Guns *and* Butter? Fighting Violence with the Promise of Development

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Abstract

There is a growing awareness in developing countries that government anti-poverty programs may play an important role in the fight against internal security threats. In addition to improving economic conditions, such programs may demonstrate the government's commitment to the fight against poverty and may therefore make civilians more willing to share information on insurgents with the police. In this paper, we test the importance of this citizen-support channel by analyzing the impact of a large public-works program, the National Rural Employment Guarantee Scheme (NREGS), on insurgency-related violence in India. We first show that this channel implies a dynamic pattern of violence that is different from the leading alternative theories in the literature. We then test these predictions empirically by using a regression-discontinuity design. Our results suggest that insurgency-related violence increases in the short run but then trends downwards, which is consistent with the citizen-support channel playing an important role.

JEL: H12, H53, H56, I38

Keywords: public works program, National Rural Employment Guarantee Scheme, NREGA, NREGS, India, regression discontinuity design, terrorism, Naxalites, Maoists, conflict, insurgency, civil war

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1 Introduction

Civilians play a crucial role in many conflict situations from gang wars to clashes between government troops and insurgents. While they typically do not directly participate in violent encounters between two or more groups, they often have extensive information about the location, movements and goals of actors on both sides and can choose whether, and with whom, to share this knowledge.[1]

This position particularly affects civilians in developing countries where internal conflicts are relatively common.[2] Governments have traditionally relied very heavily on military force, but there is a growing awareness that this alone may not be enough to end these conflicts, since insurgents often rely on the loyalty of the local population in their guerrilla tactics and recruit members from economically marginalized groups of society. In such a situation, government anti-poverty programs that target conflict areas may improve the relationship between civilians and the government and increase the willingness of the local population to share information on the insurgents with the police.

In this paper, we study the importance of this citizen support channel by analyzing the impact of one of the world's largest development programs, the Indian National Rural Employment Guarantee Scheme (NREGS), on the incidence of internal violence in the country. NREGS is based on a legal guarantee of 100 days of employment to all rural households (about 70 percent of the population) willing to work at the minimum wage, and annual expenditures on the scheme amount to around one percent of Indian GDP. While the program's main goals are to generate labor market opportunities and to improve local infrastructure, one of the expectations of the government was to reduce incidents of violence by Naxalites, a group of Maoist insurgents that are a major internal security threat in India.

To clarify the expected empirical impacts, we first set up a simple theoretical model that

[1] Additionally, the people are also not rarely forced to pick sides in a conflict, but may switch sides if either economic opportunities or their perception of the eventual winner changes (for more on the context in question see Mukherji 2012).

[2] Over 20% of countries experienced internal military conflicts over the course of the 1990s, for example (Blattman and Miguel 2010). Cross-country data also shows a high correlation between poverty and conflict (see e.g. Collier and Hoeffler 2007). Miguel et al. (2004) and Miguel and Satyanath (2011) find that economic growth, instrumented by rainfall shocks, has a negative impact on conflicts.

extends the model by Berman, Shapiro and Felter (2011). In the model, government troops and insurgents fight over territory. A program like NREGS improves the relationship between the government and the people and makes civilians more willing to share information with the police, which in turn increases the police's efficiency in tracking down rebels. In contrast to the existing literature, the model predicts a nuanced but intuitive time path where incidents of violence should increase in the short run because of larger military action by the police and potential retaliation by the insurgents, and a decline in violence only in the longer run when the presence of insurgents has become weaker. In addition to these theoretical predictions, we also discuss the implied empirical patterns of a number of alternative theories.

We then test these predictions empirically. NREGS was rolled out non-randomly in three implementation phases, with poor districts being treated earlier. The government used a two-step algorithm to assign districts to phases, which generates state-specific treatment discontinuities and allows us to use a regression discontinuity design to analyze the empirical impact of the program. The results show that the number of fatalities in the short-run increases significantly, by about 68 percent from baseline, while the number of incidents increase by about 45 percent. Naxalites and civilians are the most affected groups, whereas there is little impact on police casualties. These results are robust across a number of different specifications. Violence increases most in the early months after implementation and trends downwards afterwards. We also find evidence of substantial spillovers of violence from treated districts to untreated neighbors.

Taken together, these empirical patterns are consistent with the predictions of our theoretical model, but are difficult to explain with most alternative theories.[3] Overall, the empirical results therefore suggest that anti-poverty programs may improve the relationship between the government and the local population, which in turn may help reduce the intensity of internal conflict in the longer run. This provides a non-negligible additional benefit of government programs which, at least initially, may occur even for programs that are prone to implementation problems.

[3]Qualitative as well as quantitative evidence from the literature on the Maoist conflict also supports the idea that the proposed mechanism is plausible in the Indian context.

Our paper contributes to a growing literature on the impact of government programs on internal violence. Most of the conflict literature looks at a snapshot of violence intensity, whereas our paper follows its dynamics. This distinction is important since a myopic view would focus only on the rise in violence, even though a more comprehensive picture highlights that violence should fall eventually. Furthermore, the role of citizens and the importance of information-sharing is not yet as acknowledged in the literature on conflict in developing countries as it is in some other areas.[4] Citizens have traditionally often been neglected in theoretical conflict models, for example.

The paper probably most closely related to ours is Berman, Shapiro and Felter (2011) who look at the impact of a development program in Iraq. They find that violence decreases after the program is introduced and argue that this is due to increased information-sharing by civilians. Given the intuitive idea that violence may well increase initially, this seems like a rather unique case. Additionally, the situation in Iraq with its heavy involvement of foreign powers and its emphasis on reconstruction programs is a relatively special situation that is not necessarily applicable to other developing countries facing internal security threats.

Some other recent papers also analyze the impact of government programs on the incidence of violence in developing countries, and either find positive or negative impacts that are typically consistent with more than one explanation (see e.g. Nunn and Qian 2012, Crost, Felter and Johnson 2012, Crost, Felter and Johnson 2013). This mixed pattern points to the nature of conflict playing a crucial role in how effective government programs are for a short- and long-term reduction in violence. The results of our paper also contribute to the broader literature on the relationship between conflict and development.[5]

The remainder of this paper is structured as follows: Section 2 provides some background on the Maoist movement and NREGS. Section 3 discusses potential hypotheses of the impact of NREGS on violence and sets up a simple theoretical model, whereas section 4 provides details on the empirical strategy and the data. Section 5 presents the main results as well as some extensions and robustness checks, and section 6 concludes.

[4] One example is the US crime literature, see e.g. Akerlof and Yellen (1994).

[5] For recent microeconomic studies, see e.g. Do and Iyer (2007), Murshed and Gates (2005), Barron, Kaiser and Pradhan (2004), Dube and Vargas (2007) and Humphreys and Weinstein (2008).

2 Background

2.1 The Naxalite Movement

According to the Government of India, the Naxalite movement is one of India's most severe threats to national security. In 2006, Prime Minister Manmohan Singh famously referred to it as "the single biggest internal security challenge ever faced by our country"[6]. Members of the movement are typically called Naxalites or Maoists, although official government documents often refer to affected districts as Left-Wing Extremism (LWE) districts.[7]

Naxalites have been operating since 1967 when landlords attacked a tribal villager in the small village Naxalbari in West Bengal and triggered an uprising. By the early 1970s, the movement had spread to Andhra Pradesh, Bihar and Orissa, but splintered into more than 40 groups. In 2004, the two biggest previously competing Naxalite groups joined hands to form the Communist Party of India (Maoist). This is believed to have substantially exacerbated India's problem with the Naxalites and to have driven the recent growth in violence in important parts of the country (Kujur 2009, Lalwani 2011). The Indian Home Ministry believed the movement to have around 15000 members in 2006, to control about one fifth of India's forests, and to be active in 160 districts (Ministry of Home Affairs 2006). As Figure 1 shows, Naxalite-affected districts are concentrated in the eastern parts of India. These areas are often referred to as the Red Corridor.

The Naxalites' main goal is to overthrow the Indian state and to create a liberated zone in central India, since they believe that the Indian government neglects the lower classes of society and exclusively caters to the elites. Traditionally, the primary targets were landlords and upper caste landowners, but attacks have recently turned into larger and better-planned strikes on government institutions and personnel (Singh undated). One common target of attacks are infrastructure projects such as railways, public buses and telecommunication tow-

[6] Hindustan Times, April 13, 2006: Naxalism biggest threat: PM

[7] There is some debate in the literature about the correct way of addressing the insurgents. Mukherji (2012) argues, for example, that the insurgents should be referred to as Maoists rather than Naxalites since the organizations that grew out of the original Naxalite movement of the 1960s mostly reject the actions of the Communist Party of India (Maoist) (CPI(M)) that is largely responsible for the violence in recent years. A number of Naxalite organizations even refer to the CPI(M) as terrorists.

ers (Ramana 2011). The strongholds of the Naxalites continue to be in tribal areas that are typically resource-rich but chronically underdeveloped. Substantial mining activities that lead to the displacement of the tribal population and the absence of economic development opportunities are believed to be an explanation for the continuing popularity of the movement among the local population (see e.g. Borooah 2008, Kujur 2009). Naxalites finance themselves mainly through levies on industries and forest contractors, both in return for protection, as well as in payment for engaging in illegal tree-felling or mining (Sundar 2011).

The Indian government has been fighting the Maoists since the 1960s, but decades of using force have been largely unsuccessful in suppressing the movement. While India officially subscribes to a population-centric approach to counterinsurgency, which relies on a mixture of force and winning over the local population through taking care of their grievances, a number of researchers note that India traditionally relies almost exclusively on military strength to fight the Naxalite movement (see e.g. Banerjee and Saha 2010, Lalwani 2011). The main responsibility in this fight rests with the civil and paramilitary forces of the state police in the affected areas, although they are often supported by central paramilitary battalions. While some states have been more successful at suppressing violence than others, the central government generally blames the overall failure to contain the Naxalites on inadequate training and equipment, as well as on poor coordination between police forces of different states. Many observers also refer to the often widespread disregard for local perceptions, however, as well as the sometimes excessively brutal nature of police force behavior that also affects many civilians. These destroy not only the trust of the local population in the Indian state but also the opportunity for police forces to take advantage of information on insurgents provided by the people (Bakshi 2009, Lalwani 2011, Sundar 2011).

It is believed by both Maoists and security forces that civilians have a lot of information on the insurgents, so pressures on the local tribal population (also called adivasis) to pick a side and cooperate with one of the conflict parties is high. The Naxalites' continued survival depends on help from civilians who hide them and provide them with resources and information. For example[8] Maoist insurgents often warn the local population not to provide

[8]There is some evidence that insurgents in turn provide civilians with some help, for example in the form

shelter or information to police forces, and instead ask them to keep track of government personnel and their actions. The government, on the other hand, often also does not seem to regard civilians as neutral. In fact, some experts claim that an important percentage of incarcerated adivasis are in jail due to false accusations of being Maoist supporters (Mukherji 2012).

In addition to these pressures, adivasis also face economic incentives to join the conflict: Their knowledge of local conditions in the often remote forest areas is very valuable for both insurgents and government troops. In areas of chronic underdevelopment with few employment opportunities, working for one of the conflict parties therefore allows the poor to earn some income (Mukherji 2012).[9]

In consequence, many adivasis are involved in the conflict as tacit supporters, informants and recruited fighters on both sides, and switching sides once conditions change is not uncommon.[10] Economic opportunities or changes in the perception of which side has the upper hand seem to affect behavior: In many states, Maoists who surrender to the police and provide information on their organization receive some land and money to start a new life, for example.[11] Vanden Eynde (2011) also shows that Naxalite violence against civilians increases after negative rainfall shocks, which is consistent with his theoretical model in which Maoists try to prevent the local population from being recruited as government informants during bad economic times. A number of instances where Maoists left leaflets after killing civilians in which they accused the victims of being police informers are also in line with the idea that Maoists retaliate against civilians believed to support the government.[12]

In light of this situation, the view that military force alone may not be effective in solving the Naxalite problem in the long run seems to have grown in recent years: In 2007, for

of teaching them more effective farming techniques (Mukherji 2012). Naxalites also claim to protect civilians from exploitation by large mining conglomerates (Borooah 2007).

[9]Many low-rank Maoists directly involved in encounters with security personnel as well as an important portion of the police force consist of young tribals, for example.

[10]See e.g. Mukherji (2012)

[11]See e.g. www.satp.org

[12]See e.g. www.satp.org for the following press releases from 2007: *"Cadres of the CPI-Maoist shot dead a 45-year-old shopkeeper at Sringeri in the Chikmagalur District, suspecting him to be a Police informer...Before fleeing, they left behind pamphlets with a message that read: 'Let us expose informers and teach them a befitting lesson.'"* *"Two brothers were killed at Tamba village by the CPI-Maoist cadres on suspicion of being Police informers... More than 20 Police informers have reportedly been killed in the last one year in Jharkhand."*

example, Prime Minister Manmohan Singh said 'Development and internal security are two sides of the same coin. Each is critically dependent on the other.'[13] He also noted that many Maoist recruits come from economically deprived and marginalized groups of society. The central government has therefore shown a growing interest in increasing economic development in underdeveloped areas of the country through anti-poverty programs, in the hope that an improvement in the local population's situation would lead to a reduction in Naxalite violence (Ramana 2011). The National Rural Employment Guarantee Scheme is by far the most ambitious and largest anti-poverty program introduced by the Indian government, and some case studies suggest that the program may have indeed helped to reduce violence in certain areas (see oneworld.net 2011 for a case study of Balaghat in Madhya Pradesh).

