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Guns *and* butter? Fighting violence with the promise of development

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A B S T R A C T

There is growing awareness that development-oriented government policies may be an important counter-insurgency strategy, but existing papers are usually unable to disentangle various mechanisms. Using a regression-discontinuity design, we analyze the impact of one of the world's largest anti-poverty programs, India's NREGS, on the intensity of Maoist conflict. We find short-run increases of insurgency-related violence, police-initiated attacks, and insurgent attacks on civilians. We discuss how these results relate to established theories in the literature. One mechanism consistent with the empirical patterns is that NREGS induces civilians to share more information with the state, improving police effectiveness.

1. Introduction

Internal military conflicts between government troops and insurgents are common in many developing countries. Governments have traditionally relied heavily on military force, but there is a growing awareness that this alone may not be enough to end violence since insurgents often rely on the loyalty of the local population in their guerrilla tactics and recruit members from economically marginalized groups. In such situations, government anti-poverty programs are increasingly seen as a potential tool for reducing conflict intensity by raising the opportunity cost of being an insurgent and improving the willingness of civilians to support the government.[1] At the same time, however, such programs may increase violence, for instance the resources flowing into conflict areas may make territorial control of these locations more attractive for insurgents.[2]

What effect government programs have on internal conflict intensity is therefore an empirical question. Across a number of different countries and types of programs, recent papers find both positive and negative impacts of government programs on internal conflicts that are typically consistent with more than one explanation.[3] Given this heterogeneity, a deeper understanding of how government programs of different types and across different contexts affect internal violence is of high policy relevance.

In this paper, we analyze the impact of the world's largest public-works program, the Indian National Rural Employment Guarantee Scheme (NREGS), on the incidence of Maoist violence in the country, which the Indian Prime Minister referred to as the “single biggest internal security threat”.[4] NREGS is based on a legal guarantee of 100 days of public-sector 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 goal is to generate labor market opportunities, one of the expectations of the government was to reduce incidents of Maoist-related violence.

Based on the existing literature, it is unclear how NREGS should be expected to affect insurgency-related violence. NREGS operates on a much larger scale than the programs analyzed in the existing within-country analyses, and large implementation problems especially in the initial stages seem to have severely limited the monetary benefits for the poor. Furthermore, as a public-works program, the employment guarantee scheme is a different type of government intervention than the ones analyzed in the literature. These differences in context,

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[1] See e.g. Grossman (1991) for an opportunity cost model and Berman et al. (2011b) for a model of civilian support in the context of street gangs.
[2] See e.g. Hirshleifer (1989), Grossman (1991), and Skaperdas (1992).
[3] See e.g. Berman et al. (2011a, 2011b), Nunn and Qian (2012), Crost et al. (2012), and Dube and Vargas (2013).
[4] Hindustan Times, April 13, 2006: Naxalism biggest threat: PM.

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delivery procedures, and scale may have important consequences for the relevance of the various mechanisms via which NREGS may affect conflict.[5]

Our empirical estimation strategy relies on the fact that 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: in the first step, each state received a quota of treatment districts proportional to the prevalence of poverty in that state, and in the second step this quota was then filled with the poorest districts according to districts being ranked on a poverty index. This procedure generates state-specific treatment discontinuities and allows the use of a regression-discontinuity design to analyze the empirical impact of the program. The results show that treatment at the cutoff leads to about 914 more fatalities in about 368 additional incidents over the following year. We find that more attacks are initiated by the police, that insurgents are the most affected group, and that there is little impact on police casualties. There is also some evidence of an increase in the number of attacks by insurgents on civilians. The results are robust across different specifications and predominantly concentrated in the short run.

We discuss the empirical predictions of the most prominent theories in the existing literature. While a public-works program like NREGS may be seen as a combination of an employment intervention and an infrastructure program, the program in its early days hardly seemed to create any non-public assets or destroyable infrastructure (Ministry of Rural Development, 2010). This means that NREGS does not provide many appropriable assets and limits the opportunities for the insurgents to sabotage the scheme. While the public-works scheme also suffers from implementation problems, the actual and especially the expected future benefits from the scheme may therefore play a larger role in explaining the empirical patterns.[6]

Overall, our paper contributes to our understanding of the impact of government programs on insurgency-related violence in a number of ways. First, the empirical findings suggest that NREGS led to an increase in violence in the first year of implementation, and especially the first few months. This means that dynamic patterns are important, which so far have been largely ignored in the literature. Second, the results and circumstantial evidence are consistent with a citizen-support explanation in which the introduction of NREGS makes civilians more likely to assist the state in the fight against insurgents, although we cannot fully reject other non-mutually excludable explanations, such as a battle over expected future resources. Third, while most of the existing literature focuses on programs that are implemented quite well, the Indian context provides the often more realistic case of a government initiative that at least initially faced severe implementation issues. Our results paired with other evidence from the literature suggest that the promise of development in the form of anticipated program benefits may already have important consequences for conflict intensity. Fourth, in contrast to most of the existing literature that focuses on infrastructure programs and food-aid schemes, NREGS is mainly a job-creation program. Based on our results, the impacts of a public-works program on violence are more similar to infrastructure programs (Crost et al., 2014) and food-aid schemes (Nunn and Qian, 2012) than US-implemented reconstruction programs (Berman et al., 2011b) at least in the short run, albeit for plausibly different reasons. Fifth, the program in question is much larger in scale than the other studied programs and the conflict has been the major internal security threat for one of the world's largest countries since the late 1960s.

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

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”.[7] Members of the movement are typically called Naxalites or Maoists.

Naxalites have been operating since 1967, but violence exacerbated after the two biggest previously competing Naxalite groups joined hands to form the Communist Party of India (Maoist) in 2004 (Lalwani, 2011). The Indian Home Ministry believed the movement to have around 15,000 members in 2006, and to be active in 160 districts (Ministry of Home, 2006). Fig. 1 shows all the districts that experienced at least one Maoist incident between January 2005 and March 2008, the period studied in this paper, in black, dark grey and light grey. As can be seen, 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. Decades of using military force have been largely unsuccessful in suppressing the movement. A number of researchers note that India traditionally relies almost exclusively on military strength to fight the Naxalites (see e.g. Banerjee and Saha, 2010; Lalwani, 2011). Many observers also refer to the often widespread disregard for local perceptions as well as the sometimes excessively brutal nature of police force behavior that affects many civilians (Bakshi, 2009; Lalwani, 2011; Sundar, 2011).

Both Maoists and security forces believe that civilians have a lot of information on the insurgents, so pressures on the local tribal population (called *adivasis*) to pick a side and cooperate with one of the conflict parties are high. The Naxalites' continued survival depends on help from civilians who hide them and provide them with resources and information. Maoist insurgents often warn the local population not to provide shelter or information to police forces, for example, and instead ask them to keep track of government personnel and their actions. *Adivasis* also face economic incentives to join the conflict: many areas face chronic underdevelopment, and since their knowledge of local conditions in the often remote forest areas is very valuable, working for one of the conflict parties allows the poor to earn some income (Mukherji, 2012).

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.[8] 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, accusing them of being police informers, are also in line with the idea that Maoists retaliate against civilians who help the police.[9]

In light of this complex situation, the view that military force alone is not effective in solving the Naxalite problem in the long run seems to

[5] See e.g. Berman et al. (2013).

[6] See e.g. Dutta et al. (2012) and Niehaus and Sukhtankar (2013) for implementation issues with NREGS.

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

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

[9] See Online Appendix for some examples and details about the connection between the Maoist conflict and politics.

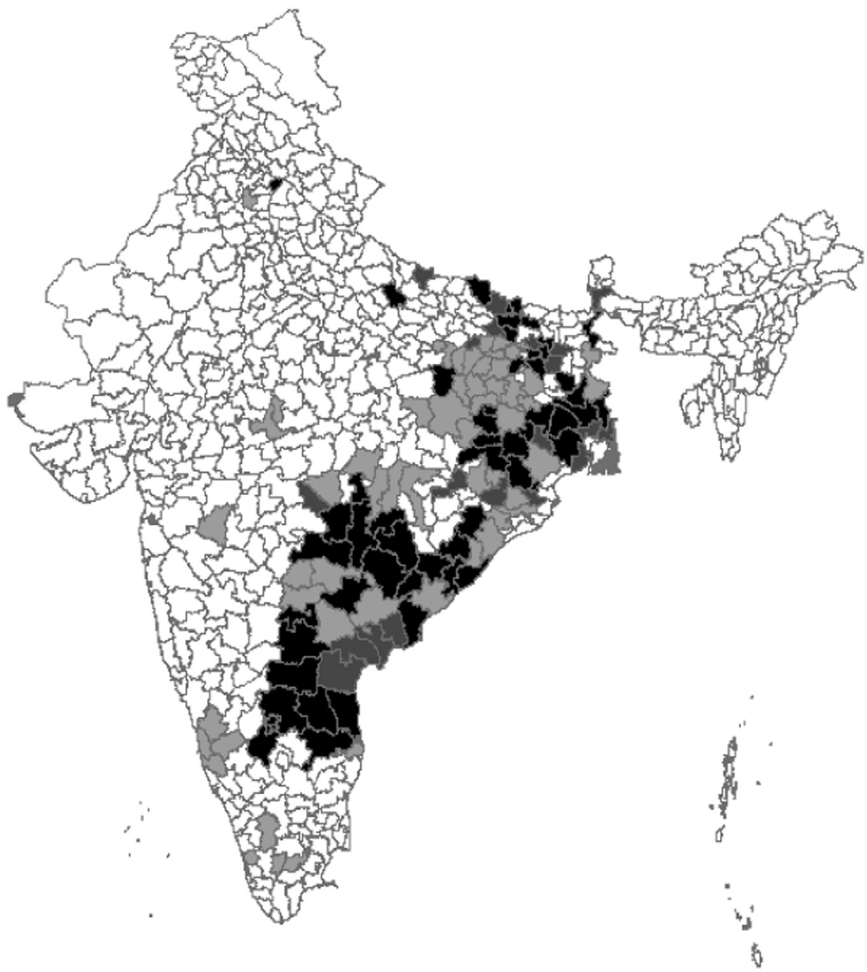


Fig. 1. Red Corridor Districts and NREGS Phase. *Note*: Red corridor districts are districts that had at least one Naxalite incident in the analysis period (January 2005–March 2008). Red corridor districts predicted to receive NREGS in the first, second, and third phases based on the algorithm are in black, dark grey, and light grey, respectively.

have grown in recent years. The central government has shown a growing interest in increasing economic development in underdeveloped areas 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). NREGS is by far the most ambitious and largest anti-poverty program introduced by the Indian government.

Conflict intensity seems to have decreased in recent years. The Maoists have been forced to move out of many traditional areas of their control (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.[10] 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.[11]

2.2. NREGS

The National Rural Employment Guarantee Scheme (NREGS) 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. 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.[12]

NREGS was rolled out non-randomly in accordance with a poverty ranking 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 operates in all rural districts in India (Ministry of Rural, 2010).

Many of the poorest Indian districts are also those heavily affected by Naxalite violence, as can be seen from Fig. 1. The figure shows Maoist-affected districts predicted to receive NREGS in Phase 1, Phase 2, and Phase 3 in black, dark grey, and light grey, respectively. A large proportion of Maoist-affected districts are poor enough to be assigned to the first implementation phase.

An emerging literature suggests that implementation issues may substantially limit the effectiveness of the program, with widespread rationing of NREGS employment especially in poorer states and corruption in the form of underpaid wages and ghost workers (Dutta et al., 2012; Niehaus and Sukhtankar, 2013). The available information on the implementation of NREGS suggests that NREGS creates hardly

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

[11] See Online Appendix for details.

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

any appropriable assets. A breakdown of project categories reveals that NREGS focuses on drought-proofing measures and does not generate a lot of infrastructure improvement or physically appropriable assets.[13] Since we focus on a similar time interval as the existing literature, the impacts of the scheme on violence in this paper are therefore unlikely to be driven by any substantial household income increases or by a fight for the direct control of the assets created by NREGS, although this does not rule out that expected monetary inflows to treatment districts make territorial control of those areas more attractive.

NREGS differs from previous, mostly unsuccessful anti-poverty programs because of its legal status, scope, and prominence in the government's agenda, which may have made the promise of development more credible.[14] Zimmermann (2015) finds effects of NREGS on general election outcomes that are consistent with such a view: districts that had just started implementing NREGS in the year prior to the election were more likely to vote for the government and seemed less sensitive to implementation quality than areas with longer access to the program.

