
… × 2002	−0.005	−0.018	−0.003	−0.016	0.032	−0.030	0.030	−0.033
	(0.011)	(0.016)	(0.011)	(0.016)	(0.046)	(0.066)	(0.046)	(0.066)
… × incentives	0.023	0.033	0.007	0.009	0.039	0.043	0.008	0.008
(2003–2008)	(0.017)	(0.022)	(0.003)	(0.005)	(0.024)	(0.033)	(0.006)	(0.009)
Controls × time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	9,823	9,823	9,823	9,823	9,823	9,823	9,823	9,823
Municipalities	893	893	893	893	893	893	893	893
R^2	0.151	0.150	0.152	0.151	0.190	0.204	0.190	0.205

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (columns 1, 2, 5, and 6) or a linear trend (columns 3, 4, 7, and 8), both for the period from 2003 to 2008. Time dummies are interacted with the following set of time-invariant predetermined municipal controls: quartic polynomial for 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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.

parameter estimates are somewhat larger for the key interactions for false positives and also 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 pretrends except for the true positive specifications using casualties.

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 high-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 and 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 given (polynomial) 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 toward killing the guerilla.

Table 6—False and True Positives, Colonels, and Judicial Inefficiency, 2000–2010: Removing Outliers

	False positives				True positives			
	Incentives dummy		Incentives linear		Incentives dummy		Incentives linear	
	Cases (1)	Casualties (2)	Cases (3)	Casualties (4)	Cases (5)	Casualties (6)	Cases (7)	Casualties (8)
Dependent variable is $\log(1 + false\ or\ true\ positives)$								
Judicial inefficiency								
… × 2002	−0.006	0.033	−0.006	0.035	0.118	0.074	0.100	0.114
	(0.053)	(0.074)	(0.053)	(0.074)	(0.126)	(0.178)	(0.121)	(0.167)
… × incentives	0.159	0.228	0.041	0.061	−0.011	−0.150	−0.011	−0.021
(2003–2008)	(0.039)	(0.051)	(0.011)	(0.015)	(0.082)	(0.139)	(0.018)	(0.029)
Colonel in charge (share)								
… × 2002	−0.009	−0.022	−0.005	−0.019	0.004	0.079	0.006	0.095
	(0.014)	(0.022)	(0.014)	(0.022)	(0.048)	(0.047)	(0.048)	(0.051)
… × incentives	0.035	0.053	0.009	0.012	0.046	0.058	0.010	0.012
(2003–2008)	(0.015)	(0.020)	(0.003)	(0.004)	(0.024)	(0.032)	(0.005)	(0.007)
Controls × time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	9,332	9,332	9,332	9,332	9,332	9,332	9,332	9,332
Municipalities	893	893	893	893	893	893	893	893

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (columns 1, 2, 5, and 6) or a linear trend (columns 3, 4, 7, and 8), both for the period from 2003 to 2008. Time dummies are interacted with the following set of time-invariant predetermined municipal controls: quartic polynomial for 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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 2.5th and above the 97.5th percentile in the distribution of estimation residuals from our baseline regressions.

In addition to these robustness checks reported in the text, in the online Appendix we show that the results are robust 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; when our dependent variable is cases or casualties in levels or a dummy for the presence of any cases of casualties rather than log counts; and when we control linearly for population on the right-hand side instead of using a quartic polynomial as in our baseline.[36] The interaction with colonels remains positive, but is typically smaller in magnitude and not statistically significant when we do not include any covariates in our baseline specifications, which we interpret as reflecting the differential behavior of false and true positives in areas with different characteristics, an

[36] 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.

Table 7—False Positives, Colonels, and Judicial Inefficiency, 2000–2010: Controlling for Collateral Damage