This change in conflict intensity also seems to hold more in general: Maoists have been losing ground in a number of Indian states: Maoists are now mostly non-existent in Andhra Pradesh and have lost influence in Bihar and even their stronghold states Jharkhand and Chhattisgarh. The Maoists seem to be forced to move out of many traditional areas of Maoist control and to retreat into the Dandakaranya forest area where their headquarters are assumed to be (Mukherji 2012).

Improved access to information seems to have played an important role in this development: The Indian Home Secretary Gopal K. Pillai said in 2010, for example, that the intelligence gathering system of the police has improved over the last couple of years, making police forces more successful at catching Maoists.[14] These developments are also recognized by the insurgents, who are accusing the government of turning the local population into police informers and of using surrendered Maoists as sources of information.[15]

[13]The Indian Express, December 20, 2007: Divide, uneven growth pose threat to our security: PM

[14]Summary of a lecture given by Gopal K. Pillai on March 10, 2010, which is available at: http://www.idsa.in/event/EPLS/Left-WingExtremisminIndia

[15]According to a press report from 2007, for example: *"The CPI-Maoist reportedly issued a press release at Chintapalli village in the Visakhapatnam District, blaming the Police for turning the Girijans (local tribals) into informers by spending huge amounts of money... (and) that surrendered Maoists are helping the Police, were not leading a normal life and were always with the Police who provided them with all luxuries and used them in combing operations..."*

2.2 NREGS

The National Rural Employment Guarantee Scheme (NREGS)[16] is often referred to as the largest government anti-poverty program in the world. The scheme provides an employment guarantee of 100 days of manual public-sector work per year at the minimum wage to all rural households. The legal right to this employment is laid down in the National Rural Employment Guarantee Act (NREGA) that was passed in the Indian Parliament in August 2005. Under the scheme, all households can apply for work at any time of the year as long as they live in rural areas and their members are prepared to do manual work at the minimum wage. Wages are to be paid within 15 days after the work was performed, otherwise the worker is eligible for an unemployment allowance.[17] While the minimum wage is state-specific, NREGA specifies a floor minimum wage which was Rs. 60 per day at the introduction of the program. It has been raised over time, and was Rs. 120 per day in 2009.

NREGS was rolled out non-randomly in accordance with a poverty ranking[18] across the country in three phases: 200 districts received the scheme in February 2006 (Phase 1), whereas 130 districts started implementation in April 2007 (Phase 2). Since April 2008, the scheme has operated in all rural districts in India (Ministry of Rural Development 2010).[19] Districts that should have received the program in the first or second phase according to the reconstructed government algorithm (which will be explained in more detail below) are dark or light grey in Figures 2 and 3, respectively.

Many of the poorest Indian districts are also those heavily affected by Naxalite violence, as can be seen when comparing the red corridor districts from Figure 1 to the least developed districts predicted to receive NREGS in the earliest phase in Figure 2. One potential concern with development programs in these areas is that the presence of local governments is relatively weak and Naxalites may hinder or prevent the working of these schemes in their fight

[16]The program was renamed to Mahatma Gandhi National Rural Employment Guarantee Scheme (MGNREGS) in 2009, but the abbreviations NREGS and NREGA (for the act on which it is based) have stuck in the academic literature on this topic.

[17]For more details on the scheme see e.g. Dey et al. (2006), Government of India (2009), and Ministry of Rural Development (2010).

[18]Details on the procedures used to determine district assignment to phases will be provided below.

[19]The scheme only excludes districts with a 100 percent urban population, and is active in 99 percent of Indian districts.

against the government. Naxalites are indeed known to have blocked a number of government development programs (Banerjee and Saha 2010). Interference from Naxalites does not seem to be a large-scale problem for the working of NREGS, however: In contrast to a number of other government schemes, chief secretaries from the seven states most heavily affected by Naxalite violence believe NREGS to work relatively well in their districts[20]. Case studies in Jharkhand, Chhattisgarh and Orissa also come to the conclusion that the Naxalites are not blocking NREGS projects, with the exception of road construction projects [21] (Banerjee and Saha 2010).

As one of the most ambitious government programs in the world, NREGS has generated a lot of interest in the popular press, among policymakers and in academic research. An increasing number of papers in economics analyze the working of the employment guarantee and its impact on various outcomes of interest. By now, a number of papers suggest that implementation issues may substantially limit the effectiveness of the program: Dutta et al. (2012), for example, document widespread rationing of NREGS employment since there is often excess demand, and show that this is especially common in poorer states. Niehaus and Sukhtankar (2012a and 2012b) analyze the existence and characteristics of corruption in the implementation of NREGS in Orissa, and find that an increase in the minimum wage was not passed through to workers. NREGS seems to work much better in some other states like Andhra Pradesh, however: Johnson (2009a) finds that NREGS seems to provide a safety net for rural households since take-up of the program increases after negative rainfall shocks. Johnson (2009b) finds that the working of NREGS does not seem to be substantially affected by the specific party in power at the local panchayat level, suggesting that political pressures on NREGS in the state are not pervasive.

A number of papers have also focused on analyzing the impact of the employment guarantee scheme on rural labor markets in India. Imbert and Papp (2013), Azam (2012) and Berg et al. (2012) use a difference-in-difference (DID) approach to look at the program's impact on wages and employment by exploiting the phase-in of the program over time. The general

[20] The Times of India, April 14, 2010: Naxals backing NREGA?

[21] Maoists claim some roads were built for military counterinsurgency

conclusion from their findings is that NREGS seems to have led to higher public employment and increased private-sector wages that are concentrated in the agricultural off-season (Imbert and Papp 2013), in areas with high implementation quality (Berg et al. 2012), and strong among casual workers (Azam 2012).

All of these papers tend to focus heavily on interesting sources of heterogeneity rather than on the general impacts of the program. In addition, they need to rely on some version of the parallel trend assumption in their analyses, which may well be violated in practice since the NREGS roll-out was non-random. Zimmermann (2013a) reconstructs the government algorithm used during the roll-out of the program and uses a regression-discontinuity framework to look at the overall labor-market impacts of the program. The results show no evidence for a high take-up of NREGS or any substantial wage increases, although the time frame of the analysis would be consistent with Berg et al.'s finding that wage effects may take some time to come into effect. The scheme may have some welfare benefits by providing a safety net after bad economic shocks, however, which decreases the riskiness of self-employment activities. Zimmermann (2013) also shows that these general patterns of the NREGS impacts also hold when using a DID approach.

The empirical literature on NREGS therefore suggests that while there may be important heterogeneous impacts, the program may not be very effective overall. In consequence, it should not substantially change the economic conditions of civilians, at least in the short run and in the early stages of implementation. Since we focus on a similar time interval in our empirical analysis, the impacts of the scheme on violence in this paper are therefore more likely driven by the promise of government support rather than any substantial labor market benefits or improvements in local infrastructure. This is especially true because a breakdown of project categories reveals that NREGS focuses on drought-proofing measures and does not generate a lot of infrastructure improvement or physical assets.[22] Since a stronger commitment to development projects and the welfare of the local population in the Naxalite-affected districts presents an important shift from the traditional reliance on force

[22] According to Ministry of Rural Development (2010), for example, the breakdown of projects for the financial year 2008-09, was as follows: 46% water conservation, 20% provision of irrigation facility to land owned by lower-caste individuals, 18% land development, 15% rural connectivity, 1% any other activity.

in the conflict, NREGS could still have important impacts on the relationship between the local population and the government, however. Empirical impacts are therefore likely to be driven by the more indirect effects of NREGS. In addition to any welfare benefits from the insurance function of NREGS there could potentially be a change in the people's perception of the government and in the expectation of which side will eventually win. This last effect may be particularly strong if the police become more effective at tracking down insurgents because of improved information.

3 Theoretical Frameworks and Hypotheses

To clarify the expected empirical effects of the impact of NREGS on Maoist-related violence according to different theories, we first set up a simple model that incorporates the importance of citizen support for the government due to information-sharing. We then discuss alternative explanations and their empirical predictions.

3.1 A Simple Citizen-Support Model

The model we set up closely follows the work of Berman, Shapiro and Felter (2011) on counterinsurgency in Iraq and the Akerlof and Yellen (1994) study on street-gangs. We extend these models, however, by allowing the insurgents to fight for territory, whereas the rebels' goal in the Berman, Shapiro and Felter model is only to impose costs on the government.

3.1.1 Strategies, Sequence and Set-up

The theoretical model consists of a sequential game with three players: The government (G) and the Naxalites (N) are fighting for territory, whereas the civilian community (C) can choose how much information about the Naxalites to provide to the government. Since violence by the Naxalites and military action by the police require preparation and time, it is assumed that the civilians are the last movers.

Nature first chooses norms n, drawn from a uniform distribution $U[n_l, n_u]$, that only civilians can observe. These norms determine how much the civilians want Naxalites to be in control of the territory. Then, the Government chooses whether to provide the civilians with NREGS, g, and how much military action to take against the Naxalites, m. The government can only effectively implement NREGS if it has control of the territory. Simultaneously, Naxalites determine how much violence, v, to attempt against the government, and whether to provide the civilians with services[23], s, or to retaliate against them for sharing information, r. Assume that $n_l \leq v + g - (r + s) \leq n_u$, so that neither G nor N can single-handedly decide the outcome of information provision by C. After these decisions have been made, civilians decide how much information, i, to share with G. At the end of the game, payoffs occur, and territorial control is determined.

If the government controls the territory, we represent it as $a = 1$, else $a = 0$. The probability of obtaining control is determined by the following factors: the extent of military action m by the government, the amount of militant action by the Naxalites, v, and the amount of information civilians share with the government, i: [24]

$$P(a = 1) = p(m, v, i)$$

Military action by the government increases their hold on the territory, whereas militant action by the Naxalites will lower it. Thus, $p_m > 0$ and $p_v < 0$.[25] Information sharing helps the government, $p_i > 0$. Furthermore, the more information the government has, the less the impact of Naxalite violence, $p_{vi} < 0$, since the government can better defend themselves against Maoists' attacks. Similarly, the government's military actions are more effective the more information they have $p_{mi} > 0$.

[23]The services could be in the form of fighting for the interests of the civilians.

[24]This technology of control of territory is different from the Berman, Shapiro and Felter (2011) model. They assume $P(a = 1) = h(m)i$ which does not depend on militancy by the rebels. In our context, however, it is more realistic that rebel violence can lower the probability of government control in the territory.

[25]We assume $p_{mm} < 0, p_{ii} < 0$ and $p_{vv} > 0$. For an interior solution we need to bound the amount of militant violence $v < v^{max}$. And $p(m, v^{max}, i) \geq 0$

3.1.2 Payoffs

The community's payoffs are

$$U_C = u(c + g - n - r)a + u(c - v + s)(1 - a)$$

If the government is in control ($a = 1$), then the community consumes c, and NREGS g, but the Naxalites may choose to retaliate against the community with r (or against the police v). If the Naxalites are in control, ($a = 0$), then the community benefits from their services s, but suffers from their violence v (either because of collateral damage or empathizing with the police who are the targets). NREGS is only implemented in the regions where the government has control.[26] Because of uncertainty in who has control, the community's expected utility function is

$$EU_C = u(c + g - n - r)p + u(c - v + s)(1 - p) \tag{1}$$

The Naxalites wish to control the territory. They maximize their expected payoff[27]

$$EU_N = E[(1 - a)] - B(v) - S(s) = (1 - p(m, v, i)) - B(v) - S(s) \tag{2}$$

Where the costs of militancy $B(v)$ and of providing services $S(s)$ are increasing and convex.

The government fights for the same territory and bears increasing and convex costs of military action $D(m)$ and NREGS provision $H(g)$. [28]

$$EU_G = E[a] - D(m) - H(g) = p(m, v, i) - D(m) - H(g) \tag{3}$$

[26] Alternatively the government can use NREGS as a bargaining chip and only provide it to regions that help them win control.

[27] The payoffs for rebels and the government are different from Berman, Shapiro and Felter (2011). In their context (Iraq), the rebels maximize the cost of harm done by violence, whereas the government tries to minimize this cost.

[28] It is important to notice that the government here is not trying to maximize social welfare: Even if civilians wanted the Naxalites to be in power, the government would still fight for the territory rather than hand over the land to the rebels.

The concavity of the utility functions will ensure an existence of a Nash equilibrium of the game. The equilibrium solution of the model can be found in Appendix A. When NREGS is implemented, there is a greater information flow from civilians to the police, which police use to attack the Naxalites. The insurgents retaliate against the civilian informers and against the police.

This model generates testable predictions on the incidence of conflict: it implies that insurgency-related violence should increase in the short run after NREGS is introduced. This is because violence increases with government military action m, rebel militancy v, and retaliation r, all of which increase with g. Furthermore, g will increase the flow of information to the government, making their fight against the rebels more effective. Therefore we should expect to see more Naxalites dying or being captured when NREGS is implemented.