A number of papers also analyze the impact of the employment guarantee scheme on rural Indian labor markets. Using difference-in-difference approaches, empirical analyses often suggest low overall benefits but positive impacts on public employment and private-sector wages in the agricultural off-season, in areas with high implementation quality, and among casual workers (Azam, 2012; Berg et al., 2012; Imbert and Papp, 2015). Zimmermann (2013) uses a regression-discontinuity framework and finds that NREGS is primarily used as a safety net rather than as an additional form of employment and does not lead to an overall increase in public-sector employment, the casual private-sector wage or household income. Taken together, the empirical literature on NREGS therefore suggests that while there may be important heterogeneous impacts, overall NREGS does not raise the opportunity cost of other occupations in the traditional sense of offering a better paid job, although the program may affect opportunity costs through occupational changes induced by the safety net (Zimmermann, 2013).

Two concurrent papers in the literature analyze the impact of NREGS on Maoist violence and discuss potential explanations for their results.[15] Fetzer (2014) shows that NREGS attenuates the relationship between rainfall shocks and Maoist violence, which he attributes to a decline of the importance of income fluctuations as a driver of Maoist violence once NREGS provides a safety net during bad economic times. Dasgupta et al. (2014) use a difference-in-difference approach and find that NREGS lowers Maoist violence in the long run. This effect is concentrated in Andhra Pradesh, a state with high implementation quality relative to other areas. The authors interpret this effect as evidence of the rising opportunity cost of becoming a rebel and forgoing higher wages in the labor market.

We find a violence increase after the introduction of NREGS using a regression-discontinuity design, which is most consistent with a different mechanism than the one put forward in these two papers. We focus on the short- to medium-run impacts of NREGS on violence, as longer-run impacts of NREGS would require comparing Phase 1 and Phase 3 districts of NREGS, violating RD assumptions. The empirical results in Dasgupta et al. (2014) focus on the long run, on the other hand, and the results in Fetzer (2014) can be interpreted as a long-term effect once the system is in equilibrium. We further explain this point in our discussion of the results below.

In the next section, we provide details about the most established conflict theories and their implied predictions for the impact of NREGS on Maoist violence.

3. Theories about the impact of government programs on violence

There are a number of existing theories in the broader literature on the relationship between development and conflict that are relevant for the impact of government programs on violence. Two prevalent theories in the literature predict a fall in conflict intensity. The first theory is an opportunity-cost story: if the program provides jobs and other welfare benefits, it will increase the opportunity cost of being a Maoist. This should make retention and recruitment of rebels more difficult and decrease their ability to inflict violence (see e.g. Grossman (1991) for such a model).[16]

The second theory that predicts a fall in violence after the introduction of NREGS is a citizen-support or 'hearts and minds' explanation. The introduction of a government program like NREGS may improve the relationship of the state and its citizens by making the government's commitment to economic development more credible. This may make civilians more willing to share information with the police, which improves police effectiveness in tracking down insurgents, and in the long run leads to a decrease in violence as the insurgents lose the fight (see e.g. Berman et al. (2011b) for a model on counterinsurgency in Iraq and the Akerlof and Yellen (1994) study on street-gangs).

In contrast to these two theories, there are potential mechanisms under which we should expect an increase in violence. The first is a short-run version of the citizen-support channel, which is based on the idea that increased citizen support may well lead to an initial increase in violence through more police attacks and potential retaliatory attacks by insurgents on civilians before violence decreases in the longer run. In the Appendix, we develop a model of citizen support that takes into account potential dynamic patterns. It sets up a two-stage game between the government, the insurgents and the civilians. Unlike similar models in the literature (Berman et al., 2011b), insurgents try to acquire territorial control and can affect the probability of control by increasing the number of attacks against the police. Civilians choose how much information to share with the police, whereas the police and rebels choose the amount of military action to take. In equilibrium, the model predicts that the introduction of a government program will lead to an increase in the support provided by civilians to the police due to experienced or expected future program benefits.[17] This leads to a violence increase, which is driven by police-initiated attacks and retaliatory attacks by the insurgents against civilians. Violence levels are high in the short run, but should fall over time as the government starts winning the conflict due to better information.

A second reason for a violence increase is based on the idea of a competition for resources. NREGS may enlarge the size of the resource pie that is worth fighting over (Hirshleifer, 1989; Grossman, 1991; Skaperdas, 1992): contest models that focus on this channel usually predict that when resources rise in a region in equilibrium more effort will be put into fighting than into production. If the competition for resources channel is important, we would expect to find empirically that both rebel attacks against police forces and police-initiated attacks against the insurgents increase over time as more assets are created, but there is little reason to expect an increase in violence against civilians. The most straightforward version of this channel in the Indian context presupposes that NREGS generates appropriable re-

[13] According to Ministry of Rural (2010), the breakdown of projects for the financial year 2008–2009, was 46% water conservation, 20% provision of irrigation facility to land owned by lower-caste individuals, 18% land development, 15% rural connectivity (roads), and 1% any other activity.

[14] See the Online Appendix for extensive qualitative evidence on this point.

[15] In a policymaker-oriented extension of this paper, we show that our results are also robust to using a simple difference-in-difference strategy and provide some descriptive evidence of dynamic patterns of Maoists arrests and surrenders (Khanna and Zimmermann, 2014).

[16] This idea is also closely related to work on economic inequality and group formation in the conflict literature. See e.g. Grossman (1999) and Fearon and Helpman (2007).

[17] See Online Appendix for a more detailed discussion on why civilians may want to cooperate with the police.

sources, which as mentioned in the Background section above is unlikely to be true. Therefore, a more likely variant of the competition for resources explanation relies on the appropriation of money coming into the treatment districts instead.

A third mechanism that predicts an increase in violence is that NREGS may put a spotlight on treatment areas, encouraging the police to increase their crime reduction efforts there. As NREGS is a big program that has garnered a lot of media attention, this could incentivize state and district leaders to put pressure on the police to work harder to ensure a good image of their districts in the press, for example. This increased police effort implies the same pattern as the citizen-support channel, with an increase in violence and especially of police-initiated attacks. The spotlight theory should encourage the police to crack down on other forms of crime as well to make the security situation in their district look good, however. Moreover, there is no retaliation motive against civilians in this case.

Most of these different theories can be disentangled by focusing on the implied patterns of changes in violence and of the most heavily affected groups in combination with other circumstantial evidence provided in the background section and in the Online Appendix. To test the different explanations empirically, we exploit the roll-out of the program in a regression-discontinuity design.

4. Identification strategy, data and empirical specification

4.1. NREGS roll-out and the assignment algorithm

The Indian government used an algorithm to determine which districts would start implementing the program in which phase. Zimmermann (2013) reconstructs the algorithm from information on the NREGS roll-out 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. Second, the specific treatment districts within a state are chosen based on a development ranking, starting with the poorest districts.

We use this procedure in our empirical analysis. The 'prevalence of poverty' measure is the state headcount ratio times the rural state population, which provides an estimate of the number of below-poverty-line people per state. A state is 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 to 1994 nationally representative National Sample Survey (NSS) data.[18]

The development index used to rank districts within states comes from a Planning Commission report from 2003 that created an index of economic underdevelopment. The index was created from three outcomes for 17 major states: agricultural wages, agricultural productivity, and the district proportion of low-caste individuals – Scheduled Castes and Scheduled Tribes (Planning Commission 2003).[19] Districts were then ranked on their index values. In addition to the algorithm, the government had a separate list of 32 districts heavily affected by Maoist violence.[20] These districts were not subject to the algorithm and all received NREGS in the first implementation phase. In order to closely replicate the algorithm used, we drop these districts from our sample. Our results are robust to including them and assigning them a predicted treatment status based on their economic development index values.

The two-step algorithm results in state-specific treatment cutoffs. 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 correspond to the Phase 1 and Phase 2 NREGS roll-out, respectively. We exploit both cutoffs in a regression-discontinuity framework.

Ranks are made phase- and state-specific and are normalized so that a district with a normalized state-specific rank of zero is the last program-eligible district in a state in a given phase.[21] This allows the easy pooling of data across states.

The overall prediction success rate of the assignment algorithm is 83 percent in Phase 1 and 82 percent in Phase 2. It is calculated as the percent of districts for which predicted and actual treatment status coincide.[22] This means that there is some slippage in treatment assignment in both phases, and considerable heterogeneity in the performance of the algorithm across states.[23] Nevertheless, the algorithm performs quite well in almost all states and the prediction success rates are also considerably higher than the ones that would be expected from a random assignment of districts, which are 40.27 percent for Phase 1 and 37.45 percent for Phase 2, respectively.[24] Overall, this suggests that the proposed algorithm works well for predicting Phase 1 and Phase 2 district allocations.

The RD framework crucially relies on the assumption that beneficiaries were unable to perfectly manipulate their treatment status, so that observations close to the treatment cutoff 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 the algorithm in either step. This seems plausible: the headcount poverty ratio used data from the mid-1990s, which had long been available by the time the NREGS assignment was made. The economic underdevelopment index was also constructed from outcome variables collected in the early 1990s, eliminating the opportunity for districts to strategically misreport information. Additionally, the suggestion of the original Planning Commission report was to target the 150 least developed districts, but the eventually implemented NREGS cutoffs were higher than this in Phase 1 (200 districts in Phase 1). Lastly, the Planning Commission report lists the raw data as well as the exact method by which the development index was created.[25]

Figs. 2a and b look more closely at the distribution of index values over state-specific ranks. They 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 smooth at the cutoff of 0, again suggesting that manipulation is not a big concern.

Another way of analyzing whether manipulation is likely to be a

[18] We use the state headcount ratios from Planning Commission (2009), since the original headcount ratio calculations do not have estimates for new states that had been created since then. As official Planning Commission estimates, they are likely to be closest to the information the Indian government would have had access to at the time of NREGS implementation.

[19] Data on the outcome variables 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. There are no Maoist-related incidents in NREGS districts in the dropped states in our sample period.

[20] See e.g. Planning Commission (2005).

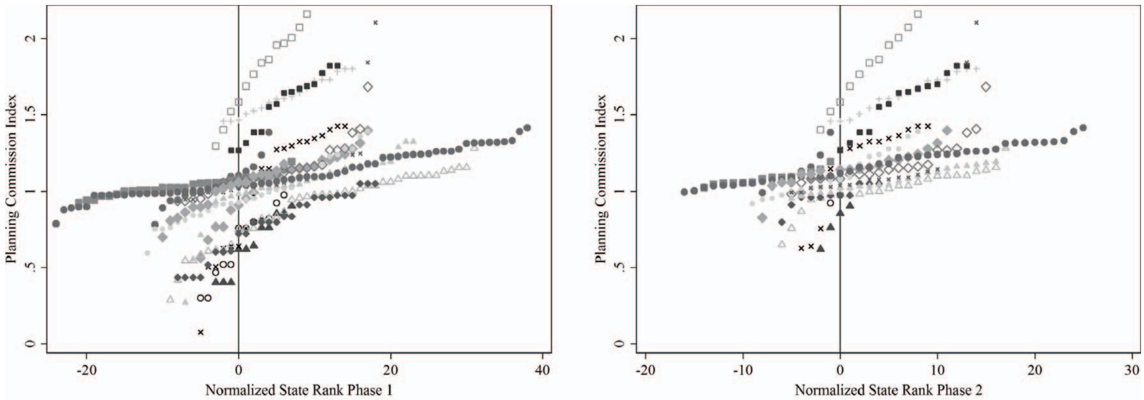
[21] Rank data in the 17 major states is complete for all rural districts. Rank data is available for 447 of 618 districts. Data for the index creation was unavailable in some states, in most cases because of internal stability issues during the early 1990s when most of the data was collected. We exclude these states from the analysis.

[22] Prediction success rates for Phase 2 are calculated after dropping Phase 1 districts.

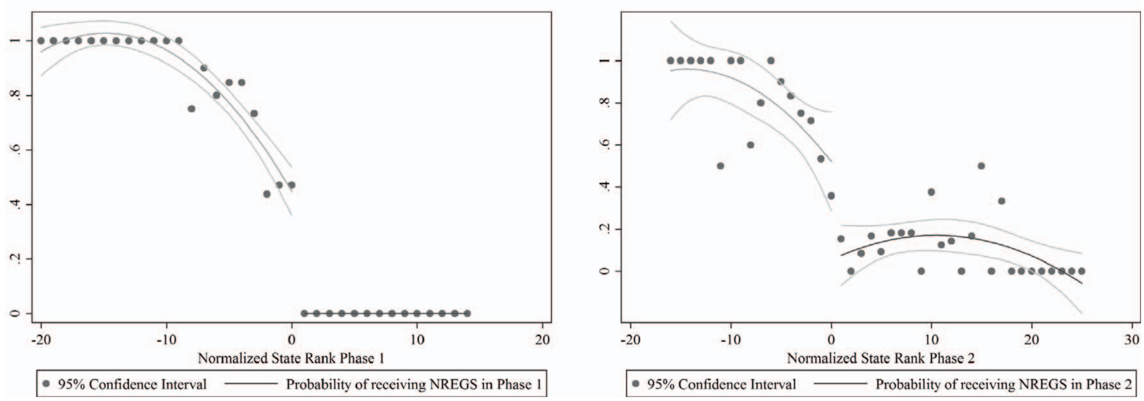
[23] See the Online Appendix for details on how the political reality of Indian politics may explain the slippage and why we do not think that the fuzziness of the discontinuity creates problems for internal validity or the representativeness of the estimates reported in this paper.