	Incentives dummy		Incentives linear		Incentives dummy		Incentives linear	
	Cases (1)	Casualties (2)	Cases (3)	Casualties (4)	Cases (5)	Casualties (6)	Cases (7)	Casualties (8)
Dependent variable is $\log(1 + \text{false positives})$								
Judicial inefficiency								
… × 2002	−0.024	0.022	−0.034	0.016	−0.020	0.026	−0.034	0.017
	(0.050)	(0.072)	(0.051)	(0.073)	(0.051)	(0.073)	(0.052)	(0.074)
… × incentives	0.168	0.222	0.043	0.061	0.172	0.226	0.043	0.061
(2003–2008)	(0.039)	(0.049)	(0.011)	(0.015)	(0.040)	(0.051)	(0.011)	(0.015)
… × 2009					0.017	0.015	0.003	0.007
					(0.046)	(0.050)	(0.047)	(0.052)
Colonel in charge (share)								
… × 2002	−0.008	−0.021	−0.005	−0.018	−0.008	−0.021	−0.003	−0.016
	(0.015)	(0.022)	(0.015)	(0.022)	(0.015)	(0.023)	(0.016)	(0.023)
… × incentives	0.029	0.043	0.008	0.011	0.029	0.044	0.008	0.011
(2003–2008)	(0.015)	(0.020)	(0.003)	(0.004)	(0.016)	(0.021)	(0.004)	(0.005)
… × 2009					−0.000	0.003	0.004	0.007
					(0.012)	(0.014)	(0.013)	(0.016)
True positives polynomial								
True positives	0.101	0.145	0.103	0.145	0.101	0.145	0.102	0.145
	(0.088)	(0.058)	(0.088)	(0.058)	(0.088)	(0.058)	(0.088)	(0.058)
(True positives)^2	−0.052	−0.134	−0.057	−0.135	−0.053	−0.134	−0.057	−0.135
	(0.192)	(0.078)	(0.192)	(0.078)	(0.192)	(0.078)	(0.192)	(0.078)
(True positives)^3	−0.006	0.054	−0.003	0.054	−0.006	0.054	−0.003	0.054
	(0.123)	(0.030)	(0.123)	(0.030)	(0.123)	(0.030)	(0.123)	(0.030)
(True positives)^4	0.013	−0.006	0.012	−0.006	0.013	−0.006	0.012	−0.006
	(0.023)	(0.003)	(0.023)	(0.003)	(0.023)	(0.003)	(0.023)	(0.003)
Controls × time effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	9,823	9,823	9,823	9,823	9,823	9,823	9,823	9,823
Municipalities	893	893	893	893	893	893	893	893
R^2	0.114	0.101	0.115	0.102	0.114	0.101	0.115	0.102

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (columns 1, 2, 5, and 6) or a linear trend (columns 3, 4, 7, and 8), both for the period from 2003 to 2008. Time dummies are interacted with the following set of time-invariant predetermined municipal controls: quartic polynomial for 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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.

interpretation that also receives support from the fact that without covariates there is evidence of some pretrends as well. In addition, we investigated whether false positives are driven solely by mobile brigades by separately including the share of colonel-led regular and the share of mobile brigades (see online Appendix Table A-12). We find that both types of brigades are associated with greater true and false positives, suggesting that our results are not driven just by mobile

brigades.[37] We also verified the robustness of our baseline specification to including the total number of brigades interacted with time effects as an additional control, indicating that the importance of colonels is not driven simply by competition between more brigades within a jurisdiction.

We also investigated whether there are significant spillovers from the career concern-induced incentives in neighboring municipalities. This 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, and we found no significant effects from incentives or judicial institutions of neighboring areas on false or true positives in a given municipality.

Finally, while our preferred measure of judicial inefficiency normalizes judicial complaints by total complaints, thus removing the propensity to differentially report judicial problems across municipalities (see Section III), we verified that our results are similar (though less precisely estimated) when we use judicial complaints per capita as our measure of judicial inefficiency (see online Appendix Table A-14).[38]

D. *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.

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 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 high-powered incentives. However, there is a significant and large negative differential effect in 2002, which is concerning. In Table 9, we investigate the source of this pretrend, and show that it is caused by outliers. When we take out outliers in the same manner as in Table 6, the impact of high-powered incentives on the quality of local judicial institutions remains similar as in Table 8, but the pretrend in 2002 disappears. Overall, though the pretrends 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.

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[37] Specifically, both types of colonel-led brigades are associated with more false positives during the incentives period (with coefficients that are significant at least at the 95 percent level). While the coefficient for mobile brigades is larger, the implied sizes are similar because the scale and variation of these variables are different.

[38] We also explored whether areas with the combination of both bad institutions and more colonels have the strongest reaction in false positives. However, the triple interaction of the high-powered incentives period, colonels, and judicial inefficiency is imprecisely estimated and not statistically significant (see online Appendix Table A-17).