The long-run impacts may be very different, however. If the government is more effective at catching Naxalites, this will speed up the end of the conflict, and in the long run violence will fall because the government is winning the war. These considerations suggest that the dynamic development of violence intensity will be important. Empirically, we should therefore expect violence to increase initially, but to trend down afterwards.

3.2 Alternative Theories

The citizen-support channel emphasized in the model provides only one theory of the impacts the introduction of a large anti-poverty program like NREGS should have on insurgency-related violence. A number of alternative theories from the broader literature on the relationship between development and conflict are relevant in this context, however, and often generate very different empirical predictions.

While our model implies that violence should rise initially, two established theories in the literature predict a fall in the incidence of conflict. The first theory considers the problem of credible commitment: In a situation where the government has been unable to credibly commit to economic development and in which insurgents fight for better economic conditions, a program like NREGS may solve this problem and 'complete the contract' (Powell 2006)

between rebels and the government. The costs of dismantling NREGS may be high because of constitutional obligations and the political popularity the scheme may garner, and thereby force the government to keep implementing the program. [29] In this case, the introduction of NREGS gives insurgents fewer reasons to continue their struggle, and violence should fall. This assumes, of course, that NREGS is actually effective as an anti-poverty program. Since there is evidence of severe implementation problems and relatively small direct income benefits from the scheme, this theory is less compelling a priori.[30]

The second theory that predicts a fall in violence after the introduction of NREGS is an opportunity-cost story: If the program provides jobs and other welfare benefits from participation in the scheme, then the program will increase the opportunity cost of being a Maoist (Grossman 1991). Naxalite supporters should therefore drop out of the organization to take advantage of the improved economic opportunities, and rebels should find it harder to recruit new soldiers, both of which decrease the strength of the insurgents and their ability to inflict violence (see e.g. Grossman 1991 for such a model). [31] In consequence, insurgency-related violence should fall. Again, for this explanation to apply NREGS needs to actually generate economic benefits.

Both of these explanations imply a dynamic pattern where violence may stay constant or fall in the short run and will then trend down over time as the economic benefits of the anti-poverty program are realized more fully. One of the most widespread theories in the literature suggests, however, that we should expect violence to increase after the introduction of NREGS. This theory focuses on the idea of competition for resources (see e.g. Hirshleifer 1989, Grossman 1991, Skaperdas 1992): If NREGS increases the wealth of a region, then this creates a larger resource pie that is worth fighting over. Contest models that focus on this

[29] Zimmermann (2013b) finds that the duration of NREGS receipt may have had an important influence on national election outcomes, for example.

[30] Additionally, there may be asymmetric information about the dismantling costs of NREGS, and the rebels may expect the government to renege on the promise (Dal Bo and Powell 2007). The Indian government has a long history of development programs that were often temporary and largely ineffective, which may affect present expectations on the longevity of this scheme and could further weaken any conflict-reducing impacts.

[31] This idea is also closely related to literature on economic inequality and group formation in the conflict literature. Grossman (1999) argues, for example, that incentives such as wages, opportunities to loot and protection from danger are often used to motivate participation. In this view, economic inequality may lead to conflict because there is more to gain from victory (Fearon 2007).

channel usually predict that when resources rise in a region in equilibrium more effort will be put into fighting rather than production. Again, this presupposes that NREGS generates resources that can be appropriated through violence. The dynamic implications of this theory are that we should expect violence to either remain constant or to increase in the short run, and to then increase over time as the economic benefits are realized and appropriable assets are created.

Overall, the theories considered in this paper therefore fall into three categories according to the dynamic pattern they predict: If violence remains constant or decreases in the short run and then continues decreasing over time, this provides evidence of an opportunity cost story or a commitment problem explanation being important. If violence remains constant or increases in the short run and then trends up afterwards, this points to a resource competition story. And if violence increases in the short run and then starts decreasing over time, this supports the idea that the citizen-support channel drives the results. We will test these predictions empirically.

4 Identification Strategy, Data and Empirical Specification

4.1 NREGS Roll-out and the Assignment Algorithm

The challenge in analyzing the impact of NREGS empirically is that the program was rolled out non-randomly to less economically developed districts first. The Indian government used an algorithm, however, to determine which districts would start implementing the program in which phase. Zimmermann (2013a) reconstructs the likely algorithm from available information on the NREGS rollout and institutional knowledge about the implementation of development programs in India. The algorithm has two stages: First, the number of treatment districts that are allocated to a given state in a given phase is determined. It is proportional to the prevalence of poverty across states, which ensures inter-state fairness

in program assignment.[32] Second, the specific treatment districts within a state are chosen based on a development ranking, with poor districts being chosen first.

We use this procedure in our empirical analysis. The 'prevalence of poverty' measure used in the first step of the reconstructed algorithm is the state headcount ratio times the rural state population, which provides an estimate of the number of below-the-poverty-line people living in a given state and shows how poverty levels compare across states. In the first step of the algorithm, a state is therefore assigned the percentage of treatment districts that is equal to the percentage of India's poor in that state. For the calculations, we use headcount ratios calculated from 1993-1994 National Sample Survey data.[33]

The development index used to rank districts within states comes from a Planning Commission report from 2003 that created an index of 'backwardness', which is a term often used in India to refer to economic underdevelopment. The index was created from three outcomes for the 17 major states for which data was available: agricultural wages, agricultural productivity, and the proportion of low-caste individuals (Scheduled Castes and Scheduled Tribes) living in the district (Planning Commission 2003).[34] Data on these outcomes was unavailable for the remaining Indian states, and it is unclear whether a comparable algorithm using different outcome variables was used for them. We therefore restrict our empirical analysis to these 17 states. Districts were ranked on their index values.

Because of the two-step procedure of the algorithm, the resulting cutoffs for treatment assignment in a given phase are state-specific. Since implementation proceeded in three phases, two cutoffs can be empirically identified: the cutoff between Phase 1 and Phase 2, and the cutoff between Phase 2 and Phase 3. These cutoffs correspond to Phase 1 and Phase 2 NREGS rollout, respectively. We exploit both cutoffs in a regression discontinuity

[32] In practice this provision also ensures that all states (union territories are usually excluded from such programs) receive at least one treatment district.

[33] We use the rural state headcount ratios from Planning Commission (2009), since the original headcount ratio calculations do not have estimates for new states that had been created in the meantime. Since these are official Planning Commission estimates, they seem like the best guess of the information the Indian government would have had access to at the time of NREGS implementation. NSS data is nationally representative household survey data. The newest available information on headcount ratios at the time would have been the 1999-2000 NSS data, but these were subject to data controversies and therefore not used.

[34] The purpose of the index was to identify especially underdeveloped districts for wage and self-employment programs and, as mentioned above, it was used in pre-NREGS district initiatives, although those programs were much less extensive than NREGS and usually envisioned as temporary programs.

framework.

Since treatment cutoffs differ by state, ranks are made phase- and state-specific for the empirical analysis and are normalized so that a district with a normalized state-specific rank of zero is the last district in a state to be eligible for receiving the program in a given phase. This allows the easy pooling of data across states, since the treatment effect can then be measured at a common discontinuity in each phase. Negative numbers are assigned to districts with lower ranks than the cutoff rank, whereas positive numbers are assigned to the districts that are too developed to be eligible according to the district ranking and will function as control districts in our empirical analysis.

Table I reports how well the algorithm predicts NREGS receipt in Phase 1 and Phase 2 for the 17 major states.[35] The first column reports the number of non-missing rank districts per state. Columns 2 and 3 show the actual number of NREGS districts of each state in Phase 1 and Phase 2, respectively, whereas columns 4 and 5 provide the prediction success rate of the algorithm for Phases 1 and 2. The prediction success rate is calculated as the percent of districts for which predicted and actual treatment status coincide. Figures 2 and 3 also show the prediction success of the algorithm graphically for Phase 1 and Phase 2, respectively, with light grey districts being correctly predicted NREGS districts in a given phase, whereas dark grey districts are those that should have received NREGS but did not actually get the program in that phase.

As Table I shows, the overall prediction success rate of the assignment algorithm is about 84 percent in Phase 1 and about 82 percent in Phase 2.[36]. This means that there is some slippage in treatment assignment in both phases, and there is considerable heterogeneity in the performance of the algorithm across states. Nevertheless, the algorithm performs quite well in almost all of the 17 states. Overall, Table Table I therefore suggests that the proposed

[35]Rank data in the 17 major Indian states is complete for all districts classified as rural by the Planning Commission in their report, so there is no endogeneity in the availability of data in these states. Urban districts in the Planning Commission report are districts that either include the state capital or that have an urban agglomeration of more than one million people. Rank data is available for 447 of 618 districts in India. Data for the index creation was unavailable in some states, in most cases because of internal stability and security issues during the early 1990s when most of the data was collected. We exclude these states from the analysis.

[36]Prediction success rates for Phase 2 are calculated after dropping Phase 1 districts from the analysis.

algorithm works quite well for predicting Phase 1 and Phase 2 district allocations.

To achieve internal validity, the RD framework crucially relies on the idea that beneficiaries were unable to perfectly manipulate their treatment status, so that observations close to the treatment cutoff value are plausibly similar on unobservables and differ only with respect to their treatment status (Lee 2008). In the case of the two-step RD, this means that districts should not have been able to manipulate their predicted status under the algorithm in either step.

This seems plausible: As mentioned above, the headcount poverty ratio used to calculate the number of treatment districts for a state in the first step of the algorithm used data from the mid-1990s, which had long been available by the time the NREGS assignment was made.[37]

Like the information used for the first step, it also seems unlikely that it was possible to tamper with the data used in the second step of the algorithm: The 'backwardness' index was constructed from outcome variables collected in the early to mid-1990s, eliminating the opportunity for districts to strategically misreport information. Additionally, the suggestion of the original Planning Commission report had been to target the 150 least developed districts, but NREGS cutoffs were higher than this even in Phase 1 (200 districts received it in Phase 1). Therefore, districts would have had an incentive to be among the 150 poorest districts but not to be among the 200 or 330 least developed districts for Phase 1 and Phase 2, respectively. Lastly, the Planning Commission report lists the raw data as well as the exact method by which the development index was created, again eliminating room for districts to manipulate their rank. Overall, it therefore seems like manipulation of the rank variable is not a major concern.[38]

[37]The algorithm also uses state rural population numbers from the 2001 Census to transform headcount ratios into absolute numbers, but those figures were also long publicly available at the time. The RD may be potentially fuzzier than it really is because of some potential for measurement error introduced into the algorithm at this step since the exact numbers the government used in this step are not known, but this should not introduce systematic bias into the empirical analysis.

[38]This does not mean that actual treatment assignment was not subject to political pressures since Table I shows that compliance with the algorithm is often below 100 percent. Zimmermann (2013b) shows that deviations from the algorithm are correlated with party affiliation. This is also consistent with Gupta (2006) who analyzes the relationship between rule deviations and party affiliation for an earlier program. This paper ignores the fact that the program most likely also used the two-step algorithm, however, could substantially

Figures 4 and 5 look more closely at the distribution of index values over state-specific ranks. Ideally, the assignment variable should be continuous at the cutoff, since discontinuities at the cutoffs are typically taken as signs of potential manipulation. The figures therefore plot the relationship between the Planning Commission's index and the normalized state-specific ranks for the Phase 1 and Phase 2 cutoffs, respectively. For most states, the poverty index values seem pretty smooth at the cutoff of 0, again suggesting that manipulation of the underlying poverty index variable is not a big concern.

Another way of analyzing whether manipulation is likely to be a problem is to test whether there are any discontinuities at the cutoffs in the baseline data. If districts close to the cutoff are really similar to each other, so that outcome differences are just due to the different treatment status, we should not find significant impacts in the baseline data. Table II presents the results of such an analysis for the outcome variables used in this paper (the number of violence-affected individuals, of fatalities, injuries, major incidents and total incidents) for the time period before NREGS was rolled out to any phase.[39] It documents that out of the 30 estimated coefficients, only one is statistically significant. Overall, Table II therefore suggests again that manipulation is unlikely to be an important problem. Furthermore, the significant estimate of the total number of incidents is negative, whereas we find a positive treatment impact of NREGS, implying that, if anything, this effect is biased downwards.[40]

Finally, we need to verify that there really is a discontinuity in the probability of receiving NREGS at the state-specific cutoff values. Figures 6 and 7 show the probability of receiving NREGS in a given phase for each bin, as well as fitted quadratic regression curves and corresponding 95 percent confidence intervals on either side of the cutoff. The graphs demonstrate that the average probability of receiving NREGS jumps down at the discontinuity, although this discontinuity is much stronger in Phase 2 than in Phase 1. This suggests that there is indeed a discontinuity in the probability of being treated with the employment guarantee

affect the results.

[39] Since Phase 2 received the program later than Phase 1, the pre-treatment phase for Phase 2 in theory lasts an additional year. As we later show explicitly, however, it looks like Phase 2 was affected by spillover effects from Phase 1 districts before it actually received NREGS.

[40] Our main results also include the baseline outcome variable as a regressor, which controls for any baseline differences and should soak up some of the residual variance.

scheme at the cutoff. Since the treatment discontinuity for Phase 1 is relatively small, we later use a doughnut-hole approach as a robustness check, which is discussed in more detail in the empirical specification section.