[24] Part of the fuzziness of the treatment discontinuity is potentially due to measurement error in the headcount ratios if the Indian government used different values than the ones reported in Planning Commission (2009). See Online Appendix for details.

[25] Even though predicted assignment was non manipulable, this does not mean that actually receiving the program was not subject to political pressures. It can be shown that deviations from the algorithm are correlated with party affiliation. See the Online Appendix for details on how the political reality of Indian politics may explain the slippage and why we do not think that the fuzziness of the discontinuity creates problems for internal validity or the representativeness of the estimates reported in this paper.



(a) Distribution of Index - Phase 1 (b) Distribution of Index - Phase 2



(c) Phase 1 Discontinuity (d) Phase 2 Discontinuity

Fig. 2. Distribution of Index and discontinuities by phase. *Note*: First row plots the distribution of the index by state. Second row shows the treatment discontinuities for each phase, dropping the phase far away from the cutoff (Phase 3 in (c), Phase 1 in (d)). Negative and zero normalized state rank numbers are districts that should have received NREGS based on the government algorithm, whereas positive numbers are assigned to districts that should have been ineligible.

problem is to test whether there are any discontinuities at the cutoffs in the baseline data. Table 2 presents the results of such an analysis for the main outcome variables used in this paper for the time period before NREGS was rolled out to any phase, and Fig. 3 shows the results graphically. Appendix Table A2 and Fig. A1 present baseline results for employment, wages and education from a large household-survey, and in the Online Appendix we show balance on a number of other demographic, geographic and political baseline variables. Overall, these tables and figures suggest again that manipulation is not a problem.

Finally, we need to verify that there really is a discontinuity in the probability of receiving NREGS at the state-specific cutoff values. Figs. 2c and d 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 about 40 percentage points at the discontinuity in both phases. This suggests that there is indeed a discontinuity in the treatment probability at the cutoff.

4.2. Data and variable creation

The primary source of data used in this paper comes from the South Asian Terrorism Portal (SATP). The SATP aggregates and summarizes news reports on Naxalite-related incidents, and such summaries usually contain the location of the incident (district), the date of the incident, the number of casualties (Naxalites, civilians, or police), and the number of injuries, abductions or surrenders. The source also codes the incident as 'minor' or 'major.'

In many cases, the party initiating an incident can be identified from the newspaper description, and we manually code up these details for the incidents in our sample. Events are labeled as police initiated, Maoist initiated against the government or Maoist incidents against civilians.[26]

Using this information we construct violence intensity variables at the district-month level, with 'no incidents' being coded as zero. If some information is unclear, we verify 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 districts in the final phase started receiving NREGS in April 2008. This gives us data before and after implementation of the program, 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 Planning Commission (2003).

Table 1 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 0.32 incidents a month .

[26] See the Online Appendix for more information about the dataset and potential limitations as well as some coding examples.

Table 1
Summary statistics.

	Mean Red corridor districts	Mean All districts	Total All districts
Deaths	0.441	0.116	2030
Injured/abducted/ captured	0.553	0.146	2545
Affected	0.994	0.262	4575
Major Incidents	0.058	0.015	267
Total Incidents	0.317	0.084	1458
Maoists killed	0.162	0.043	744
Civilians killed	0.166	0.044	763
Police killed	0.114	0.030	523

A unit of observation is a district in a given month and year (January 2005–March 2008). "Red Corridor" districts are Maoist-affected districts. "Affected Persons" indicates the number of persons killed, injured, abducted or captured. "Major Incidents" indicates the number of Major Incidents as coded by the SATP website. "Total Incidents" is the number of total Maoist-related incidents.

Table 2
Baseline pre-treatment results.

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Linear	0.516	0.0664	0.0142	−0.0231
	(0.781)	(0.317)	(0.0319)	(0.110)
R-squared	0.002	0.002	0.005	0.000
Linear flexible slope	0.0787	−0.146	−0.00853	−0.0900
	(0.535)	(0.277)	(0.0271)	(0.120)
R-squared	0.001	0.000	0.000	0.000
Quadratic	0.199	−0.162	−0.0143	−0.108
	(0.758)	(0.349)	(0.0345)	(0.138)
R-squared	0.002	0.000	0.000	0.000
Outcome mean	0.580	0.263	0.035	0.170

Unit of observation is district-month. Regressions contain 2964 observations in 228 district-clusters (Phase 1 and Phase 2 districts) in the 13 months prior to the treatment. Controls include baseline averages of each dependent variable and police force changes. Reported coefficients come from a two-staged least squares regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank). "Affected Persons" indicates the number of persons killed, injured, abducted or captured. "Major Incidents" indicates the number of Major Incidents as coded by the SATP website. "Total Incidents" is the number of total Maoist-related incidents.

We also collect data on the police force from the Indian Ministry of Home Affairs, which contains state-level information on the number of police officers, police posts and stations, as well as some other measures of police strength. District-level data on other types of crimes also come from the Ministry of Home Affairs.

4.3. Empirical specification

The state-specific district ranks of the algorithm can be used as a running variable in an RD framework.[27] Ideally, we would restrict the data to observations in the close neighborhood of the cutoff and estimate the treatment effect using local linear regressions. As the number of observations near the cutoff is limited, however, we are using all available relevant observations: We drop Phase 3 districts in our analysis of the first phase of NREGS, and drop Phase 1 when analyzing the Phase 2 cutoff. This larger bandwidth will improve the precision of the estimates due to an increased sample size, but potentially introduces bias since observations far away from the cutoff can influence the estimates (Lee and Lemieux, 2010).

We address this concern in three ways that are often used in the parametric RD literature: first, all result tables show the estimated coefficients for three different parametric specifications (linear, linear with slope of regression line allowed to differ on both sides of the cutoff, quadratic). The quadratic flexible specification is always outperformed statistically by the linear flexible specification, and using F-tests we cannot reject the null hypothesis that other higher-order polynomial terms are irrelevant.[28] 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 varying the bandwidth and restricting the sample to observations closer to the cutoff. Third, Figs. 4a–f show the non-parametric relationships between the main outcome variables and also plot linear and quadratic polynomial regression curves. Similar to the summary statistics, they show that insurgency-related violence intensity is low in many districts. We therefore also test the robustness of our results to using a zero-inflated Poisson model.

Due to the fuzzy RD, we use a two-stage least squares specification where actual NREGS receipt is instrumented with predicted NREGS treatment according to our algorithm, although intent-to-treat effects are also reported in the appendix. We run results separately by cutoff. Most of our empirical analysis focuses on the Phase 1 cutoff, since baseline Maoist violence levels in districts near the Phase 2 cutoff are much lower. The bulk of the effect of NREGS on violence should therefore occur in early treatment districts.

We run the regression equation below where $f(.)$ is a function of actual NREGS receipt *nregs* (instrumented with predicted NREGS receipt) and the district's rank based on the state-specific normalized index *rank*. To increase the precision of our estimates, we control for the baseline outcome variable y_{iO}.[29] We show results for linear, linear with flexible slopes and quadratic functions of $f(.)$:

$$y_{ij} = \beta_0 + \beta_1 nregs_i + f(rank, nregs) + \beta_2 y_{i0} + \epsilon_{ij}$$

y_{ij} is an outcome variable in district i and month j, and the coefficient of interest is β_1. Standard errors are clustered at the district level.

5. Results

5.1. Main results

The main results are presented in Tables 3–5. Table 3 shows the impact of NREGS on Maoist incidents for the four main outcome variables in the first year after the program was introduced: individuals affected (deaths/injuries/abductions); deaths; major incidents; and total incidents. Panel B normalizes the variables by the 2001 Census population counts, showing the results per-10 million people. Each panel has three different specifications: linear, linear with a flexible slope, and quadratic.[30]

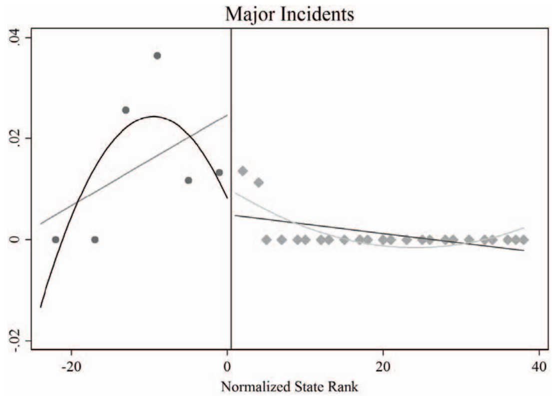
Panel A of Table 3 shows that violence increases in Phase 1 districts after NREGS is introduced. Depending on the specification, there is a rise of about 0.55–0.75 deaths per month in a given district. At a mean of about 0.44 deaths per month in a Red Corridor district, this amounts to about a 125% increase from the baseline level. Similarly, the number of affected persons increases by about 0.56–0.73 units per district-month, which amounts to a rise of 56%. The number of total incidents

[27] Our results are also robust to using the poverty index values as the running variable.

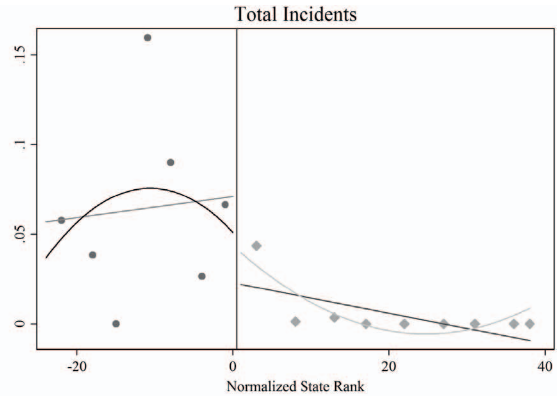
[28] More flexible models also tend to be unstable in the second stage of the two-stage least squares estimation, although the coefficients are often qualitatively similar to the quadratic results. Gelman (2014) discourages the use of higher-order polynomials.

[29] The results are robust to controlling for month and year fixed effects, or month by year fixed effects.

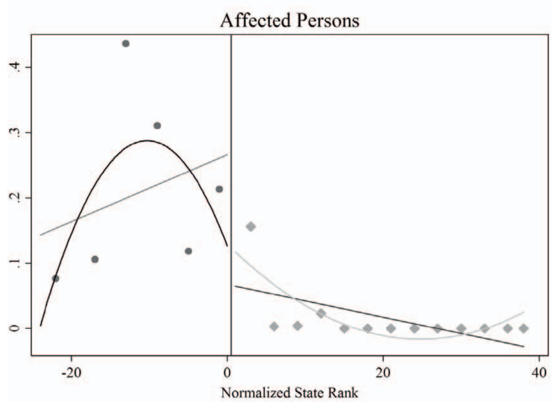
[30] Specifications control for (estimated) police force changes, but the results are robust to excluding these controls. They are presented in Panel B of Appendix Table A8.