TABLE 8—JUDICIAL INEFFICIENCY AND COLONELS, 2000–2010

	Baseline		Post trends		Municipal trends	
	Dummy (1)	Linear (2)	Dummy (3)	Linear (4)	Dummy (5)	Linear (6)
Dependent variable is judicial inefficiency						
Colonel in charge (share)						
… × 2002	−0.025	−0.025	−0.029	−0.029	−0.032	−0.032
	(0.013)	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)
… × incentives (2003–2008)	0.015	0.003	0.010	0.002	0.017	0.004
	(0.007)	(0.001)	(0.008)	(0.002)	(0.008)	(0.001)
… × 2009			−0.013	−0.013		
			(0.010)	(0.010)		
Controls × time effects	✓	✓	✓	✓	✓	✓
Observations	9,823	9,823	9,823	9,823	9,823	9,823
Municipalities	893	893	893	893	893	9,823
R^2	0.030	0.030	0.030	0.030	0.140	0.140

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (odd columns) or a linear trend (even columns), both for the period from 2003 to 2008. Time dummies are interacted with the following set of time-invariant predetermined municipal controls: quartic polynomial for 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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 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.

Table 10 turns to the effect of high-powered incentives on attacks by the guerillas, paramilitaries, and the government. Since high-powered incentives were ostensibly directed at increasing the state's military control, we should find a decline in illegal armed groups attacks and possibly an increase in government attacks against these groups in places where the military has stronger incentives. But the pattern is quite different. For guerilla attacks, we find no effects from the interactions with the share of colonels or local judicial institutions. For paramilitary attacks, we also see no effects for the interactions with the period of high-powered incentives. These results suggest that high-powered incentives as a counterinsurgency strategy were ineffective and did not increase the state's ability to contain nonstate armed actors. Though this is a little speculative, the most likely explanation for this paradoxical result is that high-powered incentives increased extrajudicial killings and eroded trust in the military and government institutions, potentially worsening the security situation. Equally paradoxically, we also see a decrease in government attacks against nonstate actors in areas with higher-powered incentives, suggesting that these incentives may have even failed to induce the military to intensify their efforts to combat these groups.

In sum, these results paint a picture of high-powered incentives for the military being fairly ineffective as a counterinsurgency strategy. 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.

TABLE 9—JUDICIAL INEFFICIENCY AND COLONELS, 2000–2010: REMOVING OUTLIERS

	Baseline		Post trends		Municipal trends	
	Dummy (1)	Linear (2)	Dummy (3)	Linear (4)	Dummy (5)	Linear (6)
Dependent variable is judicial inefficiency						
Colonel in charge (share)						
… × 2002	0.010	0.012	0.005	0.005	−0.003	−0.003
	(0.010)	(0.011)	(0.011)	(0.011)	(0.013)	(0.013)
… × incentives (2003–2008)	0.024	0.005	0.018	0.003	0.016	0.003
	(0.008)	(0.001)	(0.008)	(0.002)	(0.007)	(0.001)
… × 2009:II			−0.012	−0.013		
			(0.011)	(0.011)		
Controls × time effects	✓	✓	✓	✓	✓	✓
Observations	9,332	9,332	9,332	9,332	9,332	9,332
Municipalities	893	893	893	893	893	893
R^2	0.044	0.044	0.044	0.044	0.162	0.163

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (odd columns) or a linear trend (even columns), both for the period from 2003 to 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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 2.5th and above the 97.5th percentile in the distribution of estimation residuals from our baseline regressions.

V. Concluding Discussion

Creating a secure environment for civilians and defeating various nonstate armed groups and insurgencies preying on them are some of the most pressing problems facing poor and even some middle-income countries around the world. Long-running conflicts have sometimes motivated governments to turn to high-powered incentives for their military and security services to intensify the fight against the insurgents, even though judicial and other institutions are typically quite weak and incapable of constraining misbehavior by the state's agents. President Uribe's policy of strengthening the military and its incentives to combat the guerilla after he came to office in 2002, is emblematic of this type of effort, especially in the way it has taken place in the context of very weak institutions.