4.2 Data and Variable Creation

The primary source of data used in this paper comes from the South Asian Terrorism Portal (SATP). This is a website managed by a registered non-profit, non-governmental organization called the Institute of Conflict Management in New Delhi. The Institute provides consultancy services to governments, and does extensive research on insurgency-related activities. The SATP aggregates news reports on Naxalite-related incidents and summarizes them. The summary usually contains (a) the location of the incident (district), (b) the date of the incident, (c) number of casualties (Naxalites, civilians, or police), and (d) the number of injuries, abductions or surrenders. For example the following summary: *"Andhra Pradesh, 2007: February 7 Police kill two CPI-Maoist cadres near Karampudi village in the Guntur District."* is coded as two Naxalite deaths in Guntur district in February. The source also codes the incident as 'minor' or 'major.'

Using this information we can construct variables at the district-month level. 'No incidents' are coded as 0. If some information is unclear, we verified the information by searching for the source news reports. We use data between January 2005 (the earliest time for which data is available on the website) and March 2008 since the remaining rural districts started receiving NREGS in April 2008. Thus, we have enough data both before and after implementation of the program to be able to analyze changes with the introduction of NREGS, with about two years of post-treatment data for Phase 1 districts and a year's worth of after-NREGS data for Phase 2 districts. This dataset is then merged with information on the poverty rank from the 2003 Planning Commission Report.

As is common with this kind of dataset, there are certain limitations to using it: The number of Naxalites killed or injured is difficult to verify, and security forces may have an incentive to overstate their accomplishments by inflating the numbers. This concern is

mitigated to some degree, however, by the fact that police are required to disclose names and ranks of the Maoists killed to validate their reports. Additionally, some minor incidents in remote areas may not reach the newspapers.[41]

These limitations introduce measurement error into the analysis, but should not systematically bias our regression discontinuity (RD) results. For a systematic bias, there would have to have been a systematic change in the reporting of Naxalite incidents when NREGS was (or rather should have been) introduced in a certain district. We have no reason to believe that any such systematic change in reporting occurred. The SATP data is therefore our most credible source and better than any administrative sources which may strategically misreport information.

Table III shows some summary statistics for our primary variables of interest. Our dataset records 1458 incidents, covering a total of 2030 fatalities. 267 of these incidents were coded by the SATP source as 'major'. Furthermore, in this 39-month period, 2545 people were either injured, abducted or surrendered to the police. On average, in any given red-corridor district, there are about 0.44 deaths a month related to Naxalite activities and about 0.317 incidents a month.

We also collect data regarding the police force from the Home Ministry of India. This data is at the state level and contains information on number of police officers, police posts and stations, as well as some other measures of police strength.

4.3 Empirical Specification

Since NREGS was rolled out based on an algorithm that assigned state-specific ranks to districts, this algorithm can be used as a running variable in an RD framework. In an ideal case, we would restrict the data to observations in the close neighborhood of the cutoff and estimate the treatment effect at the cutoff using local linear regressions. As the number of observations near the cutoff is limited in our case, however, we are also using observations

[41] Sundar (2011) points out two further potential issues: The data source misses the deaths of the members of certain vigilante groups like the Salwa Judum, and Naxalites are often blamed for civilians killed by security forces to protect the integrity of the police.

further away. Such an increase of the bandwidth will increase the precision of our estimates because of a higher number of observations, but potentially introduces bias since observations far away from the cutoff can influence the treatment effect at the cutoff (Lee and Lemieux 2010).

We address this concern in three ways: First, all results tables show the estimated coefficients for linear and quadratic regression curves in the running variable with and without constraining the slope of the curves to be the same on either side of the cutoff. F-tests reject the null hypothesis that higher-order polynomials add important flexibility to the model, and the quadratic flexible specification is always outperformed statistically by the linear flexible specification, so we do not include the flexible quadratic specification estimates.[42] Second, while our results use all districts of the treatment and control phase in a given specification, we test the robustness of our main estimates by restricting the sample to observations closer to the cutoff. Third, Appendix Figures C.13 to C.17 show the non-parametric relationships between the main outcome variables of interest and also plot quadratic polynomial regression curves. Similar to the summary statistics, they show that insurgency-related violence intensity is low in many districts and that a count-data model is a potentially preferable way of fitting the data. We therefore test the robustness of our results to the use of a zero-inflated Poisson model.[43]

Since the algorithm only generates a fuzzy RD, we use a two-stage least squares specification where actual NREGS receipt in a given phase is instrumented with predicted NREGS

[42] More flexible models also tend to be unstable in the second stage of the two-stage least squares estimation procedure, although the estimated coefficients are often qualitatively similar to the quadratic results.

[43] Our results are also robust to two additional concerns: The first one is the choice of the running variable. Our analysis uses the poverty rank as the running variable, whereas an alternative would be to use the underlying poverty index values. We use the rank since it is the variable that treatment is based on: The first step of the algorithm specifies the size of the treatment group, and the poorest districts are then chosen to fill this quota. This implies that what determines a district's distance from the cutoff is its rank rather than its poverty index value, since in many situations a district could be assigned a very different index value without altering its rank and therefore likelihood of being treated. Additionally, the conditional mean function of the outcomes of interest is flatter when using the rank rather than the index, which means that a large bandwidth is less problematic when using the rank variable. Our results are robust to using the index value as a running variable instead, however, even though they tend to be less precise. The second potential issue is that the rank variable is not truly continuous since, by construction, the distance between any two observations is identical. This characteristic is not unlike the measurement error for many discrete variables. Dong (2013) suggests a simple method of adjusting the RD estimates to this problem, and our results are robust to using this method.

treatment according to our algorithm, although intent-to-treat effects of the main results are reported in the appendix. To increase the precision of our estimates, we control for the baseline outcome variable. To ensure that the RD results are not affected by observations far away from the cutoff, we run results separately by cutoff, and drop the observations that should not affect the treatment effect at a given cutoff: For Phase 1 district assignment, we only include Phase 1 and Phase 2 districts in the analysis, and drop Phase 3 districts. Similarly, for Phase 2 cutoff regressions, we drop Phase 1 observations and only consider the remaining districts.

The equation below shows the regression equation for one of the specifications we run, which is linear in the running variable but does not constrain the coefficients to be the same on either side of the cutoff:

$$y_{ij} = \beta_0 + \beta_1 rank_i + \beta_2 nregs_i + \beta_3 nregs * rank_i + \beta_4 baseline y_{ij} + \eta_j + \epsilon_{ij}$$

where the subscripts refer to district i in month j since one observation is a district-month. y is an outcome variable of interest, *rank* is a district's rank based on the state-specific normalized index, and η are time fixed effects. The coefficient of interest is β_2, and *nregs* is actual NREGS receipt in a given phase, which is instrumented with predicted NREGS receipt. Standard errors are clustered at the district level.

As the theoretical model points out, the introduction of NREGS may influence the efficiency of the police in capturing Naxalites because of a better information flow between the local population and the police, since public goods provision and military action are strategic complements. While we cannot explicitly test this channel with our available data, one way in which police efficency might increase without working through the information channel is through an increase in the size of the police force. Controlling for the strength of the police force therefore keeps this channel constant, and more directly measures whether the same number of policemen now lead to different outcomes than before. Since we do not have data on the actual police force in a district, we estimate it using state-level data from the Indian Home Ministry. Any change in the police force for a given state is assumed to be attributable to NREGS districts only. In reality, these state-level estimates most likely

overemphasize the change in the police force and may therefore provide us with conservative estimates of the impact of NREGS. In our main results, we therefore add police force controls and later compare these estimates to specifications without such controls.

5 Results

5.1 Main Results

Our model predicts that after the introduction of NREGS, we should expect a rise in the incidence of Naxalite-related violence and then a downward trend over time, whereas alternative theories imply very different time patterns. Tables IV and V as well as Figures 8 to 12 present our main results that test these predictions.

Table IV shows the main results of the impact of NREGS on Naxal incidents for the five main outcome variables: individuals affected (deaths/injuries/abductions); deaths; injuries, abductions or surrenders; major incidents; and total incidents related to Naxal violence. Panel A shows the impact on Phase 1 districts only, whereas Panel B looks at the effect on Phase 2 districts, both for the time periods for which the next implementation phase had not yet received NREGS. Each panel has three different specifications: linear, linear with a flexible slope, and quadratic. As mentioned above, specifications in this table control for (estimated) police force changes.[44]

Panel A of Table IV shows that violence as measured by all outcome variables increases in Phase 1 districts after NREGS is introduced. Depending on the specification, there is a rise of about 0.3 to 0.45 deaths per month in a given district, for example. At a mean of about 0.44 deaths per month in a Red Corridor district, this amounts to about at least a 68% increase from the baseline level. Similarly, there are about 0.2 more injuries/abductions/surrenders in a given district-month unit. The number of total incidents rises by about 0.15 per month, which is about a 50% increase from the baseline mean. These results are robust across the different parametric specifications. A crude calculation would suggest that these effects

[44]Not controlling for police force changes does not change the results substantially. These results are presented in Table B.14.

translate into between 830 and 1240 more fatalities in the year after implementation.

Panel B reveals that there are no similar impacts of NREGS on insurgency-related violence for Phase 2, where all coefficients are statistically insignificant and typically much smaller in magnitude than the corresponding Phase 1 districts. Given that many red corridor districts were assigned to the first implementation phase, as can be seen by comparing Figures 1 and 2, this result is not surprising since Phase 2 districts tend to have a much lower presence of Maoists than Phase 1 districts.

Since we have monthly data, it is possible to look at dynamic patterns. Focusing on the Phase 1 implementation group, Table V divides the post-treatment period into the short run (Panel A) and the medium run (Panel B). The short run is defined as the first 7 months after NREGS eligibility, whereas the medium run is 8 to 14 months after eligibility. The results show that the bulk of the impact occurs in the short run. Fatalities are somewhere between 5 and 7 times higher, and the total number of incidents are between 2 to 5 times higher in the short run than in the medium run.

This downward trend in violence over time can also be seen graphically in Figures 8 to 12 that plot the estimated RD coefficients separately for each month and for all three parametric specification. The red vertical line denotes the time in which NREGS implementation began officially. Like Table V, the figures reveal that the increase in violence is highest in the early months after NREGS implementation and that violence decreases over time.

5.2 Who is Affected?

Since the data allows us to distinguish between civilians, Naxals and the police force, we can study the impact of NREGS on each of these groups in terms of fatalities, injuries and abductions. According to the predictions from our theoretical model, the police should now have better information to catch the Naxalites and the Naxalites, in turn, may want to retaliate against civilians for helping the police. Thus, we should see the bulk of the impact concentrated on Naxals and civilians. Tables VI and VII report the empirical results of this analysis, focusing again on Phase 1 districts.

Panel A of Table VI presents the RD results for fatalities classified by each of these groups. Civilian casualties rise by about 0.2 deaths a month, whereas Naxal casualties increase by around 0.15 deaths a month after the introduction of the NREGS. The police force does not see a statistically significant increase in fatalities, and the magnitudes are also much smaller.

In Panel B of table VI we create a more comprehensive variable called 'affected' and use it as an outcome measure in the RD analysis. This variable captures deaths, injuries, captures, surrenders and abductions for each of the three populations. Again, the results provide some evidence for Naxals and civilians being more affected by the increase in violence after the introduction of NREGS.

Table VII looks at the fatality outcome by group in the short and the medium run. As the table reveals, civilian deaths are concentrated in the short run, whereas there is no particularly strong time pattern for Naxalites or the police force. The coefficients for the Naxalites are again larger than those for the police, but both are imprecisely estimated. This overall pattern is consistent with the idea that civilians share information with the police, which leads to an overall increase in violence as the police force becomes more efficient, but that this also makes them the target of attacks by the insurgents.

5.3 Spillovers

One interesting extension of the main results is to analyze whether the higher violence in the NREGS districts also had impacts on surrounding districts that did not necessarily already have the program. This is especially true because information about NREGS can travel across district boundaries, but also because the insurgents can cross over into different terrain.

Our theoretical model does not make any direct predictions about spillover effects to neighboring districts. If what makes civilians willing to share information with the police is mainly the promise of development rather than the actual benefits received from the program, however, then we may find that civilians change their behavior even in still untreated districts. This effect may occur especially in Phase 2 districts once Phase 1 districts have started implementation, since the people in those districts can take Phase 1 implementation as a

signal of the government's commitment to following through with the program and may be aware of the fact that their districts will receive the treatment soon. We would then expect to find positive spillover effects of the program. Positive effects could also be due to other changes, however, for example because insurgents are leaving Phase 1 districts where the police are now cracking down on them to find new hideouts in surrounding districts.

Table VIII shows the spillover results, focusing again on the Phase 1 cutoff. The outcome variables here are the average violence levels in a district's neighboring districts, and the regressions control for the number of neighbors and the number of Phase 1 NREGS neighbors a given district has since the number and treatment status of a district's neighbors is not random. The results indicate that NREGS has positive spillovers on surrounding areas on all measures of violence.