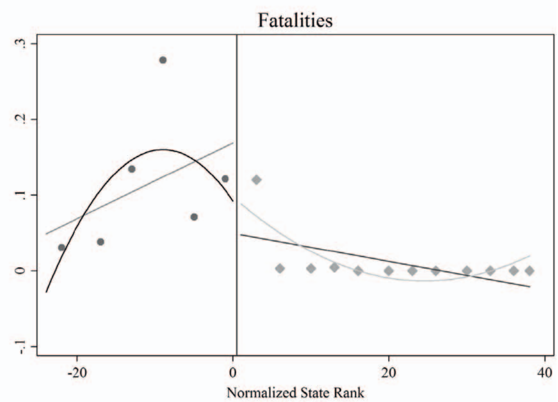
(a) Total Number of Major Incidents



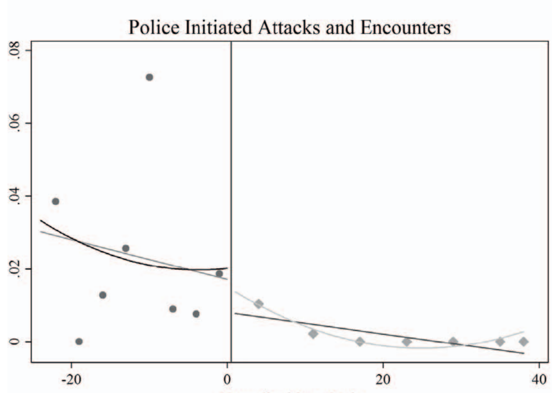
(b) Total Number of Incidents



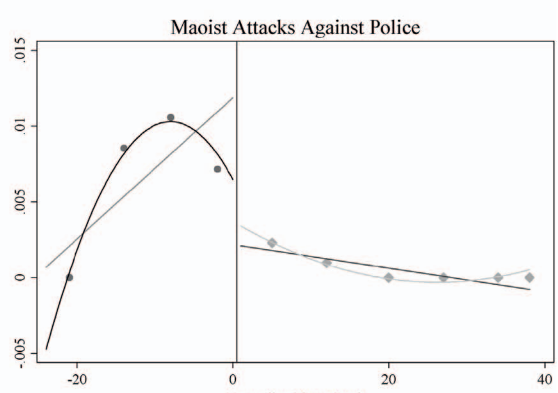
(c) Persons Killed, Injured, Abducted or Captured



(d) Total Number of Persons Killed



(e) Police-initiated Attacks

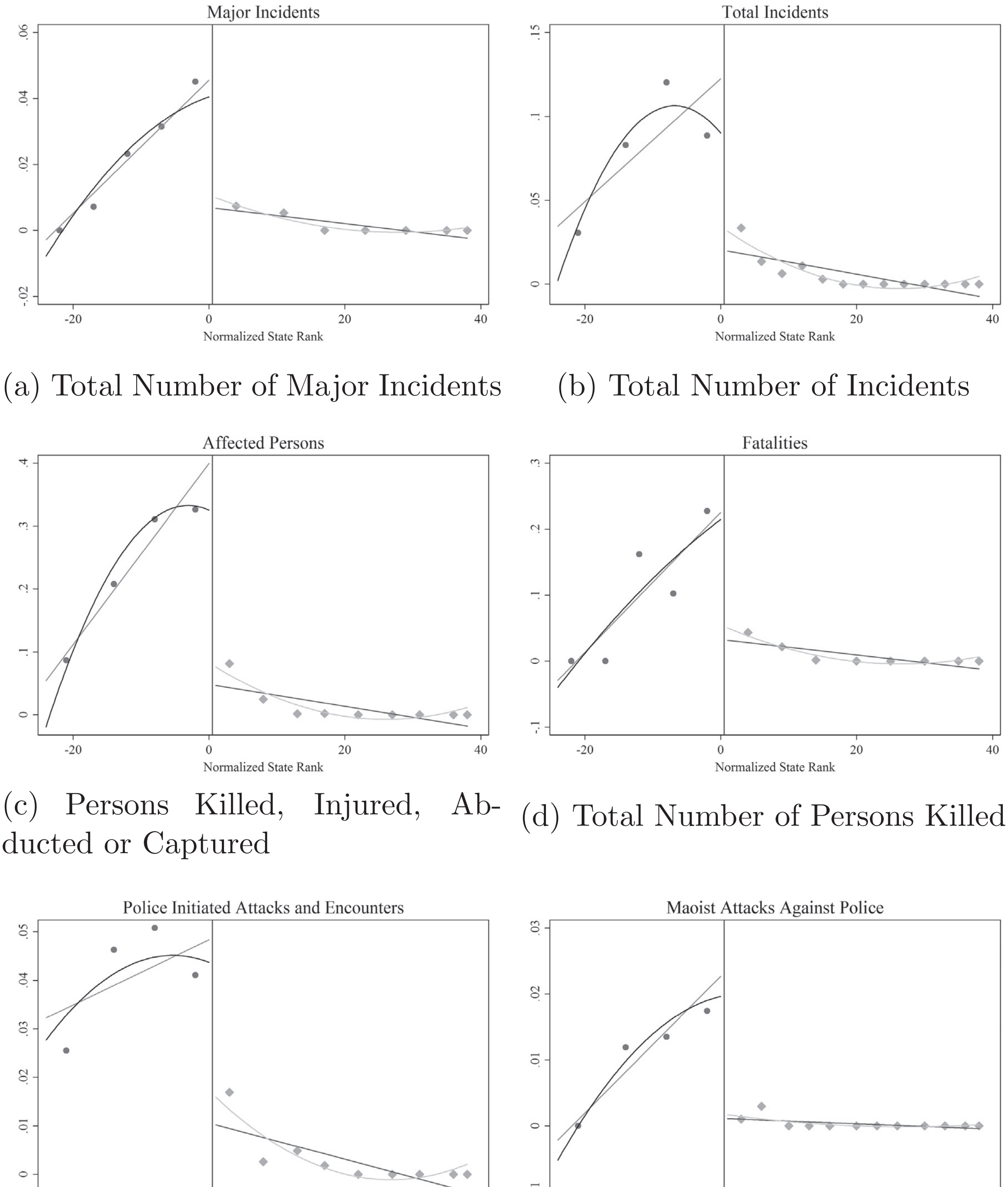


(f) Maoist Attacks Against Police

Fig. 3. Pre-treatment discontinuities for main variables. *Note*: The graphs use the optimal quantile-spaced binning procedure suggested by Calonico et al. (2015). Two (out of 5811) observations are dropped for being gross outliers in the base. Linear and quadratic polynomials are fitted through the underlying data and not just the bins. Unit of observation is a district-month, with 228 districts across a 14-month period post Phase 1 implementation and pre Phase 2 implementation.

rises by about 0.22–0.27 incidents per month, or about 70%. These results are robust across the different parametric specifications. A crude calculation suggests that these effects translate into between 785 and 1071 more fatalities in about 314–385 more incidents in the year after implementation. Panel B shows similar impacts once we normalize the variables by population.

Figs. 4a–f use linear and quadratic polynomials and the optimal quantile-spaced binning procedure suggested by Calonico et al. (2015) to plot the primary variables against the rank variable, and show a significant discontinuity at the cutoff for all the variables of interest. Fig. 5 plots the monthly RD coefficients for the number of incidents, where the first vertical line depicts the time when the employment guarantee act was passed, and the second vertical line marks when Phase 1 was implemented. The figure shows that, across the different specifications, the increase in violence is almost immediate after NREGS introduction. Similarly, Fig. 6, which plots the monthly RD coefficients for the number of persons affected, shows that while there is an immediate increase in violence, there is also a slight dissipation of



(a) Total Number of Major Incidents (b) Total Number of Incidents

(c) Persons Killed, Injured, Abducted or Captured (d) Total Number of Persons Killed

(e) Police-initiated Attacks (f) Maoist Attacks Against Police

Fig. 4. Discontinuities for main variables. *Note*: The graphs use the optimal quantile-spaced binning procedure suggested by Calonico et al. (2015). Linear and quadratic polynomials are fitted through the underlying data and not just the bins. Unit of observation is a district-month, with 228 districts across a 14-month period post Phase 1 implementation and pre Phase 2 implementation. See Appendix for a version of this graph without outliers.

effects over time. The figures therefore suggest that violence increases almost immediately after program introduction rather than slowly over time.

The data allows us to distinguish between civilians, Maoists and the police force. In many cases, it is also possible to code up which conflict party initiated the attack. Table 4 reports the empirical results of this analysis, focusing again on Phase 1 districts. Panel A shows that an important part of the increase in violence comes from police-initiated attacks on the Maoists. This is consistent across specifications. The results also show the Maoists retaliating against civilians, but not a very large increase in Maoist-on-police attacks.

Panel B of Table 4 presents the RD results for fatalities by group. Civilian and police casualty estimates are small and imprecisely estimated, whereas Naxal casualties increase by between 0.3 and 0.4 deaths a month after the introduction of the NREGS, an effect that is also statistically significant at the 5% level. Appendix Table A6 presents

Table 3
Main results.

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Panel A: non-normalized				
Linear	0.636**	0.594**	0.104***	0.272**
	(0.294)	(0.275)	(0.0402)	(0.112)
R-squared	0.487	0.438	0.380	0.457
Linear flexible slope	0.561*	0.548*	0.0843**	0.228**
	(0.300)	(0.301)	(0.0376)	(0.105)
R-squared	0.487	0.439	0.386	0.466
Quadratic	0.723**	0.746**	0.124***	0.274**
	(0.351)	(0.352)	(0.0475)	(0.128)
R-squared	0.487	0.434	0.375	0.456
Outcome mean	0.580	0.263	0.035	0.170
Panel B: per capita				
Linear	2.388**	1.845*	0.537**	1.182**
	(1.133)	(1.044)	(0.240)	(0.538)
R-squared	0.525	0.521	0.561	0.682
Linear flexible slope	1.906*	1.536	0.391**	0.959*
	(1.047)	(1.113)	(0.187)	(0.541)
R-squared	0.525	0.521	0.562	0.684
Quadratic	2.278*	2.388*	0.601**	1.060*
	(1.209)	(1.275)	(0.246)	(0.607)
R-squared	0.525	0.521	0.560	0.683
Outcome mean	6.577	3.012	0.360	1.748

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. Regressions contain 3192 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. Reported coefficients come from a two-staged least squares regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank). "Affected Persons" indicates the number of persons killed, injured, abducted or captured. "Major Incidents" indicates the number of Major Incidents as coded by the SATP website. "Total Incidents" is the number of total Maoist-related incidents.

Table 4
Who initiates the attacks and who is killed.

Specification	Panel A: who initiates		
	Police on police	Maoist on police	Maoist on civilians
Linear	0.110*	0.0250**	0.0945*
	(0.0610)	(0.0121)	(0.0496)
R-squared	0.133	0.180	0.350
Linear flexible slope	0.0889*	0.0218**	0.0626
	(0.0470)	(0.0111)	(0.0394)
R-squared	0.140	0.181	0.357
Quadratic	0.100*	0.0252*	0.0898*
	(0.0607)	(0.0135)	(0.0528)
R-squared	0.136	0.180	0.351
Outcome mean	0.057	0.028	0.071
Specification	**Panel B: who is killed**		
	Civilians killed	Police killed	Maoists killed
Linear	0.146	0.0456	0.356**
	(0.119)	(0.0677)	(0.176)
R-squared	0.337	0.176	0.204
Linear flexible slope	0.132	0.0339	0.306**
	(0.120)	(0.0508)	(0.152)
R-squared	0.337	0.176	0.211
Quadratic	0.168	0.0980	0.394**
	(0.140)	(0.0749)	(0.200)
R-squared	0.337	0.175	0.200
Outcome mean	0.095	0.079	0.089

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Panel A shows the results for 'who initiates the attacks and against whom.' Panel B reports the results for 'who is killed'. Controls include baseline averages of each dependent variable and police force changes. Regressions contain 3192 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. Reported coefficients come from a two-staged least squares regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank).

the per-capita results, which again show that the Maoist deaths contribute to most of the new casualties. The police force does not experience a statistically significant increase in fatalities, and the magnitudes are also much smaller.

Focusing on the dynamic patterns of Phase 1, Table 5 divides the post-treatment period into the short run (Panel A) and the medium run (Panel B). There are 14 months before Phase 2 receives NREGS, so we divide them equally into the short run (first 7 months after NREGS eligibility) and the medium run (months 8 through 14). The results show that an important part of the impact occurs in the short run. The impact on the number of affected persons is somewhere between 1.6 and 2.2 times higher, and fatalities are 1.4 to 1.6 times higher in the short run than in the medium run.

5.2. Discussion

Overall, the results therefore show that violence increases after the introduction of NREGS, with the bulk of the effect being concentrated in the first couple of months. Naxals are the most affected group, and there is an increase in police-initiated attacks as well as in insurgents-on-civilian violence. These patterns let us distinguish between the theories of how NREGS should affect insurgency-related violence that were discussed in detail in Sections 2 and 3.

Two established theories in the literature predict that violence should decrease after the introduction of the anti-poverty program. Since NREGS provides new employment opportunities, it raises the opportunity cost of being a Maoist supporter, making it harder to recruit and retain them and leading to a decrease in violence. The second theory is a 'hearts and minds' explanation that suggests that the introduction of NREGS should improve the relationship between civilians and the government, making civilians more likely to provide information to the police. This will lower violence as the rebels are starting to lose the fight. Both these explanations are not consistent with the immediate increase in violence that we find empirically. This is not entirely surprising since existing research on the economic impacts of NREGS as well as qualitative evidence suggest that take-up rates of the program were low in the first couple of months and heavily affected by implementation challenges.[31] For the time period that we are studying, opportunity costs are therefore likely to have been low, whereas a reduction in violence via the 'hearts and minds story mostly applies to the long run once the insurgency has been weakened. Our results therefore do not rule out that both channels play a role in the longer run. This distinction between the short and the long run makes

[31] See Background section for a number of citations on the topic and a more detailed discussion.