We have shown that these efforts appear to have created very significant unintended consequences while also weakening the judicial dimension of state capacity. After presenting a simple multitasking 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 nonstate armed actors, particularly guerilla combatants, 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

TABLE 10—GUERILLA, PARAMILITARY, AND GOVERNMENT ATTACKS AND COLONELS, 2000-2010

	Guerilla		Paramilitary		Government	
	Dummy (1)	Linear (2)	Dummy (3)	Linear (4)	Dummy (5)	Linear (6)
Dependent variable is dummy variable for attacks by group						
Judicial inefficiency						
… × 2002	0.160	0.157	−0.110	−0.082	−0.005	−0.024
	(0.178)	(0.173)	(0.092)	(0.088)	(0.095)	(0.091)
… × incentives (2003–2008)	0.001	−0.001	−0.064	−0.005	0.041	0.003
	(0.093)	(0.019)	(0.065)	(0.013)	(0.062)	(0.012)
Colonel in charge (share)						
… × 2002	0.046	0.052	−0.015	−0.014	−0.050	−0.050
	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)	(0.050)
… × incentives (2003–2008)	0.013	0.005	−0.000	−0.000	−0.039	−0.008
	(0.024)	(0.005)	(0.017)	(0.003)	(0.016)	(0.003)
Controls × time effects	✓	✓	✓	✓	✓	✓
Observations	9,823	9,823	9,823	9,823	9,823	9,823
Municipalities	893	893	893	893	893	893
R^2	0.122	0.122	0.120	0.120	0.105	0.105

Notes: Panel estimation from 2000 to 2010 with municipality and time (year) fixed effects. In "… × incentives (2003–2008)," the variable shown is interacted with a dummy that equals one (odd columns) or a linear trend (even columns), both for the period from 2003 to 2008. Time dummies are interacted with the following set of time-invariant predetermined municipal controls: quartic polynomial for 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, 1999 coca cultivated area per 100 hectares, and 1995–1999 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.

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 counterproductively, in these same places both guerilla and paramilitary attacks do not appear to decline and, if anything, may have increased somewhat.

Though the situation in Colombia is unique, as we pointed out in the introduction, there are many other examples of the use of high-powered incentives as a counter-insurgency strategy. The available evidence suggests that in these cases too, there were widespread abuses and violence against civilians. In Guatemala, Peru, and South Africa, for example, post-conflict truth and reconciliation commissions have documented widespread killings of civilians.

In Peru, the commission documented 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 counterinsurgency 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 the incentives and impunity given to the military: "by privileging a military approach, one of the main objectives of the

counterinsurgency strategy was the elimination of members, sympathizers or collaborators of the armed insurrection, even more than the objective of capturing them to be judged by the competent judicial authorities" (p. 146). The report emphasizes the importance of the lack of judicial control in these outcomes as well (p. 176).

The Guatemalan commission 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).

The South African commission also reaches similar conclusions on the pervasiveness of human rights abuses (Truth and Reconciliation Commission of South Africa 1998, p. 62).

Our results are relevant beyond the issue of the use of high-powered incentives, for the broader question of state-building in conflict-riven societies. The use of high-powered incentives, though extreme in many ways, is consistent with the dominant paradigm on the state in the political science and political economy literatures, which views the establishment, by any means, of the state's monopoly of violence over its territory as the first and unrivaled prerequisite for building a state and its capabilities; other aspects of state capacity, including bureaucratic, fiscal and administrative capacity and rule of law, can be developed thereafter, once this monopoly of violence is secure.[39] This view reaches much farther than academic circles, and has become the guiding principle for US interventions in Afghanistan and Iraq in recent years (e.g., as articulated in Fukuyama 2004, and further emphasized in Giustozzi 2011). 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." Though many practitioners recognize that several aspects of state capacity need to be built ultimately, they typically end up endorsing the security first view (e.g., OECD 2010; Grävingholt, Leininger, and von Haldenwang 2012).

Our results can then be interpreted as highlighting how efforts to build the state's monopoly of violence by focusing primarily on military dominance can backfire with tragic consequences. Not only did the introduction of high-powered incentives for the military in Colombia bring about significant loss of innocent human life, but our findings suggest that this overall approach was counterproductive even in terms of the explicit goals it was trying to achieve. This reading of the evidence, together

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[39] The clearest articulations of this view are in the context of the "state first" or "security first" theories of state-building, often associated with Huntington's (1968) seminal work. This approach maintains that the Weberian monopoly of violence needs to be imposed on society before other aspects of state capacity can be developed, and thus views state-building as a top-down process (generally proceeding without the consent or participation of society). It is historically illustrated by 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 2014). Several important critiques of this interpretation of the historical process of state-building should be noted in this context, however (e.g., Rudolph and Rudolph 1979, Berman 2016).

with the recent literature on different counterinsurgency strategies discussed in the introduction, suggests that the implications of focusing on military victory at the expense of all else, for example, by using high-powered incentives, in the absence of accountability, can be highly perverse, and that the goal of attaining a legitimate monopoly of violence may be better served by attempting to build state institutions in multiple dimensions simultaneously.

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