An implication of this increase in violence is that there are already treatment impacts in districts that have not yet formally received NREGS. As discussed above, we may expect this effect to be particularly strong for Phase 2 districts that will be treated next. If this is true, we should find that violence is significantly higher in Phase 2 than in Phase 3 districts once Phase 1 receives NREGS, (even though we earlier showed that there is no discontinuity in violence for Phase 2 districts before they receive the program). Table IX shows the results of this analysis, and confirms that this effect does indeed hold empirically. Taken together with the main results for Phase 2 from Table IV, this means that violence levels in Phase 2 districts increase when Phase 1 receives NREGS and that there is no longer any impact once NREGS actually starts being implemented in Phase 2 districts.

5.4 Robustness Checks

The results presented so far show an increase in the incidence of violence, captured by a rise in fatalities, injuries/abductions/surrenders, and the number of incidents overall. While the results are consistent across the parametric specifications presented in the tables, a number of checks can be performed to further test the robustness of the estimates. Since the effects are concentrated in Phase 1, we focus on this implementation phase in these additional

specifications.

One important concern is that there may be measurement error in the rank variable that is used as the running variable, which may lead to districts right at the cutoff being assigned to the wrong side of the cutoff. We provide a robustness check by using a doughnut-hole approach that drops the districts with state-level ranks lying between -1 and 1 (the cutoff is at a state-specific rank of 0). These results are presented in appendix table B.10. They are similar in magnitude and statistical significance to our main results, implying that the estimated treatment effects do not seem to be driven by measurement error of the observations close to the cutoff.

Another potential concern with the main specifications is the nature of the data. All outcomes are count-data outcomes, but we estimate the treatment effects within a normal regression framework rather than using count-data models. Appendix Table B.11 therefore presents the the results from a Zero-Inflated Poisson Count-Data Model. The Poisson model is the most widely used count-data model (Cameron and Trivedi 2013). Since the data has an excess of zero-values (i.e. no causalities in a given district-month), we use the zero-inflated version of this model. The coefficients are interpreted as the change in the log-counts of the dependent variable on introduction of NREGS, and again show the same qualitative patterns as our main results.

Our main results are also robust to a number of other specifications presented in the appendix: Table B.12 estimates the intent-to-treat (ITT) version of the main results, whereas Table B.13 shows the results when the violence data is normalized using 2001 Census district-wise population counts. Table B.14 reproduces the main results without controlling for the strength of the police force and shows that this does not make much difference for the results. Table B.15 presents the main results when varying the bandwidth by restricting the analysis to observations closer to the cutoff. As expected, our results are less precisely estimated the more restrictive the sample definition is. The qualitative patterns are similar for most estimates, however, although the coefficients on two outcomes switch signs in the most conservative specification.

Lastly, Table B.16 presents the results using a difference-in-difference (DID) approach rather than the RD, which is the most common empirical identification strategy used to study the impacts of NREGS in the literature. While the DID approach estimates the overall average treatment effect and therefore a different parameter than the RD specifications, the results are again qualitatively similar. The bulk of the impact again occurs in Phase 1 districts, where fatalities, injuries and total number of incidents rise for the districts that receive NREGS.

5.5 Discussion

Overall, the empirical patterns presented in the results section suggest that insurgency-related violence increases in the short-run and then trends down over time. There is some evidence that this higher level of violence mainly affects civilians and the Naxalites rather than the police, and we find positive spillovers of the program to surrounding but untreated districts.

All of these empirical patterns are consistent with the theoretical predictions of our model in which civilians are willing to share information with the police after NREGS implementation starts, which allows government troops to crack down more efficiently on the Maoists.

A number of alternative theories fail to explain these empirical results. Both the opportunity cost explanation and the credible commitment theory can match the downward trend of violence, but predict that violence should remain constant or fall in the short run rather than increase. A model that focuses on the competition between government and insurgents over the resource pie in a district cannot fully explain the results, either. While such a theory is consistent with a short-run increase in violence, it would predict an upward trend in violence (as more resources are accumulated) that we do not find empirically. Furthermore, such a channel would not necessarily predict the sharp increase in civilian casualties that we find in the short run since insurgents have no reason to target the local population.

Additionally, all of these alternative theories rely on NREGS actually generating economic benefits and, in the case of the last theory, appropriable assets that can be fought over. As mentioned above, NREGS projects mainly focus on drought proofing and land development,

however, which are not easily appropriable resources, and there is growing evidence that the labor-market impacts of the scheme may be low. Our explanation, on the other hand, is also consistent with the government's perception that information-gathering has improved in recent years and with other research like Vanden Eynde (2011) who also finds results consistent with an information-sharing dynamic.

Our proposed mechanism could still be incorrect, however, if the violence impacts occur for a different reason that is correlated with NREGS treatment, but is not due to the anti-poverty program changing civilians' willingness to share information with the police. One such explanation could be a change in the police strategy: As NREGS is a big program that has garnered a lot of attention in the media, the situation in treatment districts may have been more in the spotlight than before the program. This, in turn, could incentivize the state and district leaders to put pressure on the police to work harder than before to ensure a good image of their districts in the press. In consequence, the police in treatment districts may be more efficient than in control districts after NREGS is introduced, but without working through the information-sharing channel.

We cannot rule out such an explanation completely, although some of our empirical results provide evidence against some versions of this explanation: If the effects are driven by police-forces being shifted from neighboring districts into NREGS districts, then the rise in incidents in NREGS districts should mirror a fall in neighboring districts. However, we show that there is a significant rise in violence in neighboring districts as well. Furthermore, if civilians are not sharing information on the insurgents with the police, then neither government troops nor Maoists have an incentive to target them. Yet, we find that civilian deaths increase significantly in the short run. Lastly, potentially the easiest way for the police to increase its efficiency in treatment areas would be to increase the size of the police force. Our main results control for the strength of the police force, however, and excluding these variables does not change our results.

6 Conclusion

This paper has analyzed the impact of introducing a large public-works program in India, the National Rural Employment Guarantee Scheme (NREGS), on incidents of left-wing violence by the Naxalite movement. We exploited the fact that the program was phased in over time according to an algorithm that prioritized economically underdeveloped districts in a regression discontinuity (RD) approach. The results are robust across a number of different specifications and show a substantial rise in fatalities and incidents in the districts that received NREGS in the short run, and a downward trend in violence over time. A myopic snapshot of the impacts would miss the important dynamics of the overall trends. There is some evidence that this higher level of violence primarily affects Naxalites and civilians, with little impact on the police force. The impact is largest among districts that received NREGS in the first phase of the rollout, but there are positive spillovers of violence to neighboring districts before those have access to NREGS.

We have also shown that these empirical patterns are consistent with a model in which civilians share information with the police in response to the 'promise of development' provided by NREGS, whereas the results are difficult to explain with a number of alternative theories in the literature. This theoretical framework may help answer Max Weber's (1965) question on whether the State should use force *or* development to tackle internal conflict. It is possible that for the Indian government, which has been trying to fight the Naxalites for over 30 years, the best strategy may be to combine both force *and* development.

Overall, our paper therefore suggests that civilians may play a key role in internal conflicts that should not be neglected. This paper may therefore be relevant in the broader developing-country context where governments facing internal security threats may be able to use development programs to win the support of civilians. [45]

The conclusions we draw in this paper need to be qualified in an important respect, however: A growing body of literature questions the effectiveness of NREGS as a tool for *actual* development, at least in the short run. This implies that what may win over the rural

[45] While a large number of developing countries experience internal conflicts, Maoist-related violence has been experienced by countries like Ecuador and Peru, Nepal, Phillipines and even Turkey.

community at least initially is the *promise* of development rather than substantial actual changes. Such a mere promise of development may not be a credible enough tool to ensure the aid received from the civilian population over a longer period of time, however. Once civilians realize that the program is not delivering on its promises, this may not only stop civilian aid in exchange for the benefits of the program, but may lead to distrust in government programs in general. Therefore, it is important that the Indian government takes steps to ensure that NREGS is implemented effectively and the promise of development is indeed fulfilled.

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Table I: Prediction Success of Algorithm for Major Indian States

		actual NREGS		prediction success rate	
	district N	Phase 1	Phase 2	Phase 1	Phase 2
Andhra Pradesh	21	13	6	0.90	0.75
Assam	23	7	6	0.91	0.75
Bihar	36	22	14	0.81	1.00
Chhattisgarh	15	11	3	0.73	1.00
Gujarat	20	6	3	0.80	0.93
Haryana	18	2	1	0.72	0.94
Jharkhand	20	18	2	0.85	1.00
Karnataka	26	5	6	0.88	0.52
Kerala	10	2	2	0.77	1.00
Madhya Pradesh	42	18	10	0.76	0.88
Maharashtra	30	12	6	0.93	0.56
Orissa	30	19	5	0.73	0.91
Punjab	15	1	2	1.00	0.93
Rajasthan	31	6	6	0.90	0.72
Tamil Nadu	26	6	4	0.88	0.95
Uttar Pradesh	64	22	17	0.88	0.79
West Bengal	17	10	7	0.76	1.00
Total	447	180	100	0.84	0.82

Table includes all districts with non-missing development index rank for 17 major Indian states (the only missing districts in these states are urban districts according to the Planning Commission report definition from 2003 and therefore include either the state capital or an urban agglomeration of at least one million people). Column 1 provides the number of non-missing rank districts in each state. Columns 2 and 3 give the actual number of treatment districts per state in a given phase of NREGS rollout. Columns 4 and 5 give the success rate of the algorithm in predicting a district's treatment status (NREGS or no NREGS) in a given phase. The proposed algorithm states that the number of treatment districts a state is assigned in a given phase is proportional to the percent of India's poor living in that state, and that this quota should be filled with the least developed districts according to the Indian Planning Commission's ranking of districts (Planning Commission 2003).

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Table II: Baseline Tests

	Panel A: Phase 1				
Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	0.135	-0.0461	0.181	0.00309	-0.0572
	(0.439)	(0.183)	(0.274)	(0.0185)	(0.0678)
Linear Flexible Slope	0.0288	-0.0749	0.104	0.000202	-0.0667
	(0.362)	(0.165)	(0.217)	(0.0167)	(0.0723)
Quadratic	-0.262	-0.265	0.00266	-0.0234	-0.146*
	(0.333)	(0.181)	(0.180)	(0.0186)	(0.0867)
	Panel B: Phase 2				
Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	0.0485	0.0435	0.0137	0.00455	0.000226
	(0.0674)	(0.0575)	(0.0239)	(0.00334)	(0.0104)
Linear Flexible Slope	0.0427	0.0570	-0.00500	0.00665	0.0102
	(0.0869)	(0.0709)	(0.0349)	(0.00605)	(0.0184)
Quadratic	0.0374	0.0536	-0.00653	0.00683	0.00893
	(0.0873)	(0.0692)	(0.0389)	(0.00641)	(0.0193)

Panel A: 231 clusters in 13 months pre-treatment (3003 observations). Panel B: 237 clusters in 13 months (3081 observations). Sample: January 2005 till February 2006.
Controls include time fixed effects and estimated police-force changes. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table III: Summary Statistics

	Mean	Mean	Total
	Red Corridor	**All Districts**	**All Districts**
Deaths	0.441	0.116	2030
Injured/Abducted/Captured	0.553	0.146	2545
Minor Incidents	0.259	0.068	1191
Major Incidents	0.058	0.015	267
Total Incidents	0.317	0.084	1458
Maoists Killed/Injured/Captured	0.433	0.114	1991
Civilians Killed/Injured/Captured	0.357	0.094	1644
Police Killed/Injured/Captured	0.204	0.054	940

A unit of observation is a district in a given month and year (i.e. district-month-year). There are a total of 39 months from January 2005 till March 2008. "Red Corridor" districts are districts with Maoist-related incidents. "Major Incidents" indicates number of Major Incidents as coded by the SATP website. "Total Incidents" is number of total Maoist-related incidents.

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Table IV: Regression Discontinuity

Specification	Panel A: Phase 1 Affected	Fatalities	Injuries	Major#	Total#
Linear	0.514**	0.398**	0.194*	0.0682**	0.170**
	(0.212)	(0.170)	(0.115)	(0.0267)	(0.0770)
Linear Flexible Slope	0.430**	0.298*	0.188**	0.0556**	0.136*
	(0.189)	(0.164)	(0.0896)	(0.0248)	(0.0737)
Quadratic	0.569**	0.448**	0.202	0.0822**	0.153*
	(0.250)	(0.205)	(0.129)	(0.0336)	(0.0884)
	Panel B: Phase 2				
Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	-0.0793	-0.118	0.0285	-0.00350	0.0161
	(0.163)	(0.129)	(0.0839)	(0.00637)	(0.0269)
Linear Flexible Slope	-0.0315	-0.114	0.0787	-0.00283	0.0359
	(0.190)	(0.145)	(0.106)	(0.00620)	(0.0347)
Quadratic	-0.0102	-0.0969	0.0828	-0.00139	0.0395
	(0.184)	(0.135)	(0.111)	(0.00540)	(0.0346)

Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Panel A contains 3234 observations in 231 clusters, and Panel B contains 2844 observations in 237 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table V: The Short Run v The Medium Run

Specification	Panel A: Affected	Short Fatalities	Run Injuries	Major#	Total#
Linear	0.732***	0.513**	0.309**	0.105**	0.236**
	(0.269)	(0.210)	(0.132)	(0.0417)	(0.117)
Linear Flexible Slope	0.682***	0.486**	0.274***	0.102**	0.236*
	(0.256)	(0.209)	(0.106)	(0.0449)	(0.120)
Quadratic	0.865***	0.630**	0.328**	0.142**	0.274*
	(0.324)	(0.264)	(0.135)	(0.0568)	(0.143)
Specification	**Panel B: Affected**	**Medium Fatalities**	**Run Injuries**	**Major#**	**Total#**
Linear	0.354*	0.260**	0.105	0.0391*	0.127*
	(0.212)	(0.130)	(0.158)	(0.0234)	(0.0691)
Linear Flexible Slope	0.270	0.165	0.0974	0.0238	0.0689
	(0.189)	(0.136)	(0.125)	(0.0249)	(0.0820)
Quadratic	0.276	0.244*	0.0458	0.0312	0.0459
	(0.242)	(0.145)	(0.187)	(0.0251)	(0.0780)

Short-run is defined as months 1 through 7 after receiving NREGS. The Medium-run is defined as months 8 through 14 of receiving NREGS. Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Each panel contains 1386 observations in 231 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table VI: Who is Affected?