Table 5
The Short Run and the Medium Run.

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Panel A: Short Run				
Linear	0.778**	0.641**	0.123**	0.273*
	(0.383)	(0.322)	(0.0552)	(0.142)
R-squared	0.655	0.599	0.510	0.577
Linear flexible slope	0.760*	0.671*	0.113*	0.251*
	(0.420)	(0.364)	(0.0585)	(0.139)
R-squared	0.655	0.598	0.513	0.581
Quadratic	0.967**	0.855**	0.162**	0.311*
	(0.488)	(0.425)	(0.0712)	(0.170)
R-squared	0.654	0.595	0.501	0.571
Panel B: Medium Run				
Linear	0.484*	0.458**	0.0855**	0.273***
	(0.273)	(0.222)	(0.0371)	(0.106)
R-squared	0.359	0.360	0.273	0.370
Linear flexible slope	0.396*	0.397*	0.0639**	0.224**
	(0.233)	(0.223)	(0.0285)	(0.0955)
R-squared	0.359	0.361	0.279	0.381
Quadratic	0.445	0.520**	0.0862**	0.240**
	(0.281)	(0.259)	(0.0361)	(0.108)
R-squared	0.359	0.358	0.273	0.376
Outcome mean	0.580	0.263	0.035	0.170

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Panel A shows Short Run impacts (months 1 through 7 post NREGS). Panel B shows the Medium Run (months 8 through 14).Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. Regressions contain 1596 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 7 month periods. “Affected Persons” is the number of persons killed, injured, abducted or captured. “Fatalities” is total number of deaths. “Major Incidents” is the number of ‘Major Incidents’ as coded by the SATP website. “Total Incidents” is the number of total Maoist-related incidents. Coefficients come from 2SLS regression, where actual NREGS treatment is instrumented with predicted treatment based on the algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable.

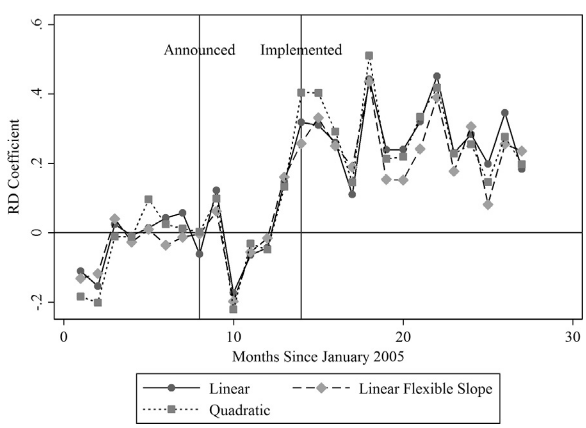


Fig. 5. Monthly RD coefficients – total number of incidents.

our paper consistent with Fetzer (2014), who finds a decrease in violence in Maoist-affected NREGS districts, which is attributed to NREGS dampening the effect of poor rainfall lowering the opportunity cost of violence. While our paper focuses on the initial months of program implementation, the results in Fetzer (2014) can be thought of as more long-run results once the system has achieved a new equilibrium.

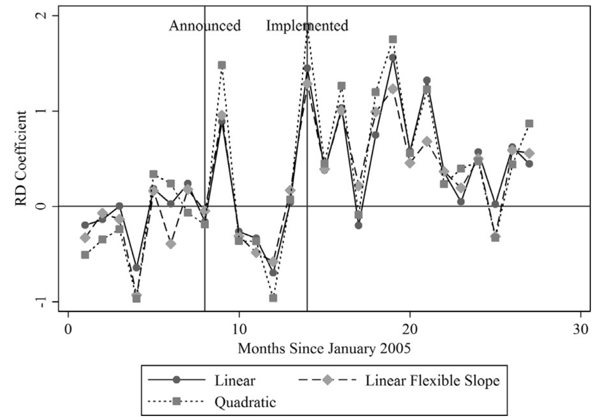


Fig. 6. Monthly RD coefficients – number of persons affected. *Note*: Coefficients of month-by-month RD regressions of number of incidents and number of persons affected. The first vertical line indicates the passage of the Act in Parliament, the second vertical line indicates the first month of implementation in Phase 1. Our analysis ends 14 months after program implementation when implementation started in Phase 2 districts. Each point is the coefficient for a different regression restricting the sample to the corresponding month.

Among the theories that predict a violence increase, the almost immediate rise in violence after the introduction of NREGS makes the straightforward competition for resources explanation and a sabotage story less plausible. A common version of the competition for resources theory relies on both governments and Maoists increasing attacks in treatment districts in an attempt to control the assets created by NREGS. Both channels imply that violence should increase more strongly over time once assets can be appropriated or more projects can be sabotaged, which is the opposite of the empirical patterns we find. Additionally, implementation delays and the fact that most projects in practice do not generate appropriable assets provide qualitative evidence against such explanations.[32] In our dataset, we also have no instances of insurgent attacks taking place at NREGS worksites.

The spotlight theory predicts a more active police force in treatment districts due to increased media attention, which is consistent with the rise in police-initiated attacks that we find. We would not necessarily expect Maoists to retaliate against civilians, however, and may instead expect to find an increase in Maoist attacks on the police, which is not what we find. In the appendix, we provide a further test of the plausibility of the spotlight theory: If police officers feel an increased pressure to perform better in treatment areas because of increased attention paid to NREGS districts, then we may expect that this should also apply to other crimes. Appendix Table A1 provides no evidence of NREGS having a statistically significant impact on crime, however, and the magnitudes tend to be small. This reduces the plausibility of a spotlight explanation.

The results overall suggest that two explanations are the most plausible: First, a competition for resources explanation that focuses on the appropriation of expected future monetary inflows rather than created assets, since such an expectation could make current territorial control of treatment districts more desirable. And second, the alternative citizen-support channel, which claims that police forces become more effective due to increased information given to the police as a result of a better relationship between civilians and the government. Both channels predict that the police will become more active, which is consistent with the rise in police-initiated attacks that we find. Under the citizen-support channel, retaliatory responses against civilians, for which there is also some empirical evidence, are also plausible since the insurgents may want to retaliate against civilians for helping the police. The empirical results therefore fit the citizen-support channel very well,

[32] See Background section for further details.

including the fact that Naxals and civilians are the most affected groups in terms of injuries and casualties. There is much less reason to expect retaliation against civilians under the competition for resources explanations, and the competition for resources story may lead us to expect a large increase in the number of Maoist attacks on the police as they are battling for territorial control, which is not what we find. But with further refinements this channel is not mutually excludable from the citizen-support channel and can be made to fit the results, so we cannot rule out this alternative explanation entirely. A couple of extensions to the main results can be used to further probe whether the citizen support channel is plausible in this context, however.

5.3. Extensions

If the citizen-support channel is relevant, we should expect it to be especially important in areas where program awareness and implementation quality are higher, although this may also be driven by the expectation of higher monetary flows that can be consistent with a competition for resources explanation. We should therefore expect the number of police-initiated attacks to be higher in these areas than in the rest of the country. One measure of implementation quality often used in the existing NREGS literature are the so-called 'star states' where, based on field reports, awareness of the program and implementation quality tend to be much higher than in the rest of the country (Dreze and Khera, 2009; Khera, 2011). In Table 6, the NREGS treatment variable is interacted with an indicator variable equal to one if a state is a 'star state', and zero otherwise. As the table shows, police-initiated attacks are indeed higher in star-state NREGS districts than in other treatment districts.

Additionally, the citizen-support channel implies that we should expect the increase in violence to be concentrated in states with high police capacity rather than in areas with low police effectiveness. Appendix Table A3 splits the sample into states with high and low police effectiveness and shows that the results are indeed driven by states with high police capacity.

Given that violence levels increase almost immediately after the introduction of NREGS despite severe challenges with implementation in practice, an implication of this is that civilians may be willing to cooperate with the police now in anticipation of future program benefits, either because they believe that they will actually receive the benefits soon, or because they believe that a successful implementation of NREGS is conditional on civilian cooperation and/or on better state control at the local level.[33] Zimmermann (2015) finds results consistent with NREGS having such an effect at the time of the Indian general elections in 2009, where the districts with shortest exposure to the program were more likely to vote for the government and were less sensitive to implementation quality than areas with longer access to the program.[34]

If the promise of development is important, then we may find that civilians also change their behavior even in still untreated districts. This effect may occur especially in Phase 2 districts 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 their districts receiving the treatment soon. We would then expect to find positive spillover effects of the program onto Phase 2 districts.

Appendix Table A4 confirms that this effect does indeed hold empirically, although the magnitudes are much smaller than for the main results. At the time that Phase 1 districts have access to NREGS (and other phases do not) there is an increase in violence in Phase 2 districts (Panel A). The magnitude of the Phase 2 estimates is about one fifth of that for the Phase 1 estimates for the affected measure, about one-eighth for the number of incidents, and insignificant for major incidents. This violence increase dissipates over time, and by the time Phase 2 is in the spotlight, there is no longer any impact (Panel B).[35]

Overall, all of these empirical patterns are consistent with the predictions of a citizen-support model in which civilians are willing to share information and other forms of support with the police after NREGS implementation starts, which allows government troops to crack down more efficiently on the Maoists. They are difficult to explain with many alternative explanations, however, although we cannot completely rule out that a variant of the competition for resources explanation as an alternative explanation.

To further support our explanations in this paper, the Online Appendix contains detailed circumstantial evidence, institutional background information and qualitative evidence needed for a better understanding of the plausibility of the citizen-support channel in this context. The citizen-support channel assumes that citizens were aware of NREGS and the promised benefits, for example, since otherwise they

Table 6
Who initiates the attacks: Star States vs. Non-Star States.

Specification	Police on maoist	Maoist on police	Maoist on civilians
Linear:			
NREGS	0.0609	0.0294**	0.0735*
	(0.0380)	(0.0141)	(0.0390)
NREGS*Star States	0.116*	−0.0130	0.0593
	(0.0685)	(0.00899)	(0.0560)
Star States	−0.0136	0.00688*	−0.0305
	(0.0170)	(0.00380)	(0.0269)
R-squared	0.136	0.181	0.351
Linear flexible slope:			
NREGS	0.0257	0.0228*	0.0244
	(0.0217)	(0.0127)	(0.0200)
NREGS*Star States	0.363**	0.0312	0.190
	(0.174)	(0.0328)	(0.126)
Star States	−0.0222	0.00597	−0.0372
	(0.0193)	(0.00376)	(0.0279)
R-squared	0.153	0.185	0.362
Quadratic:			
NREGS	0.0592	0.0298**	0.0681*
	(0.0398)	(0.0151)	(0.0401)
NREGS*Star States	0.116*	−0.0130	0.0602
	(0.0675)	(0.00870)	(0.0548)
Star States	−0.0139	0.00695	−0.0314
	(0.0166)	(0.00429)	(0.0260)
R-squared	0.137	0.181	0.352
Outcome mean	0.057	0.028	0.071

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Star States include Andhra Pradesh, Chhattisgarh, Tamil Nadu, Rajasthan and Madhya Pradesh, which according to field reports have a higher implementation quality of the NREGS than other states (Dreze and Khera, 2009; Khera, 2011). Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. Regressions contain 3192 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. Coefficients come from a 2SLS regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank).

[33] See Online Appendix for a more detailed discussion of qualitative evidence on why this is plausible in the context on NREGS and on why civilians may want to cooperate with the police.

[34] A number of researchers believe that NREGS was important in ensuring the re-election of the Indian government (see Zimmermann, 2015 for details). Electoral benefits from government programs have also been found in other contexts (see e.g. De la, 2013; Manacorda et al., 2011).

[35] The Phase 2 results are difficult to explain with the spotlight theory. If the police or media work harder in treatment areas due to increased attention on law and order in NREGS areas, there is no reason for the police or newspaper reporters in still untreated areas to increase their effort levels.

have no reason to change their behavior. NREGS was a program that NGOs and social activists had campaigned for a while before it was enacted, and the pressure on the government to implement the program was therefore higher than for previous schemes. Different levels of government as well as various social organizations had widespread traditional and non-traditional advertisement campaigns for NREGS, which included newspapers, radio shows and theater performances. Both the government and the NGOs also highlighted that NREGS would be a different type of program from previous failed initiatives due to its legal character, the increased scope of the program, and more extensive NGO monitoring on the ground. In the Online Appendix, we provide specific examples of how the behavior of governments, social activists and NGOs in the months between the passing of NREGS in Parliament and the official implementation start date is likely to have led to higher program awareness than for previous programs, and why citizens may have plausibly expected much larger benefits from NREGS than from previous failed government initiatives. We also go into more detail on the idea of why experienced and anticipated future benefits from NREGS could be expected to induce civilians to cooperate with the police.