Panel A:	**Killed**		
Specification	**Civilians Killed**	**Police Killed**	**Maoists Killed**
Linear	0.171**	0.00116	0.188*
	(0.0786)	(0.0525)	(0.103)
Linear Flexible Slope	0.138	-0.0114	0.134
	(0.0850)	(0.0389)	(0.0816)
Quadratic	0.190*	0.0296	0.150
	(0.0977)	(0.0632)	(0.102)

Panel B:	**Injured,**	**Abducted,**	**Surrendered**
Specification	**Civilians**	**Police**	**Naxals**
Linear	0.203*	0.107	0.133*
	(0.113)	(0.0985)	(0.0704)
Linear Flexible Slope	0.178	0.0709	0.117**
	(0.123)	(0.0750)	(0.0554)
Quadratic	0.231	0.142	0.0787
	(0.143)	(0.118)	(0.0526)

Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Regressions consist of 3234 observations in 231 clusters. Unit of observation is district-month.
Panel A documents "Fatalities" (i.e. total number of deaths.) Panel B looks at "Affected": number of persons killed, injured, abducted or captured.

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Table VII: Short Run v Medium Run: Who is Affected?

	Panel A:	**Short**	**Run**
Specification	**Civilians Killed**	**Police Killed**	**Naxals Killed**
Linear	0.352**	0.0114	0.158
	(0.160)	(0.0355)	(0.100)
Linear Flexible Slope	0.351*	0.00590	0.142
	(0.181)	(0.0279)	(0.0926)
Quadratic	0.427**	0.0194	0.144
	(0.208)	(0.0435)	(0.109)
	Panel B:	**Medium**	**Run**
Specification	**Civilians Killed**	**Police Killed**	**Naxals Killed**
Linear	0.0298	-0.0107	0.208**
	(0.0270)	(0.0729)	(0.0956)
Linear Flexible Slope	0.0196	-0.0349	0.135
	(0.0246)	(0.0547)	(0.0931)
Quadratic	0.0148	0.0390	0.158
	(0.0293)	(0.0881)	(0.103)

Short-run is defined as months 1 through 6 after receiving NREGS. The Medium-run is defined as months 7 through 12 of receiving NREGS. Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Each panel contains 1386 observations in 231 clusters. Unit of observation is district-month. "Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table VIII: Spillovers to Neighboring Districts: Phase 1

Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	0.498***	0.262**	0.174***	0.0450***	0.0672**
	(0.150)	(0.110)	(0.0667)	(0.0142)	(0.0321)
Linear Flexible Slope	0.391***	0.196**	0.145***	0.0370***	0.0667**
	(0.111)	(0.0889)	(0.0421)	(0.0118)	(0.0329)
Quadratic	0.972	0.521	0.432	0.0839	0.183
	(0.813)	(0.473)	(0.342)	(0.0654)	(0.156)

Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Regressions contains 3234 observations in 231 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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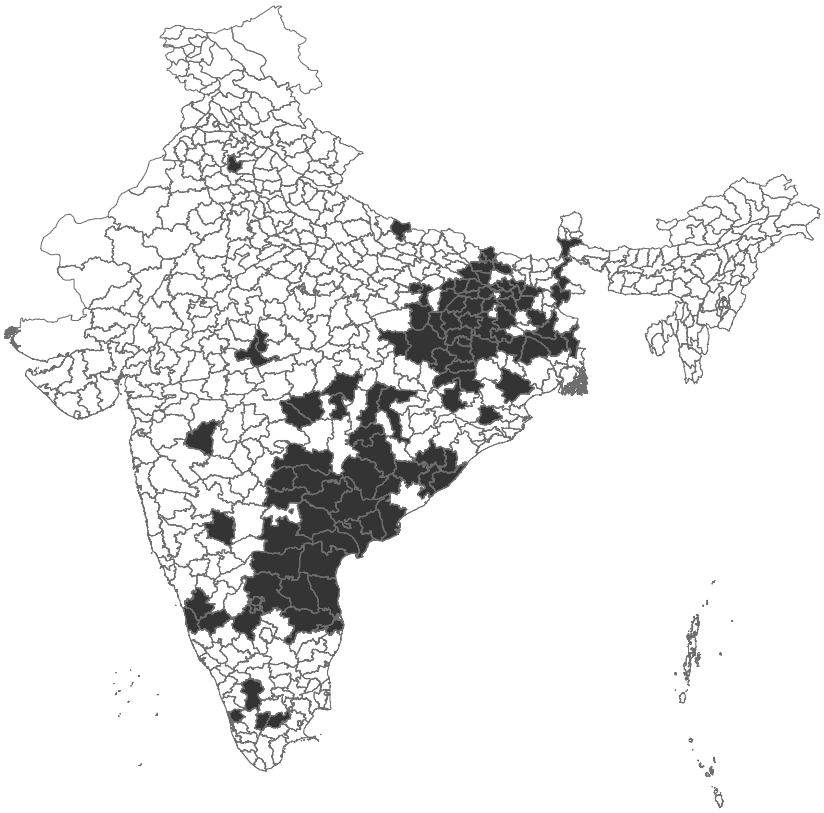
Table IX: Phase 2: Impacts Pre-Treatment

Panel A:	**After Phase 1**	**Treated**	**Before**	**Phase2**	**Treated**
Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	0.102**	0.0598*	0.0428**	-0.000103	0.0322***
	(0.0412)	(0.0317)	(0.0191)	(0.00362)	(0.0102)
Linear Flexible Slope	0.116*	0.0560	0.0617*	-0.00217	0.0233
	(0.0678)	(0.0441)	(0.0361)	(0.00593)	(0.0166)
Quadratic	0.115	0.0516	0.0650*	-0.00267	0.0226
	(0.0712)	(0.0449)	(0.0392)	(0.00611)	(0.0172)

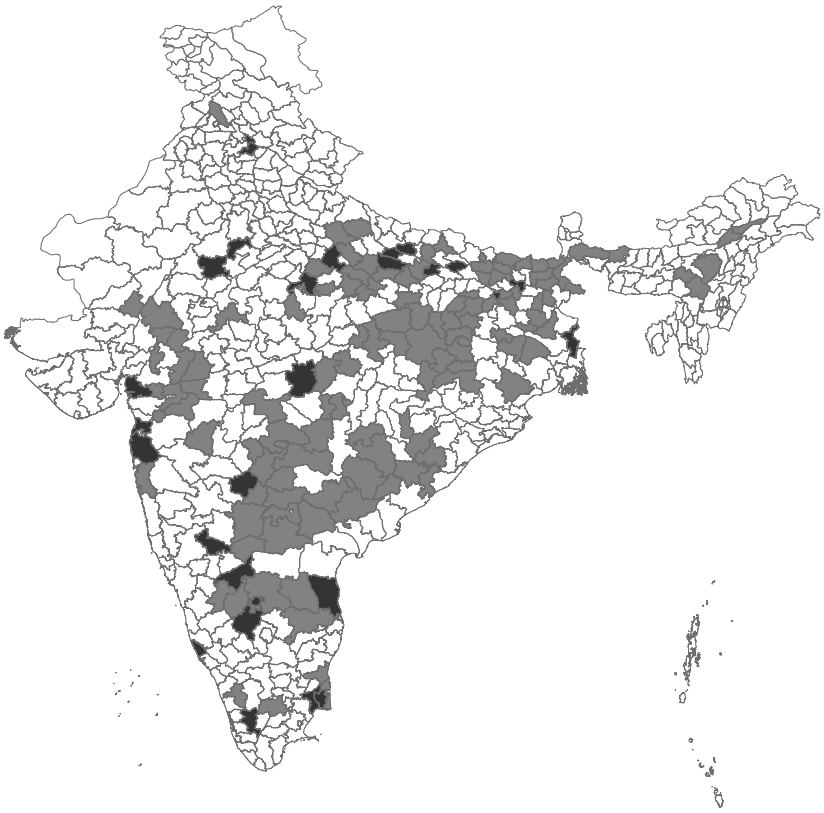
3318 observations in 237 clusters (February 2006 to April 2007).
Controls include time fixed effects and estimated police-force changes. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

Figure 1: Red Corridor Districts



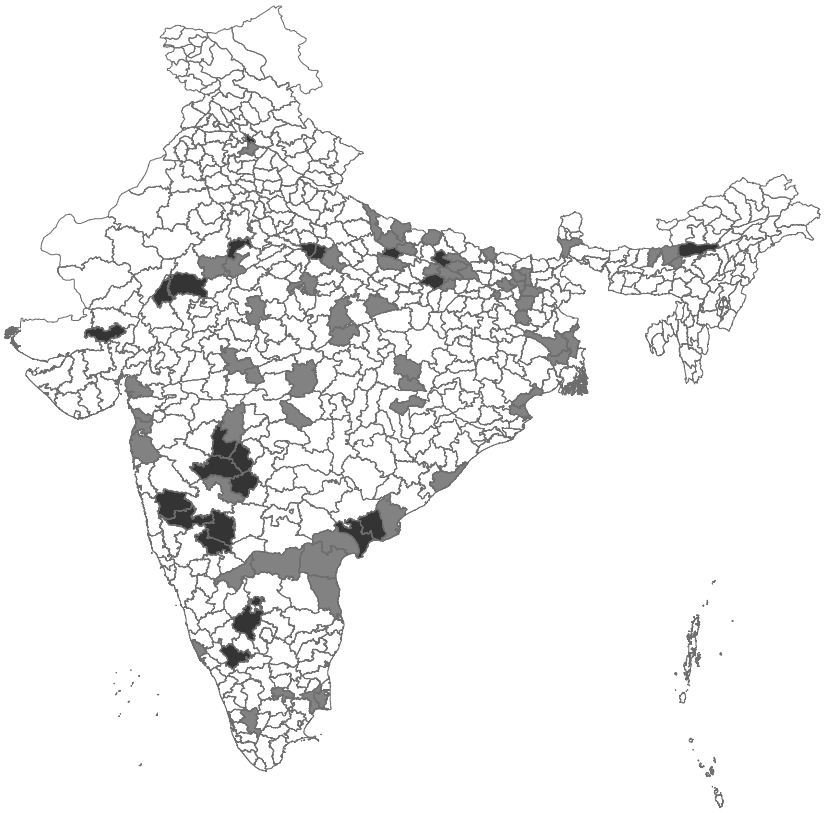
Note: Red corridor districts are all districts that had at least one Naxalite incident in the 36 months of the data used in this paper.

Figure 2: Predicted NREGS Phase 1 Assignment



Note: All colored districts are predicted to receive NREGS in the first phase based on the algorithm. Light grey districts actually receive NREGS in Phase 1, whereas dark grey districts do not.

Figure 3: Predicted NREGS Phase 2 Assignment



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Note: All colored districts are predicted to receive NREGS in the second phase based on the algorithm. Light grey districts actually receive NREGS in Phase 2, whereas dark grey districts do not.

Figure 4: Distribution of Index over State-Specific Ranks (Phase 1 Treatment Assignment)

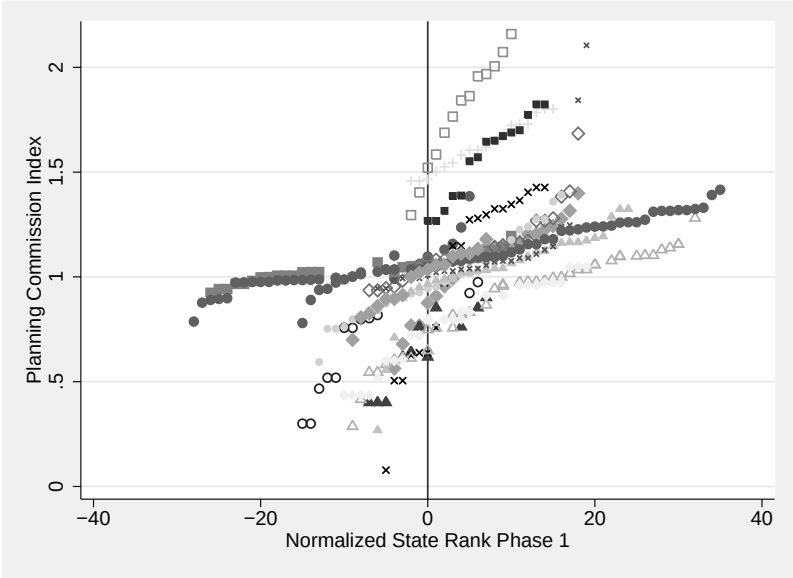


Figure 5: Distribution of Index over State-Specific Ranks (Phase 2 Treatment Assignment)

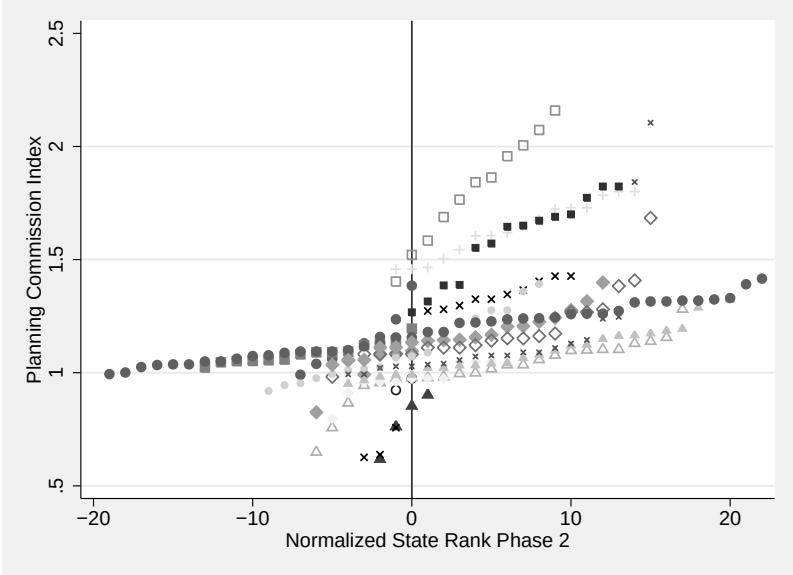
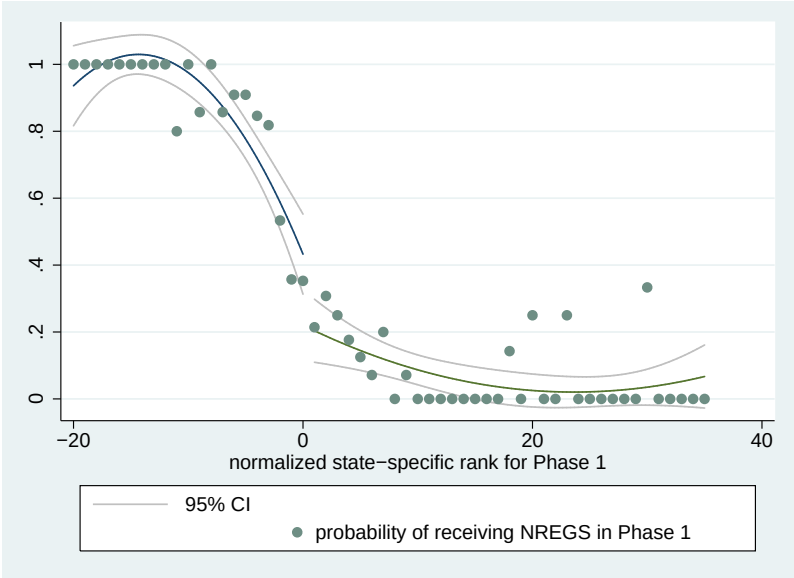


Figure 6: Discontinuity of treatment status for Phase 1



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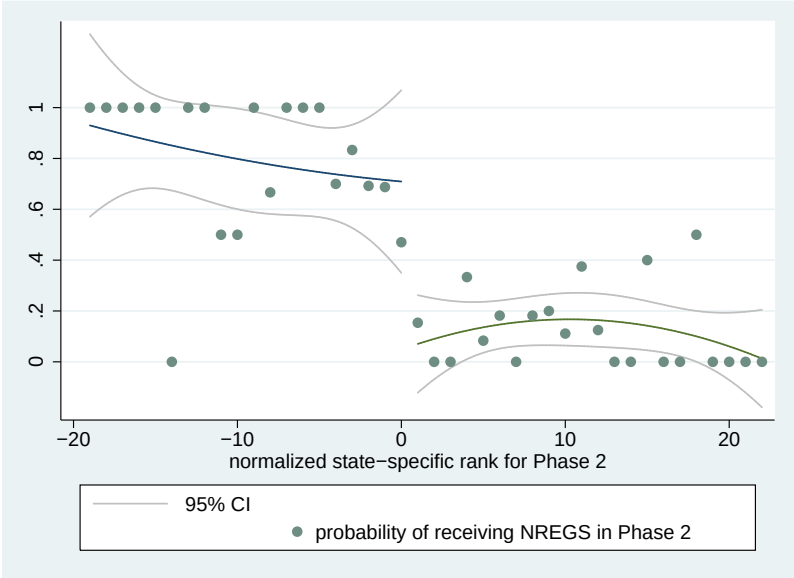
Note: The used bin size is 1, so each individual rank.

Figure 7: Discontinuity of treatment status for Phase 2



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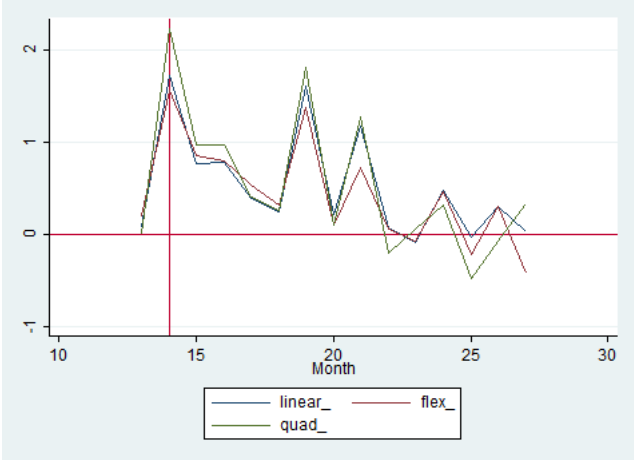
Note: Figure 7 excludes Phase 1 districts. The used bin size is 1, so each individual rank.

Figure 8: Persons Killed, Injured, Abducted or Captured



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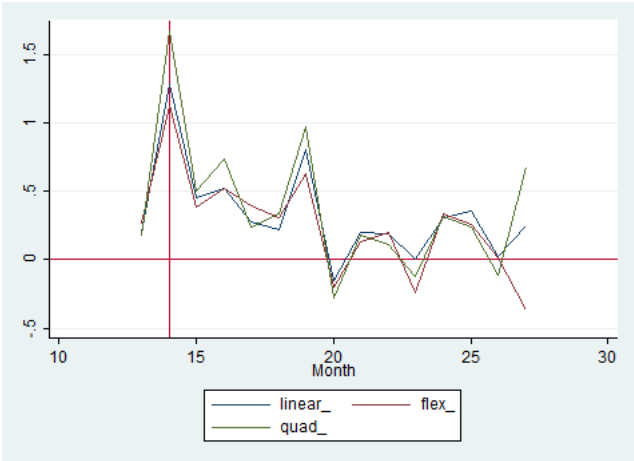
Note: Coefficients of month-by-month RD regressions of persons killed/injured/abducted/captured. Vertical line indicates month of eligibility for NREGS Phase 1. Each point on the graph is coefficient for a different regression restricting the sample to the corresponding month. 'linear' indicates the linear specification; 'flex' indicates the linear specification with flexible slope; 'quad' indicates the quadratic specification.

Figure 9: Total Number of Persons Killed



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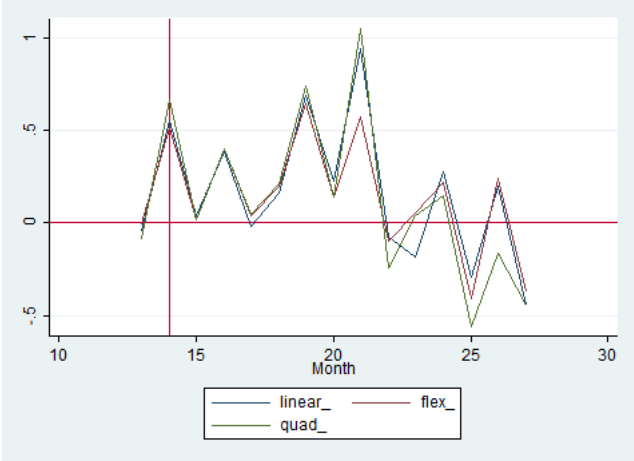
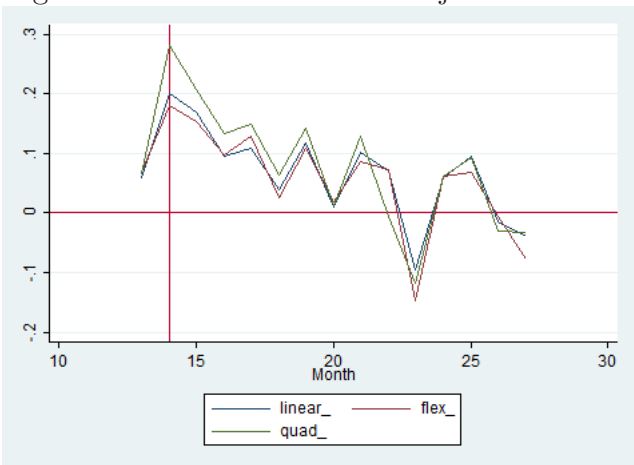
Note: Coefficients of month-by-month RD regressions of total number of persons killed.

Figure 10: Persons Injured, Abducted or Captured



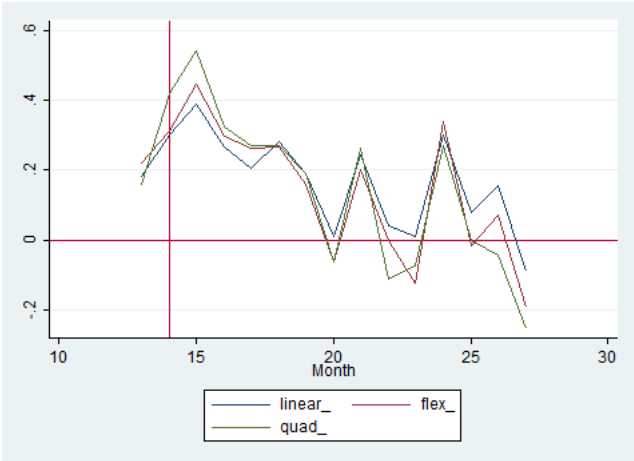
Note: Coefficients of month-by-month RD regressions of total number of persons injured/abducted/captured.

Figure 11: Total Number of Major Incidents



Note: Coefficients of month-by-month RD regressions of major incidents.

Figure 12: Total Number of Incidents



Note: Coefficients of month-by-month RD regressions of total incidents.

Appendices

A Equilibrium of the Model

The pure-strategies sub-game perfect Nash Equilibrium can be solved by backward induction. Civilians choose how much information to provide, i, in order to maximize their expected payoffs $EU_C = u(c+g-n-r)p + u(c-v+s)(1-p)$. Their first order condition is

$$\frac{\partial EU_C}{\partial i} = u(c+g-n-r)p_i(m,v,i) - u(c-v+s)p_i(m,v,i) \leq 0$$

For non-trivial solutions, their best response function is:

$$i^* = \begin{cases} 1 & \text{if } u(c+g-n-r) > u(c-v+s) \leftrightarrow n < g+v-(r+s) \\ 0 & \text{if } u(c+g-n-r) \leq u(c-v+s) \leftrightarrow n \geq g+v-(r+s) \end{cases} \tag{4}$$

NREGS provision and militancy violence will incentivize civilians to aid the government, whereas the threat of retaliation and Naxal services will reduce information flow to the authorities. Therefore, the expected information flow is

$$E(i^*) = P(n < g+v-(r+s)) = \frac{[g+v-(r+s)]-n_l}{n_u-n_l}$$

The last equality comes from the fact that community norms are assumed to follow a uniform distribution.

Solving backwards, the government and Naxalites simultaneously choose their best strategies. The government maximizes their expected payoff $(p(m,v,i) - D(m) - H(g))$ by choosing m and g. Their first-order conditions are [46]:

$$\frac{\partial EU_G}{\partial m} = p_m(m,v,i*) - D'(m) \leq 0$$

and

$$\frac{\partial EU_G}{\partial g} = p_i(m,v,i*)\frac{\partial i}{\partial g} - H'(g) \leq 0$$

Since $E(i^*) = \frac{[g+v-(r+s)]-n_l}{n_u-n_l}$, we know $\frac{\partial i}{\partial g} = \frac{1}{n_u-n_l} > 0$.

Information from civilians makes government military action more effective, $p_{mi}(m,v,i) > 0$. Therefore,

$$\left.\frac{\partial^2 EU_G}{\partial m \partial g}\right|_{v,r,s} = p_{mi}(m,v,i)\frac{\partial i}{\partial g} > 0$$

By the implicit function theorem $\left.\frac{\partial m}{\partial g}\right|_{v,r,s} > 0$ indicating that the government may increase militant action along with service provision (i.e. they are strategic complements).