5.4. Robustness checks

In order to ensure that a handful of large attacks by Maoists or police are not driving the results, we repeat the analysis by dropping all district-months wherein more than twenty persons were killed or injured.[36] The results of this analysis are shown in Appendix Table A5 and Fig. A2.

Another 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 donut-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 Panel A of Appendix Table A7. They are similar, both 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. Panel B of Table A7 presents the main results when varying the bandwidth by restricting the analysis to observations closer to the cutoff, and once again produces similar results.

Our main results are also robust to a number of other specifications presented in the appendix: Panel A of Table A8 estimates the intent-to-treat (ITT) version of the main results, while Panel B of Table A8 reproduces the main results without controlling for the strength of the police force. Both specifications consistently maintain the main results.[37]

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. Panel A of Appendix Table A9 therefore presents 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 casualties 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. The results are also robust to using other count data models like the hurdle model using a Logit-Poisson specification.

Panel B of Table A9 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. We conduct two different types of DID exercises: the Intent-to-Treat (ITT) version, where treatment is assigned based on who should have received NREGS according to the algorithm; and the Actual Treatment version where treatment depends on actually receiving NREGS. While the DID approach estimates the overall average treatment effect on the treated and therefore a different parameter than the RD specifications, the results are again qualitatively similar.

Lastly, we conduct other robustness checks not reported here by re-doing our main results after controlling for rainfall shocks in the current month and in the entire preceding year. We also control for average monthly wages, and find that our results are robust to all these specifications. To the extent that rainfall shocks and wages capture what happens to income in these regions after NREGS is introduced, these results indicate that the opportunity-cost channel is not the driving force behind these results. In other specifications, we also control for the timing of the state elections, in some specifications using not just the election month but also up to 5-months leading up to an election, and our results are unaffected by these controls.[38]

In the Online Appendix, we discuss a number of other concerns and robustness checks. We explain in more detail why the fuzziness of the RD is not a concern for internal validity and what a plausible explanation is for the deviations from the algorithm in practice. An extensive placebo analysis shows that the results we find only occur around the true cutoff, but not at artificially created cutoffs elsewhere in the distribution. We also conduct baseline balance tests across a number of geographic, demographic and political variables and show the results of a bounding exercise. These tests rule out that our results in the paper overstate the expected results from NREGS implementation due to a correlation of systematic characteristics with algorithm non-compliance.

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 Maoist violence. We exploit the fact that the program was phased in over time according to an algorithm that prioritized economically underdeveloped districts in a regression-discontinuity (RD) design. The results are robust across a number of different specifications and show a substantial rise in violence in the first year of implementation in the districts that received NREGS, especially in the very short run. Insurgents are the primary affected group as police-initiated attacks rise, with little impact on police force casualties. There is also some evidence for an increase in Maoist-initiated attacks against civilians. The impact is largest among districts that received NREGS in the first phase of the roll-out, but there are small positive spillovers of violence to the districts that are next in line to receive the program.

These empirical patterns as well as other available qualitative and quantitative evidence on the conflict are consistent with a model in which the government program makes civilians more willing to support the police because it improves the relationship between the government and the people. In contrast, the results are difficult to explain

[36] This drops the most violent eleven district-months from the districts that received NREGS. The results are robust to picking other cutoffs.

[37] One possible simultaneous change with NREGS implementation is an increase in the size of the police force. Since we do not have data on the actual police force in a district, we estimate it using state-level information, where any change in the police force for a given state is assumed to be attributable to NREGS districts. 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. Our main analysis therefore includes police force controls, although, as Panel B of Table A8 shows, the results are very similar without these controls. Other tables are also robust to dropping police controls.

[38] In the Online Appendix, we also show that our results are robust to controlling for additional baseline variables and to restricting the sample to just conflict regions.

with a number of alternative theories, although we cannot completely rule out that a competition for resources explanation fits the results. Securing the assistance of the local population may therefore be an important factor in internal conflicts and may help answer the Weber (1919) question on whether the State should use force *or* development to tackle internal conflict. 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.

It is unclear, however, how successful such strategies are likely to be in the longer run. In the Indian context, a growing literature questions the effectiveness of NREGS as a tool for *actual* development due to various implementation problems, although the program still seems to provide some benefits through its safety net function. This implies that at least a part of what may win over the local community initially are anticipated welfare benefits and the *promise* of development rather than actual changes. Such a mere promise may not be credible enough to ensure the support received from the civilian population over time, however. Once civilians realize that the program is not delivering, 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, an important component to winning continued civilian support may be to ensure that government anti-poverty programs are implemented effectively and actually fulfill the promise of development.

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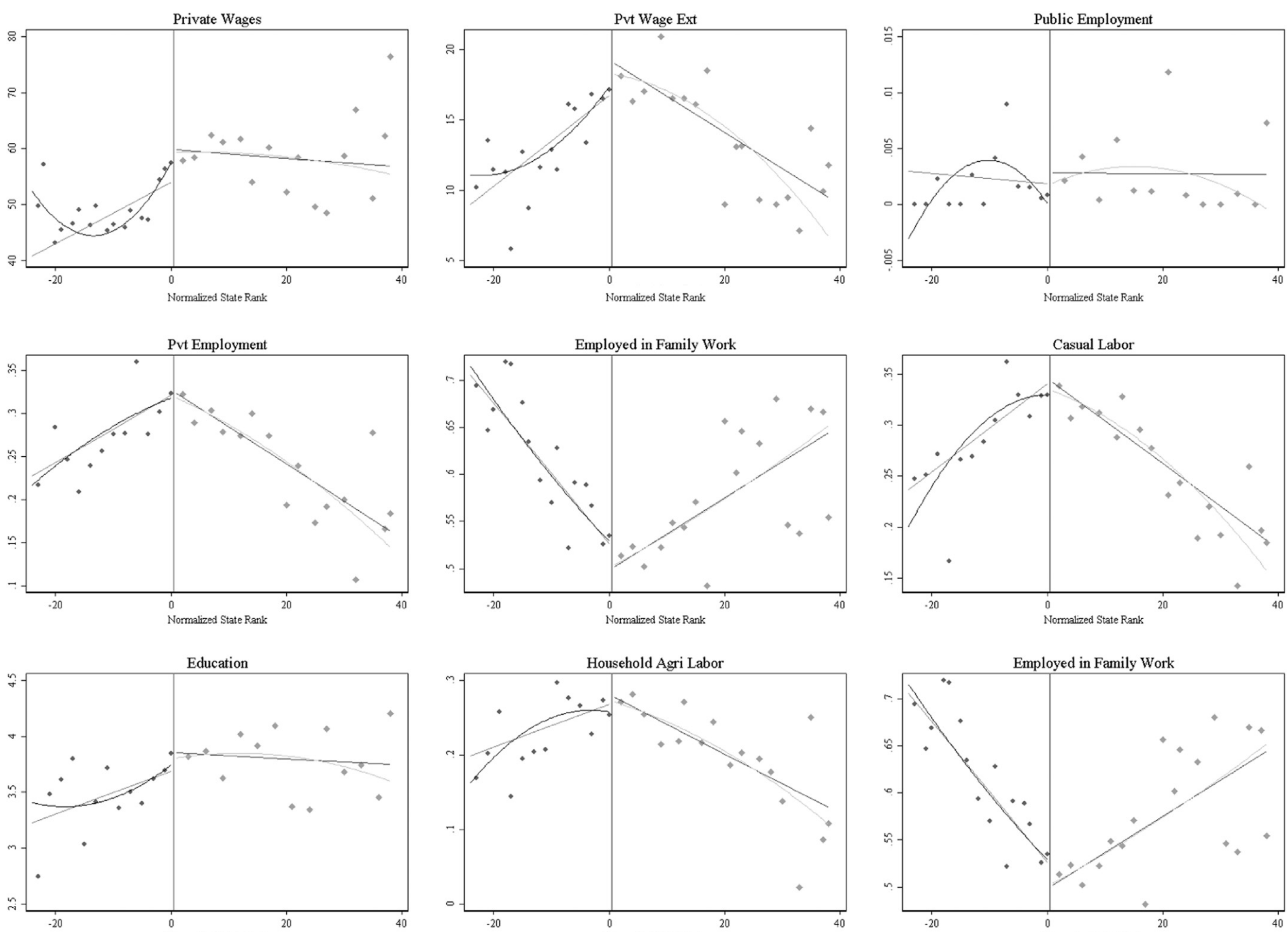
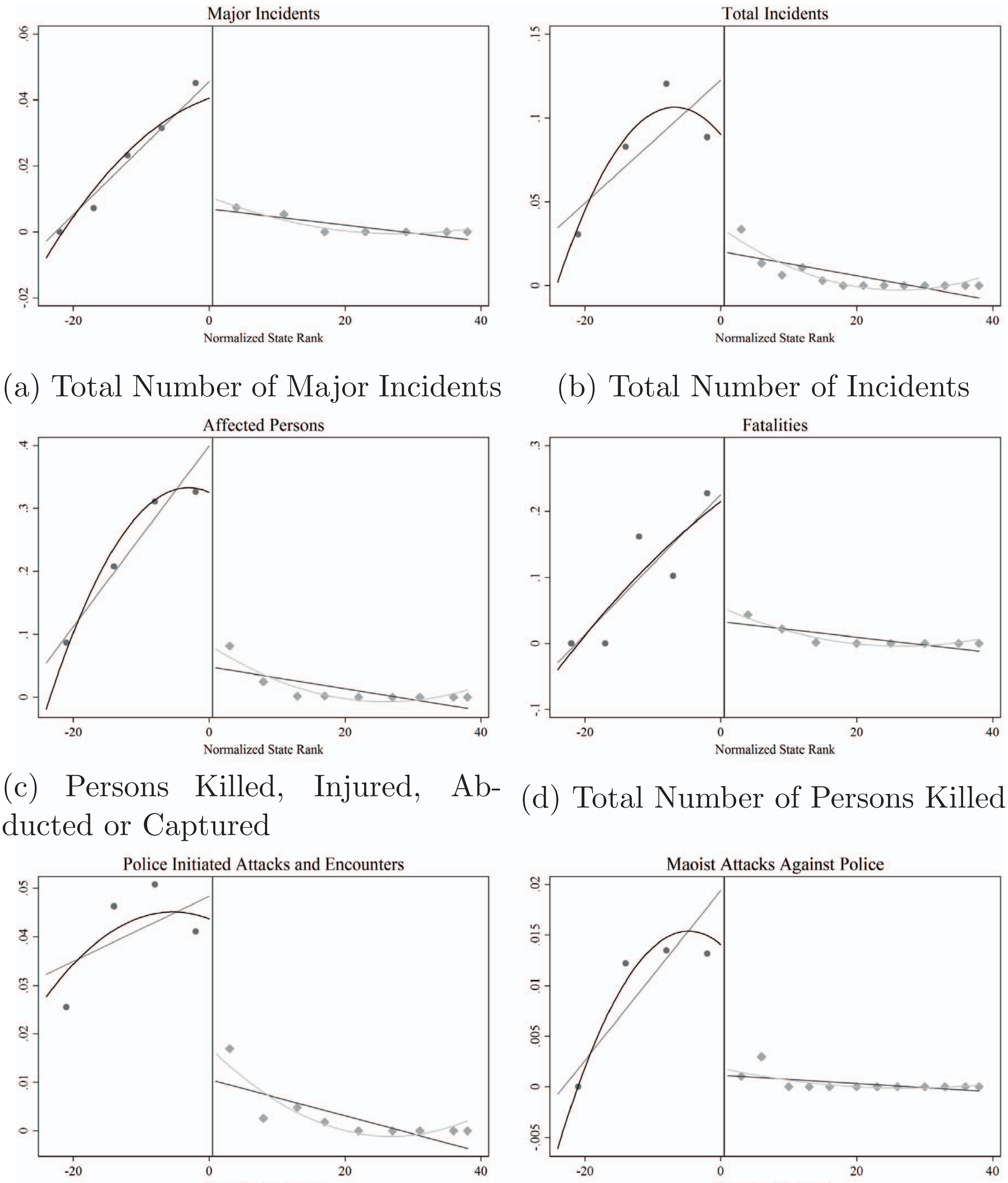


Fig. A1. Discontinuities for non-outcome variables at baseline. *Data source*: National Sample Survey of India (2004–2005) – employment and unemployment module. Dependent variables: district-level averages in 2004/2005 for male, working age workers (18–60 years) in rural areas. HH agri labor is proportion of households engaged in agricultural labor, education is years of schooling, pvt wage is private daily casual wage in past 7 days in rupees, pvt wage ext is the private daily casual wage for everyone with a positive wage and 0 for everyone with a missing wage, pvt emp, public emp, and emp in family work are the proportion of workers working in public, private casual, and family employment during last week.