Naxalites choose $s \geq 0$, $v \geq 0$ and $r_u \geq r \geq 0$, where r_u is the maximum amount of retaliation possible. Since retaliation depends on C's choice, we can assume r and i are chosen simultaneously. In such a case, $r = 0$ when $a = 0$ and $r = r_u$ when $a = 1$. So there is a corner solution for r. When NREGS is implemented, it increases i and therefore $P(a = 1)$, thus causing r to rise (holding constant m, v, s). The Naxalites maximize their expected payoff $(1 - p(m,v,i)) - B(v) - S(s)$, and their first order condition with respect to v is

$$\frac{\partial EU_N}{\partial v} = -p_v(m,v,i) - p_i(m,v,i)\frac{\partial i}{\partial v} - B'(v) \leq 0$$

If we assume that information makes rebel violence less effective, then $p_{vi} \leq 0$. This is because the government can use that information to protect themselves from militant violence and thus preserve their hold on the

[46] The second order conditions are satisfied if $p(m,v,i)$ is concave in m and g. In general, because $p_{mm} \leq 0$ and $p_{ii} \leq 0$ we get the second derivatives of EU_G with respect to m and g to be negative. In order to make stronger statements about concavity, we need assumptions on p_{mi}. For example, if $p = m^\alpha v^{-\beta} i^\gamma$, with $0 \leq m, i$ and $0 < v \leq v^{max}$ and $\alpha + \gamma \leq 1$ is one of many possible functional forms

territory. Therefore,

$$\left.\frac{\partial^2 EU_N}{\partial v \partial g}\right|_{m,r,s} = -p_{vi}(m,v,i)\frac{\partial i}{\partial g} - p_{ii}\left(\frac{\partial i}{\partial g}\right)^2 > 0$$

By the implicit function theorem $\left.\frac{\partial v}{\partial g}\right|_{m,r,s} > 0$, indicating that militant violence, in the short run, rises when the government tries to win the territory by providing public goods g. [47]

[47] This result is the exact opposite of what Berman, Shapiro and Felter (2011) show. This is due to a couple of reasons: (a) in their context of the Iraqi conflict, the rebels seek to inflict violence rather than capture territory. And the government seeks to lower the cost of violence rather than gain control of the territory. In our context, it is more of a territorial dispute. (b) Furthermore, in our model, the probability of winning control also depends on militant violence.

B Additional Tables

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Table B.10: RD: Donut Hole Approach

	Panel A: Phase 1				
Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	0.341**	0.329**	0.0879	0.0511**	0.159**
	(0.168)	(0.160)	(0.0701)	(0.0208)	(0.0649)
Linear Flexible Slope	0.341**	0.299*	0.108*	0.0454**	0.145**
	(0.173)	(0.167)	(0.0653)	(0.0219)	(0.0672)
Quadratic	0.355*	0.362*	0.0743	0.0569**	0.140**
	(0.190)	(0.187)	(0.0668)	(0.0243)	(0.0715)

The Donut-Hole approach tackles issues of measurement error by dropping observations close to the RD cutoff.
Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Regressions contain 2604 observations in 186 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table B.11: Count Data: Zero-Inflated Poisson Regressions

	Panel A:	**Phase 1**			
Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	3.027**	3.336**	1.408**	1.727	1.683**
	(1.269)	(1.441)	(0.644)	(1.061)	(0.800)
Linear Flexible Slope	2.646***	2.949***	1.531***	1.731*	1.355**
	(0.943)	(1.030)	(0.584)	(1.034)	(0.564)
Quadratic	1.639**	2.204**	1.238*	1.244	0.453
	(0.834)	(0.929)	(0.662)	(0.795)	(0.495)

Controls include time fixed effects and estimated police-force changes. Zero-Inflated Poisson regession inflate variables are: baseline dependent variable and one-year lagged dependent variable. Regressions consist of 3234 observations in 231 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table B.12: RD: Intent to Treat: Phase 1

Specification	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
Linear	0.280**	0.217**	0.106*	0.0373**	0.0937**
	(0.115)	(0.0910)	(0.0632)	(0.0144)	(0.0418)
Linear Flexible Slope	0.275**	0.194*	0.117**	0.0358**	0.0881*
	(0.119)	(0.102)	(0.0580)	(0.0156)	(0.0464)
Quadratic	0.301**	0.238**	0.107	0.0437**	0.0821*
	(0.132)	(0.106)	(0.0693)	(0.0175)	(0.0469)

Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Regression contains 3234 observations in 231 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table B.13: Per-Capita Specification

Specification	Affected	Fatalities	Injuries	Major#	Total#
Linear	1.785**	1.333**	0.716**	0.354***	0.868***
	(0.728)	(0.656)	(0.363)	(0.136)	(0.336)
Linear Flexible Slope	1.462**	0.856	0.736**	0.268**	0.674**
	(0.676)	(0.630)	(0.307)	(0.119)	(0.330)
Quadratic	1.652**	1.680**	0.498	0.394***	0.796**
	(0.795)	(0.806)	(0.337)	(0.147)	(0.361)

Dependent variables normalized per-10million persons in the district based on Census 2001 figures. Controls include baseline averages of each dependent variable, time fixed effects and estimated police-force changes. Regressions contain 3234 observations in 231 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table B.14: RD Main Results: Not controlling for Police force

Specification	Affected	Fatalities	Injuries	Major#	Total#
Linear	0.500**	0.410**	0.171	0.0665**	0.165**
	(0.212)	(0.180)	(0.109)	(0.0268)	(0.0804)
Linear Flexible Slope	0.456**	0.358**	0.164**	0.0585**	0.143*
	(0.197)	(0.181)	(0.0830)	(0.0262)	(0.0812)
Quadratic	0.632**	0.568**	0.161	0.0892**	0.174*
	(0.262)	(0.239)	(0.123)	(0.0363)	(0.104)

Controls include baseline averages of each dependent variable, time fixed effects. Regressions contain 3234 observations in 231 clusters. Unit of observation is district-month.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

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Table B.15: Varying the Bandwidth Size

Bandwidth Size	**Affected**	**Fatalities**	**Injuries**	**Major#**	**Total#**
$-x \leq rank \leq x$					
x=10	0.584*	0.441*	0.231	0.0858**	0.172
	(0.342)	(0.243)	(0.190)	(0.0429)	(0.106)
Districts	177	177	177	177	177
x=6	0.461	0.0121	0.348	0.0620	0.0660
	(0.515)	(0.414)	(0.280)	(0.0618)	(0.139)
Districts	138	138	138	138	138
x=3	0.120	-0.0930	0.192	0.0533	-0.0848
	(0.435)	(0.185)	(0.291)	(0.0738)	(0.126)
Districts	80	80	80	80	80

Regressions use the quadratic specification for Phase 1 districts only. "x" is the bandwidth size. The entire first-phase sample consists of 231 districts.

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Table B.16: Difference in Differences

	Affected	Fatalities	Injuries	Major#	Total#
	Panel A:		**Stacked**		
NREGS	0.200**	0.0495	0.151***	0.0103	0.0648***
	(0.0963)	(0.0681)	(0.0433)	(0.00789)	(0.0245)
Observations	16,986	16,986	16,986	16,986	16,986
	Panel B:		**Phase 1**		
NREGS	0.0971	0.190***	0.0460***	0.0194	0.0654**
	(0.0721)	(0.0713)	(0.0161)	(0.0126)	(0.0259)
Observations	12,069	12,069	12,069	12,069	12,069
	Panel C:		**Phase 2**		
NREGS	-0.0113	0.0197	0.0500	-0.00651*	0.0435
	(0.0348)	(0.0422)	(0.0414)	(0.00354)	(0.0409)
Observations	10,564	10,564	10,564	10,564	10,564

"Stacked" indicates combining Phase 1 and Phase 2. Unit of observation is district-month. Controls include time fixed effects and estimated police-force changes. Standard Errors clustered at district level.
"Affected" indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates total number of deaths. "Injuries" indicates number of persons injured, abducted, captured but not killed. "Major #" indicates number of 'Major Incidents' as coded by the SATP website. "Total #" is number of total Maoist-related incidents.

C Additional Figures

Figure C.13: Persons Killed, Injured, Abducted or Captured

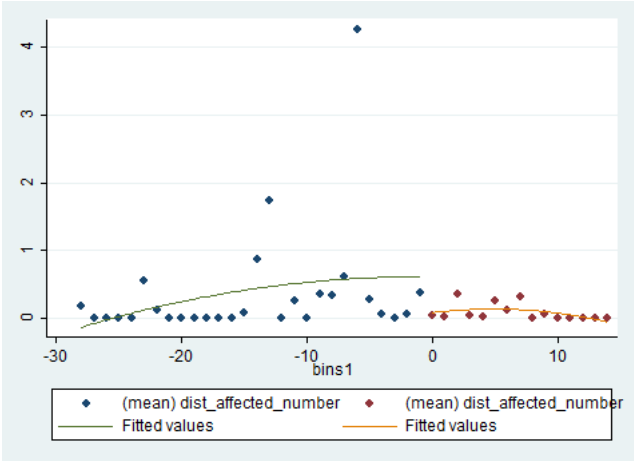
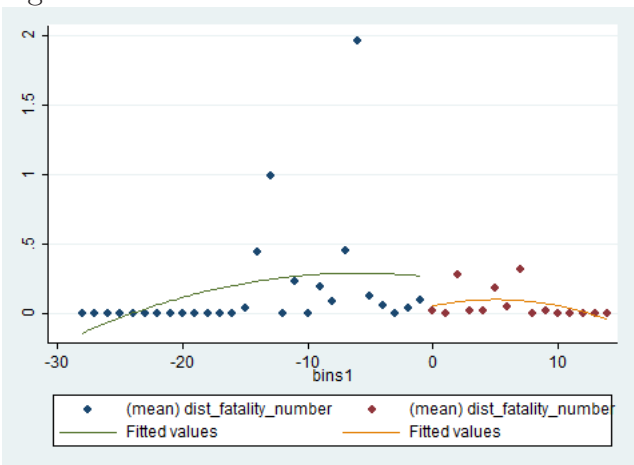


Figure C.14: Total Number of Persons Killed



Note: An observation is the average outcome at a given rank, so the bin size is 1. Fitted curves are quadratic polynomials.

Figure C.15: Persons Injured, Abducted or Captured

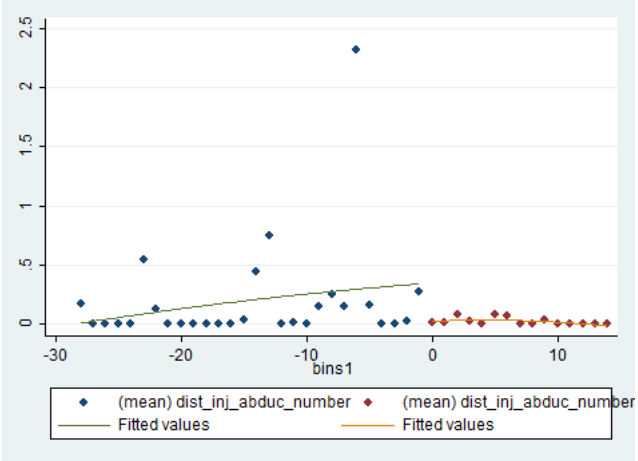


Figure C.16: Total Number of Major Incidents

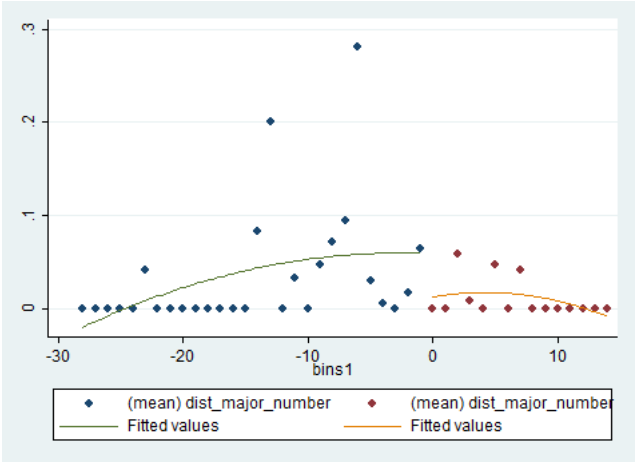
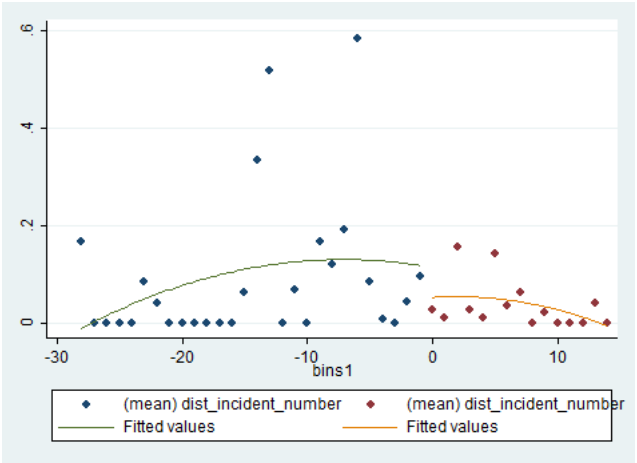


Figure C.17: Total Number of Incidents



Note: An observation is the average outcome at a given rank, so the bin size is 1. Fitted curves are quadratic polynomials.