(a) Total Number of Major Incidents (b) Total Number of Incidents

(c) Persons Killed, Injured, Abducted or Captured (d) Total Number of Persons Killed

(e) Police-initiated Attacks (f) Maoist Attacks Against Police

Fig. A2. Without outliers: main results. *Note*: The graphs use the optimal quantile-spaced binning procedure suggested by Calonico et al. (2015), and drop outliers. Linear and quadratic polynomials are fitted through the underlying data and not just the bins. Unit of observation is a district-month, with 228 districts across a 14-month period post Phase 1 implementation and pre Phase 2 implementation.

Appendix A. Additional tables and figures

See Figs. A1 and A2 and Tables A1–A9.

Appendix B. A citizen-support model

We set up a theoretical model that incorporates the importance of citizens assisting the government by sharing information, although in practice this can also include other forms of assistance. While there are some models that stress the importance of citizen support, such as the models in Berman et al. (2011b) on counterinsurgency in Iraq and the Akerlof and Yellen (1994) study on street-gangs, our model differs from those in a number of respects that fit our context better. First, we allow insurgents to fight for territory, whereas the rebels' goal in the Berman et al. (2011b)

Table A1
Other types of crime and violence: Phase 1.

Specification	Total crimes	Murder	Kidnapping	Theft	Burglary	Riots
Linear	228.7	1.534	5.545	13.13	2.489	−17.19
	(160.4)	(6.265)	(6.812)	(34.05)	(15.12)	(20.13)
R-squared	0.973	0.878	0.834	0.959	0.955	0.860
Linear flexible slope	178.4	1.955	4.994	20.38	−2.948	−8.759
	(170.5)	(5.842)	(6.935)	(35.72)	(15.35)	(17.78)
R-squared	0.973	0.878	0.835	0.959	0.955	0.863
Quadratic	228.0	5.317	5.007	27.22	4.193	−13.60
	(197.0)	(6.918)	(8.038)	(42.37)	(18.44)	(21.68)
R-squared	0.973	0.876	0.835	0.958	0.955	0.861
Outcome mean	2768.17	57.23	38.10	325.04	122.18	105.36

Regressions contains 225 observations, where the unit of observation is a district. Reported coefficients come from a two-staged least squares regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank). Data source: Home Ministry of India.

Table A2
Education, employment and wages at baseline.

	HH agri labor	HH self-emp in agri	Pvt wage	Pvt wage ext
Linear	−0.102	−0.00495	9.571	1.864
	(0.0775)	(0.0748)	(8.120)	(3.903)
R-squared	0.067	0.120	0	0.118
Linear flex slope	−0.139*	0.121	−0.260	−6.352
	(0.0803)	(0.0784)	(5.807)	(3.863)
R-squared	0.003	0.036	0.109	0.114
Quadratic	−0.142	0.0913	7.998	−2.986
R-squared	0.019	0.119	0	0.188
	Education	Pvt emp	Public emp	Emp in family work
Linear	0.351	0.0191	−0.00141	−0.0590
	(0.330)	(0.0616)	(0.00305)	(0.0668)
R-squared	0	0.116	0	0.086
Linear flex slope	0.282	−0.0939	−0.00111	0.0938
	(0.338)	(0.0654)	(0.00262)	(0.0683)
R-squared	0.022	0	0	0.056
Quadratic	0.395	−0.0622	−0.00433	0.0536
	(0.400)	(0.0764)	(0.00337)	(0.0785)
R-squared	0	0.088	0	0.151

* p < 0.10, ** p < 0.05, *** p < 0.01
Data source: National Sample Survey of India (2004–2005) – Employment and Unemployment Module. Dependent variables: district-level averages in 2004/2005 for male, working age workers (18–60 years) in rural areas. HH agri labor is proportion of households engaged in agricultural labor, HH self-emp in agri is proportion of households self-employed in agriculture, education is years of schooling, pvt wage is private daily casual wage in past 7 days in rupees, pvt wage ext is the private daily casual wage for everyone with a positive wage and 0 for everyone with a missing wage, pvt emp, public emp, and emp in family work are the proportion of workers working in public, private casual, and family employment during last week. 2SLS Regressions where treatment is instrumented with predicted treatment.

model is only to impose costs on the government. Second, in our model civilians make their information-sharing decisions before rather than after the government and the insurgents move. Lastly, we consider how aggregate violence patterns may change dynamically.

The model describes the optimal strategies in the conflict by three players, the government, the Maoists, and the civilians. In the Indian context, the employment guarantee scheme was implemented across the country and prioritized poor districts regardless of their internal security condition in the assignment algorithm. Therefore, the decision about whether, and if so, how much, to invest in anti-poverty programs like an employment guarantee scheme is taken to be exogenous.[39] There are L identical locations in the country where the government fights for territorial control with the insurgents. In each location, the probability that the government gains control of the territory $p(m, v, i)$, depends positively on the amount of governmental military action m, negatively on the amount of Maoist violence inflicted upon the police v, and positively on the amount of information that the police has i.

[39] In other contexts, a number of economic and political economy factors will enter the government's objective function in addition to anticipated internal security benefits.

Table A3
Results by police capacity.

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Panel A: high security capacity				
Linear	2.030**	2.655**	0.327**	0.923**
	(0.952)	(1.095)	(0.138)	(0.465)
R-squared	0.500	0.449	0.434	0.429
Linear flexible slope	1.933**	2.245**	0.290**	0.860**
	(0.905)	(0.989)	(0.123)	(0.411)
R-squared	0.500	0.462	0.445	0.446
Quadratic	1.976**	2.536**	0.315**	0.901**
	(0.928)	(1.040)	(0.130)	(0.446)
R-squared	0.500	0.455	0.439	0.437
Outcome mean	1.080	0.497	0.060	0.338
Panel B: low security capacity				
Linear	0.0673	0.0632	0.0341	0.0556
	(0.170)	(0.0766)	(0.0266)	(0.0400)
R-squared	0.071	0.107	0.108	0.250
Linear flexible slope	0.130	0.129	0.0315*	0.0437
	(0.111)	(0.0873)	(0.0168)	(0.0276)
R-squared	0.071	0.104	0.109	0.251
Quadratic	0.137	0.123	0.0539	0.0404
	(0.202)	(0.107)	(0.0348)	(0.0445)
R-squared	0.071	0.105	0.099	0.252
Outcome mean	0.067	0.025	0.004	0.220

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Notes: Panel A restricts the sample to states with high security capacity (Andhra Pradesh, Jharkhand, Chhattisgarh and Orissa). There are 644 observations across 46 clusters in these regressions. Panel B restricts the sample to the other states. There are 2548 observations across 182 clusters. Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. Coefficients come from a 2SLS regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank).

Civilians (C) first choose how much information i to share with the government to maximize their expected utility

$$EU_C = b(i) - c_C(r(i)) + p(m, v, i)u(y_G + g) + (1 - p(m, v, i))u(y_N) \tag{B.1}$$

where $b(i)$ is the utility derived from the benefits of sharing information with the government, which may include both monetary and non-monetary components.[40] $c_C(r(i))$ measures the disutility from sharing information because Maoists may retaliate against civilians for sharing information based on a known retaliation function $r(i)$. y_G and y_N are the benefits civilians receive when their location is under government control or Naxalite control at the end of the period, respectively, and $u(.)$ is the utility function for these benefits. g is the extra benefit to citizens from governmental programs like an employment guarantee scheme.[41]

Overall, civilians therefore take into account both costs and benefits that arise directly from providing assistance to the government and the benefits provided by whoever is in power at the end of the period, which is also influenced by the level of information.

After civilians have made their decision, government troops and the insurgents simultaneously decide on their actions. The police (G) decides how much military action m to take against the Maoists to maximize the expected utility

$$EU_G = p(m, v, i) - c_G(m) \tag{B.2}$$

For simplicity, the government's expected utility from territorial control is assumed to equal the probability $p(.)$ that the government gains control, whereas the disutility from military action is given by $c_G(m)$.

At the same time, the Naxalites (N) determine how many attacks v to plan against the government, maximizing their expected utility

$$EU_N = [1 - p(m, v, i)] - c_N(v) \tag{B.3}$$

where $c_N(v)$ are the costs incurred from violence level v and $1 - p(.)$ is the probability that the Maoists will be in control of the location at the end of the period. Additionally, the Maoists retaliate against civilians for working as police informers, where retaliation $r(i)$ increases with the amount of information and assistance provided to the police.[42]

Together, the decisions by government actors and insurgents determine the level of violence in a given location and the endogenous probability

[40] The results are not sensitive to the order of moves as long as the rebels and the police move simultaneously, and the government expenditure is decided on before the civilians move. The order used in this model is related to the context – where civilians first decide on providing tip offs to the government, and the police then act on the information.

[41] While some of the literature like Kalyvas (2006) sees territorial control as a precondition for collaboration, our model is built on the idea that the expected benefits from future territorial control by the government may induce civilians to support the government in the fight against insurgents. This support will be low if the probability of government control is very low, consistent with the idea that it is difficult for the government to receive citizen support if its position in the conflict is weak.

[42] The retaliation is used to prevent further sharing of information, which is something that is not captured in this one-period model. It is also possible to model retaliation as a decision taken simultaneously with the civilian's information sharing in order to capture the value of the 'threat' of retaliation.

Table A4
Phase 2 – while Phase 1 is treated and Phase 2 treatment.

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Panel A: during Phase 1 treatment				
Linear	0.131**	0.0825*	0.00345	0.0348***
	(0.0537)	(0.0435)	(0.00434)	(0.0122)
R-squared	0.137	0.141	0.206	0.309
Linear flexible slope	0.208*	0.129*	0.0109	0.0434*
	(0.113)	(0.0739)	(0.00910)	(0.0236)
R-squared	0.090	0.103	0.191	0.296
Quadratic	0.183**	0.114*	0.00992	0.0425**
	(0.0925)	(0.0625)	(0.00717)	(0.0199)
R-squared	0.128	0.134	0.203	0.305
Panel B: Phase 2 treatment period				
Linear	−0.161	−0.180	−0.00620	0.00399
	(0.194)	(0.168)	(0.00906)	(0.0307)
R-squared	0.055	0.038	0.044	0.184
Linear flexible slope	−0.0331	−0.137	−0.00650	0.0327
	(0.232)	(0.173)	(0.00908)	(0.0462)
R-squared	0.055	0.040	0.043	0.178
Quadratic	−0.0702	−0.134	−0.00562	0.0237
	(0.200)	(0.157)	(0.00809)	(0.0377)
R-squared	0.058	0.040	0.044	0.185
Outcome mean	0.580	0.263	0.035	0.170

Panel A contains impacts on Phase 2 districts during February 2006 and March 2007 when Phase 1 received NREGS, and Phase 2 did not. 3178 observations: 227 district-clusters (Phase 2 and Phase 3 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. Panel B shows the impact on Phase 2 districts during April 2007 and March 2008 when Phase 2 also received NREGS. 2497 observations: 227 district-clusters (Phase 2 and Phase 3 districts) in 11 months post-Phase 2 implementation and pre-Phase 3 implementation. Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. "Affected Persons" indicates the number of persons killed, injured, abducted or captured. "Fatalities" indicates the total number of deaths. "Major Incidents" indicates number of 'Major Incidents' as coded by the SATP website. "Total Incidents" is the number of total Maoist-related incidents. Coefficients from a 2SLS regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank).

Table A5
Main results: without biggest incidents (outliers).

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Linear	0.373**	0.376**	0.0724**	0.173***
	(0.189)	(0.166)	(0.0286)	(0.0637)
R-squared	0.320	0.311	0.202	0.358
Linear flexible slope	0.323**	0.344**	0.0553***	0.145***
	(0.148)	(0.169)	(0.0211)	(0.0551)
R-squared	0.321	0.312	0.207	0.364
Quadratic	0.383**	0.460**	0.0795***	0.162**
	(0.193)	(0.205)	(0.0299)	(0.0657)
R-squared	0.320	0.309	0.199	0.360
Outcome mean	0.580	0.263	0.035	0.170

Dropping outlier incidents – large attacks by Maoists or police that affect (kill/injure) more than 20 persons at a time. These eliminate the deadliest 11 district-month observations. Robust to using other cutoffs. Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. Regressions contain 3184 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. "Affected Persons" indicates the number of persons killed, injured, abducted or captured. "Fatalities" indicates the total number of deaths. "Major Incidents" indicates the number of 'Major Incidents' as coded by the SATP website. "Total Incidents" is the number of total Maoist-related incidents. Coefficients from a 2SLS regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank).

that the government gains territorial control. At the end of the period, a location either becomes controlled by the government or remains contested, and payoffs are realized. In the next period, the process is repeated in all locations that remain contested, whereas there is no further violence in government-controlled areas. Cost functions $c_C(.)$, $c_G(.)$, and $c_N(.)$ are increasing and convex.

The model can be solved by backward induction to find the pure-strategy subgame perfect Nash equilibrium. Once civilians have decided on the amount of information i^* to share with the government, the government maximizes its expected utility, taking i^* and the violence level v chosen simultaneously by the Maoists as given. The first-order condition of (2) is therefore given by

$$\frac{\partial p(m, v, i^*)}{\partial m} - c'_G(m) \leq 0 \tag{B.4}$$

This equation pins down the best response function of military action m^* for every potential violence level v chosen by the insurgents. Since $c_G(.)$

Table A6
Who initiates the attacks and who is killed (per capita).

Specification	Panel A: who initiates the attacks		
	Police on maoist	**Maoist on police**	**Maoist on civilians**
Linear	0.975**	0.133	0.491**
	(0.412)	(0.101)	(0.227)
Linear flexible slope	0.741**	0.153	0.254
	(0.375)	(0.107)	(0.171)
Quadratic	0.779*	0.102	0.436**
	(0.424)	(0.0957)	(0.208)
Outcome mean	0.578	0.329	0.753

Specification	Panel B: who is killed		
	Civilians killed	**Police killed**	**Maoists killed**
Linear	0.531	−0.232	2.051***
	(0.716)	(0.632)	(0.755)
Linear flexible slope	0.358	−0.148	1.613**
	(0.561)	(0.372)	(0.763)
Quadratic	0.595	0.201	2.137**
	(0.706)	(0.637)	(0.897)
Outcome mean	1.099	0.958	0.956

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Results are per-10 million people (based on population counts from the 2001 Census). Panel A gives the results for 'who initiates the attacks and against whom.' Panel B shows the results for 'who is killed'. Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. Regressions contain 3192 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. Reported coefficients come from a two-staged least squares regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions

Table A7
Donut Hole and varying the bandwidth.

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Panel A: Donut Hole				
Linear	0.562*	0.474*	0.0870**	0.287**
	(0.311)	(0.283)	(0.0405)	(0.117)
R-squared	0.493	0.456	0.405	0.482
Linear flexible slope	0.429	0.424	0.0598*	0.203**
	(0.280)	(0.311)	(0.0316)	(0.0943)
R-squared	0.493	0.457	0.410	0.496
Quadratic	0.628*	0.615*	0.105**	0.281**
	(0.359)	(0.365)	(0.0454)	(0.127)
R-squared	0.492	0.454	0.402	0.483
Panel B: varying bandwidth				
$-x <$ normalized state rank $\leq x$				
x=10	0.759*	0.851*	0.133**	0.312*
	(0.447)	(0.454)	(0.0614)	(0.161)
R-squared	0.494	0.456	0.420	0.483
x=9	0.849*	0.886*	0.145**	0.315*
	(0.481)	(0.497)	(0.0659)	(0.174)
R-squared	0.497	0.463	0.441	0.486
x=8	0.882*	0.930*	0.152**	0.325*
	(0.523)	(0.539)	(0.0723)	(0.189)
R-squared	0.497	0.465	0.443	0.490
Outcome mean	0.580	0.263	0.035	0.170

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Panel A produces the Donut Hole results – this tackles measurement error by dropping districts closest to the cutoff. Panel B varies the bandwidth close to the cutoff, where the bandwidth size is "x." The results presented are for the linear specification where the slope is flexible on either side of the cutoff. Controls include baseline averages of each dependent variable and police force changes. Unit of observation is district-month. "Affected Persons" the indicates number of persons killed, injured, abducted or captured. "Fatalities" indicates the total number of deaths. "Major Incidents" indicates the number of 'Major Incidents' as coded by the SATP website. "Total Incidents" is the number of total Maoist-related incidents. Reported coefficients come from a two-staged least squares regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank).

Table A8
Intent-to-Treat (ITT) and without police controls.

Specification	Affected persons	Fatalities	Major incidents	Total incidents
Panel A: ITT				
Linear	0.287**	0.269**	0.0471***	0.124**
	(0.133)	(0.122)	(0.0176)	(0.0497)
R-squared	0.488	0.443	0.391	0.479
Linear flexible slope	0.317*	0.309*	0.0487**	0.132**
	(0.166)	(0.166)	(0.0211)	(0.0600)
R-squared	0.488	0.443	0.391	0.479
Quadratic	0.335**	0.348**	0.0578***	0.129**
	(0.162)	(0.161)	(0.0215)	(0.0593)
R-squared	0.488	0.444	0.391	0.479
Panel B: without police				
Linear	0.559*	0.499*	0.0945**	0.240**
	(0.298)	(0.280)	(0.0394)	(0.109)
R-squared	0.486	0.430	0.378	0.446
Linear flexible slope	0.541*	0.510	0.0806**	0.209**
	(0.310)	(0.315)	(0.0382)	(0.106)
R-squared	0.486	0.429	0.381	0.452
Quadratic	0.721**	0.738**	0.121**	0.261**
	(0.362)	(0.371)	(0.0486)	(0.133)
R-squared	0.485	0.425	0.371	0.442
Outcome mean	0.580	0.263	0.035	0.170

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Panel A shows the Intent-to-Treat impacts from a reduced form OLS regression. Panel B shows the 2SLS results (where treatment is instrumented with predicted treatment). In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank). Controls include baseline averages of each dependent variable. Unit of observation is district-month. Regressions contain 3192 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. "Affected Persons" indicates the number of persons killed, injured, abducted or captured. "Fatalities" indicates the total number of

Table A9
Other specifications: Count Data and Difference-in-Differences.

Specification	Affected persons	Fatalities	Major Incidents	Total Incidents
Panel A: Count Data				
Linear	2.207**	2.443***	2.552***	1.619**
	(1.014)	(0.881)	(0.618)	(0.760)
Linear flexible slope	2.125***	2.306***	2.108***	1.105*
	(0.733)	(0.634)	(0.483)	(0.596)
Quadratic	2.503**	2.466***	2.204***	1.368
	(1.066)	(0.723)	(0.551)	(0.931)
Panel B: Difference in Differences				
Intent-to-Treat	0.218*	0.105	0.0147	0.106**
	(0.127)	(0.0976)	(0.0102)	(0.0503)
R-squared	0.427	0.403	0.363	0.479
Actual treatment	0.303**	0.0774	0.0122	0.117***
	(0.118)	(0.0857)	(0.00923)	(0.0443)
R-squared	0.380	0.375	0.327	0.460
Outcome mean	0.580	0.263	0.035	0.170

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Panel A shows the results using a Count Data Model. The model used in this table is the Zero-Inflated Poisson model, where the zeros are predicted using pre-treatment averages of the dependent variable. The results are very similar using Hurdle Models (Logit-Poisson). Panel B shows the Difference-in-Differences results. The Intent-to-Treat results assigns treatment status to districts who should have received NREGS, whereas the 'Actual Treatment' row assigns treatment status to districts that actually received NREGS. Controls include baseline averages of each dependent variable. Unit of observation is district-month. Regressions contain 3192 observations: 228 district-clusters (Phase 1 and Phase 2 districts) in 14 months post-Phase 1 implementation and pre-Phase 2 implementation. "Affected Persons" indicates the number of persons killed, injured, abducted or captured. "Fatalities" indicates the total number of deaths. "Major Incidents" indicates the number of 'Major Incidents' as coded by the SATP website. "Total Incidents" is the number of total Maoist-related incidents. Reported coefficients come from a two-staged least squares regression, where actual NREGS treatment is instrumented with predicted treatment based on the assignment algorithm. In each subsequent row the regressions control for linear, linear with a flexible slope and quadratic functions of the running variable (normalized state rank).

is convex in m whereas $p(.)$ is concave in m, a unique maximum exists according to the Intermediate Value Theorem that satisfies the second-order conditions.

Similarly, the first-order condition for the Maoists is given by

$$\frac{-\partial p(m, v, i^*)}{\partial v} - c'_N(v) = 0 \tag{B.5}$$

which implicitly traces out the best-response function of v^* for every potential government violence level m. Assuming that $p(.)$ is decreasing and convex in v, this once again satisfies the second-order conditions.

In equilibrium, both actors make correct predictions about the level of violence chosen by the other player, leading to a Nash equilibrium in each subgame given the level of i^* where the best-response functions for the two players intersect. Assuming that $p_{mv} = p_{vm} < 0$, [43] it can be shown that $\frac{dm^*}{dv} < 0$ and $\frac{dv^*}{dm} > 0$ using the Implicit Function Theorem, which guarantees the existence of a unique Nash equilibrium.

We assume that government military action is more effective with access to more information $p_{mi} > 0$, while more information could make Maoist attacks against the police less effective $p_{vi} \leq 0$. This, in turn, implies that according to the Implicit Function Theorem $\frac{dm^*}{di} > 0$ and $\frac{dv^*}{di} \geq 0$, so more shared information by the civilians leads to higher levels of violence by both conflict parties.[44]

Civilians decide how much information to share with the government at the beginning of the period, knowing the best response and equilibrium violence-level functions, which leads to the first-order condition

$$b'(i) - c_C(r(i)) + \frac{dp}{di}[u(y_G + g) - u(y_N)] \leq 0 \tag{B.6}$$

where $\frac{dp}{di} = \frac{\partial p}{\partial m}\frac{dm}{di} + \frac{\partial p}{\partial v}\frac{dv}{di} + \frac{\partial p}{\partial i}$.[45] By the implicit function theorem, $\frac{di^*}{dg} = \frac{-\frac{dp}{di}u'(y_G + g)}{SOC} > 0$. This implies that civilians will assist the police with more information or assistance when they receive governmental programs like NREGS.[46]

As discussed above, a higher level of shared information increases m^* and v^*. Additionally, Naxalites also retaliate more against civilians since $r(i)$ is increasing in i. This means that overall violence in a given location rises after the introduction of the government program, and the impact will be greater for districts that do a better job of implementing the program.

The equilibrium decisions by civilians, insurgents and the government determine the probability p^* that the government will gain control in a given location, or district, at the end of the period. Since all locations are identical, in expectation the number of contested territories ℓ_t will decrease over time according to the relationship

$$\ell_t = (1 - p^*)\ell_{t-1} \tag{B.7}$$

After the conflict has lasted τ periods, the number of contested location is therefore: $\ell_\tau = (1 - p^*)^\tau \ell_0$. Given the simplifying assumption in this model that violence in a location stops once the government gains control, the number of territories decreases over time until the war ends in period T when $\ell_T = 0$.

The improved information flow increases the equilibrium probability that the government gains control in a location, which will speed up the end of the conflict. With a higher government success probability more locations will fall under government control in a period than before, leading to the fewer contested territories in the next period. While violence in a given location has gone up, this effect means that the aggregate violence, averaged across locations, will fall over time as the government wins the war more quickly than it otherwise would have.

Overall, the model therefore generates a number of testable predictions about the impact of a government program like NREGS on the incidence of conflict. First, the introduction of NREGS increases insurgency-related violence in the short-run. In the longer run, violence falls. Second, the program increases the government's effectiveness in tracking down insurgents, so there are more police-initiated attacks. This also implies that insurgents should be more likely to die or to be injured/captured than before. Furthermore, civilians may be more affected by violence if the Maoists retaliate against them for sharing information.

Appendix C. Supplementary data

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jdeveco.2016.09.006.

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[43] Intuitively, a given actor's effectiveness of violence on control over a location becomes lower the higher the violence of the other conflict party.

[44] For the police, the military action is complementary to the amount of information, and thus increases with more information. The rebels, however, fight harder to hold on to territory that is slipping away.

[45] Assistance and information increases the probability of police control if $\frac{\partial p}{\partial m}\frac{dm}{di} + \frac{\partial p}{\partial i} > -\frac{\partial p}{\partial v}\frac{dv}{di}$.

[46] While the police force in practice largely consists of local officers whereas NREGS is a national program, implementation quality largely depends on local institutions. Zimmermann (2015) finds, for example, that both the government parties as well as local incumbents regardless of party affiliation benefit from NREGS in areas where the program is implemented well in the 2009 general elections. This suggests that the people are aware that local institutions and personnel matter.

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